UNIVERSITY OF OKLAHOMA

GRADUATE COLLEGE

FAR FROM VALUE-FREE: HOW A VALUE-CENTERED SCIENTIFIC PLURALISM BOLSTERS THE COGNITIVE CREDENTIALS OF SCIENCE

A DISSERTATION

SUBMITTED TO THE GRADUATE FACULTY

in partial fulfillment of the requirements for the

Degree of

DOCTOR OF PHILOSOPHY

By

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Norman, Oklahoma
2015

FAR FROM VALUE-FREE: HOW A VALUE-CENTERED SCIENTIFIC
PLURALISM BOLSTERS THE COGNITIVE CREDENTIALS OF SCIENCE

A DISSERTATION APPROVED FOR THE
DEPARTMENT OF PHILOSOPHY

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Abstract

The *value-free ideal* for science (VFI) prohibits noncognitive values (e.g., social or religious values) from influencing the practice of science. After all, a scientist should not reject an empirical theory (e.g.) on religious grounds. But while motivated by reasonable concerns, VFI overlooks *legitimate* roles for noncognitive values in science.

Contra VFI, Hugh Lacey explains that noncognitive values can *promote* scientific aims by grounding new methodologies that may lead to novel theories and extend to new domains. Yet, Lacey agrees with one aspect of VFI: noncognitive values should not serve as grounds in the empirical evaluation of theories. This has led some critics to misidentify his view merely as an updated version of VFI.

I argue that views of the kind Lacey endorses, which I call *Dialectical Empiricism* (DE), deserve further investigation. They capture what VFI gets right; but they also show that noncognitive values are essential to good science. Throughout the dissertation I explore variants of DE, demonstrating its potential and flexibility. Furthermore, I defend DE against its critics, especially those who mistake it for a version of VFI. In stark opposition to VFI, DE reveals how the illusion of value-freedom sometimes blinds us to promising alternatives to mainstream scientific approaches. I demonstrate this by applying the framework of DE to contemporary research in agricultural and nutritional science. These case studies show that noncognitive values *really do* influence scientific practices. And, most importantly, they demonstrate how embracing a value-laden view of science can open our eyes to promising alternative approaches that may have the potential to increase our knowledge of the world and of possibilities for human flourishing.

Introduction

Science is not value free, nor should it be. Myriad values play essential roles throughout the practice of science. Nonetheless, the so-called *value-free ideal* (VFI) for science remains a bogeyman in the values-in-science literature. In fact, VFI has haunted the introductions of so many articles and books that it is impossible to ignore. It has been rejected enough times as perhaps to qualify as a straw-bogeyman, well and thoroughly beaten. Yet there is a reason for the persistence of the value-free ideal: it gets *something* right. And so I will not break with tradition here. Despite its name, VFI does not require that *all* values be excluded from science; cognitive (or epistemic) values concerning, e.g., theory-evidence relationships (such as accuracy) are permitted to enter into scientists' judgements. However, other values – noncognitive values, such as social or religious values – have no legitimate role to play in scientific inquiry. This is the sense in which VFI holds up value-freedom as an ideal: science should be free from noncognitive values.

The details still must be filled in, but it is not difficult to see what VFI gets right. Even among those who vehemently (and sometimes, derisively) reject VFI, many will admit that *some* aspects of scientific inquiry ought to exclude certain kinds of values. It would be troubling if, for example, a scientist rejected an empirical theory on the grounds that it conflicted with her religious values. Or, to give an historical (and practically obligatory) example, during the "Lysenko Affair" in early 20th century Russia, social and political values (backed by power) determined which theories were accepted as scientific knowledge, regardless their (lack of) evidential support. Most will agree, consonant with VFI, that values should not influence science in this manner.

Thus, what is needed is not a wholesale refutation of the value-free ideal, but a comprehensive account of the multifarious ways in which values should and should not be involved in scientific inquiry.

Hugh Lacey has taken the lead by developing just such an account, beginning with his book, *Is Science Value Free?* (1999). Just to be clear, Lacey's answer to the titular question of his book is a resounding 'No.' According to Lacey's view, values play a variety of indispensable roles, penetrating virtually every stage of scientific activity. In particular, he argues that social values (and other noncognitive values) help to shape a plurality of scientific approaches. He holds that these values interact not only with the aims of inquiry, but with the methodologies and practices at the very core of science. However, Lacey retains one aspect of VFI; namely, what he calls the "thesis of impartiality," according to which noncognitive values have no legitimate role to play in the context of theory appraisal. While he rejects other aspects of VFI, by upholding the thesis of impartiality Lacey's approach avoids the sorts of problematic cases (like the Lysenko Affair) which everyone agrees are not characteristic of good scientific practice.

Lacey does not provide a name for the kind of view he proposes. I will call views of this kind *Dialectical Empiricism* (DE) in light of the two essential features that make them distinctive: (1) they recognize and permit "dialectical interaction" among scientific approaches and noncognitive values; (2) they preclude noncognitive values from serving as a basis for the evaluation of theories. The latter point grounds such views as a form of empiricism because they maintain that empirical methods (and their attendant cognitive values) are central to the acquisition of scientific knowledge.

Currently Lacey's is the only articulation of a view of this kind, so his work will often be our focus. However, there are many possible forms or variants of DE which bear investigation, and so I probe the boundaries of DE throughout my work. This is important because the boundaries are often the locus of philosophical disputes, and it is worth asking whether where one happens to stand on an issue has implications for DE. (Happily, exploring the boundaries also satisfies one's intellectual curiosity!) In the end I hope both to strengthen the case for DE, defend it against its critics, and highlight some directions for the pursuit of some of its more promising variations.

My project begins by presenting Dialectical Empiricism in detail and defending it from its critics, some of whom misidentify it as a contemporary version of the value-free ideal. Although DE proposes a value-laden picture of scientific practice, it upholds the thesis of impartiality which is (indisputably) a remnant of VFI, and we will see that it bears some superficial resemblances as well. Consequently, an important task is to differentiate the two views. This must be kept in mind, since the defense of DE from objections to VFI (or objections to DE, when mistaken for a version of VFI) is a recurring theme in this dissertation. More specifically, my defense of DE often takes one of two forms: In some instances I show that an objection is inapplicable because it targets an aspect of VFI *not* present in DE. In other cases, when an objection genuinely gains traction with DE, I clarify the issue (especially when there *is* overlap with VFI) and forge a response.

Chapter 1 serves as an introduction to Dialectical Empiricism, chiefly centered on Lacey's account. The central tasks of this chapter are to begin spelling out the details of DE and to distance it from VFI. Along the way I also head off some anticipated, more or less surface-level, objections.

Among the more important elements in this chapter is my clarification of the position of DE with respect to the "thesis of neutrality," which stands in opposition to the approach to neutrality taken by VFI. One way of understanding the neutrality of science is that the products of research (viz., knowledge and technology) serve all interests equally well. While DE upholds this sense of neutrality as a worthy ideal, it recognizes that maximizing neutrality *requires the influx of a plurality of values* in science (in a particular context). In sharp contrast, according to VFI neutrality is achieved by *precluding, or at least minimizing, the influence of values*. This point is not always fully appreciated. For example, Heather Douglas unequivocally and unfairly portrays DE as a contemporary version of VFI despite this important difference between the two views (Douglas is the focus of Ch. 3).

In Chapter 2 I fortify the defense of DE against an important objection given by Helen Longino. Longino's objection specifically targets VFI, but an aspect of VFI that DE explicitly endorses as well; namely, she challenges the distinction between cognitive and noncognitive values. At the heart of Longino's argument are a set of *feminist* values, which she claims may function as cognitive values (in certain contexts) even though they clearly have a "political valence." Responding to Longino is crucial because the cognitive/noncognitive value distinction is central to the thesis of impartiality, which DE cannot do without. After all, if noncognitive values cannot be identified unambiguously, then it becomes unclear how they can safely be excluded from the evaluation of scientific theories.

One significant contribution I make to the dialectic is the articulation of a distinction between 'being a cognitive value' and 'having cognitive value.' In short, a feature ***is*** a cognitive value if it serves as one of the criteria for the empirical evaluation of theories. On the other hand, a feature of a theory *has* cognitive value if it leads to the theory's acceptance by increasing the degree to which the theory manifests cognitive values; in such cases the value of that feature *derives from* cognitive values. This helps in understanding Longino's feminist values, which *are not* cognitive values, even though they may *have* cognitive value.

Although I reject Longino's argument against the cognitive/noncognitive value distinction, I maintain that feminist values may play a legitimate and invaluable role in the production of scientific knowledge. In his account of DE, Lacey gives an interpretation of Longino's view that helps to establish this point. I strengthen the case for the legitimacy of interpreting Longino from within the framework of DE by applying it to one of her central case studies. Specifically, we will see that the interpretation via DE is a good fit for Longino's feminist critique of the "biological basis for behavioral sex differences."

Chapter 3 concerns two objections to VFI developed by Heather Douglas in her book, *Science, Policy, and the Value-Free Ideal* (2009). While these objections specifically target the value-free ideal, Douglas explicitly identifies DE as a contemporary version of VFI throughout her book, and so these objections call for a response. In this chapter I focus on two central objections: First, Douglas argues that VFI (and therefore DE) does not adequately account for the crucial role of noncognitive (in particular, social) values in light of 'inductive risk.' As a gloss, inductive risk concerns the risk associated with the possible consequences of acting on a theory, should that theory turn out to be false. Contra VFI, Douglas believes inductive risk requires that noncognitive values should influence scientists' assessments of the appropriate evidential standards for theory acceptance. Douglas's second argument pertains to the appropriate role for cognitive values in the context of theory appraisal. While VFI affirms a legitimate role for cognitive values as grounds for theory acceptance, Douglas maintains that *no* values (neither cognitive nor noncognitive) should serve in this role.

In response to the first objection I maintain that Douglas's critique, while basically correct, simply does not apply to DE. While her arguments are incisive against VFI, they only cut through aspects of VFI *which DE rejects*. I identify the problem with Douglas's position as a failure to recognize in DE (and in general) a distinction between the cognitive attitudes of 'acceptance' and 'endorsement.' In short, the distinction gets at the difference between judgments regarding *what should count as scientific knowledge* and those regarding *what should be endorsed as a basis for action*. Unlike the first objection, the second one *does* gain traction with DE. Douglas argues that cognitive values should not play a role in the evaluation of theories, but I maintain that her position presupposes an implausibly narrow conception of the aims of science. Moreover, my analysis reveals that a distinction central to Douglas's view – that between 'direct' and 'indirect' roles for values – stands in significant tension with her views about the function of cognitive values.

The notion of 'acceptance' of scientific theories is central to this chapter. Indeed, Lacey's account of DE proposes the thesis of impartiality (that noncognitive values should not ground the evaluation of theories) in terms of theory acceptance. However, while the notion of 'acceptance' is a staple in the contemporary values-in-science literature, there are philosophers (and presumably scientists) who would dispute that a theory is ever really 'accepted.' Thus, in this chapter I explore the possibility of articulating a version of DE that invokes a Bayesian account of theory appraisal as an alternative to one involving acceptance.

Chapter 4 is dedicated to investigating in greater detail one of the aforementioned 'boundaries' of DE in order to see how novel views of its kind might be developed. The theme of this chapter is the debate surrounding theoretical monism and pluralism. I focus especially on Sandra Mitchell's account of theoretical pluralism, which she calls "Integrative Pluralism." I argue that while DE is strictly compatible with either a monist or pluralist interpretation of scientific theories, it bears strong affinities with the latter view. According to DE, the need for methodological pluralism is partly driven by the complexity of values that (dynamically) shape the explanatory goals of inquiry. Similarly, although it takes a different route, Mitchell's view is that the complexity of phenomena – specifically, biological phenomena – helps to shape the explanatory goals of inquiry.

In order to bring out this similarity between DE and Integrative Pluralism I provide a detailed analysis of Mitchell's view. Of particular importance is her portrayal of Integrative Pluralism as an incompatible alternative to Paul Sherman's version of theoretical pluralism, the "Levels-of-Analysis" approach. (The reason why she takes their views to be incompatible need not be spelled out here.) I show that, to the contrary, their views are compatible because they both justify their preferred forms of theoretical pluralism by distinguishing among different explanatory goals (within the domain of biology).

While the affinities between DE and Integrative Pluralism do not logically require that DE must endorse theoretical pluralism, I maintain that they are an especially good fit. I therefore conclude that a version of DE which incorporates (something like) Integrative Pluralism bears further investigation and development.

In Chapters 5 and 6 I apply DE to actual scientific practices, focusing on contemporary developments in agriculture and nutrition. One aim of these chapters is to show that noncognitive values *really do* interact with scientific approaches. Thus, in Chapter 5, I compare the values involved in biotechnological and agroecological approaches to agriculture. This is the primary example Lacey discusses in his books, and it provides an excellent illustration of his view (and of DE, generally). I build on his work by explicating specific connections between noncognitive values and methodologies in competing approaches to agricultural science. In Chapter 6 I provide a novel application of DE to research in nutritional science, once again revealing specific interactions among noncognitive values and methodologies. In both cases the contrast of the different approaches and their associated values makes it clear that even those approaches which are ostensibly 'value-free' are actually value-laden.

Finally, I argue that these examples demonstrate how recognizing a role for noncognitive values can be beneficial for both cognitive and moral reasons. The danger of only pursuing (apparently) value-free approaches to science is that it may lead to the mistaken belief that all other approaches are value-laden and therefore illegitimate. When in this vein we marginalize or overlook viable alternatives, we miss out on possibilities for gaining scientific knowledge that cannot be grasped from within a purportedly value-free approach. When those overlooked possibilities also hold the promise to significantly improve people's lives, our blindness results in both a cognitive and a moral failure. On the other hand, if we embrace values in science and allow them to play their proper roles, then we will be in a better position to increase our knowledge of the world and of possibilities for human flourishing.

Part I: Theory

Chapter 1: Values and Impartiality in Science

Science is not value free, nor should it be. Myriad values play essential roles throughout the practice of science. Nonetheless, the so-called *value-free ideal* (VFI) for science remains a bogeyman in the values-in-science literature. In fact, VFI has haunted the introductions of so many articles and books that it is impossible to ignore. It has been rejected enough times as perhaps to qualify as a straw-bogeyman, well and thoroughly beaten. Yet there is a reason for the persistence of the value-free ideal: it gets *something* right. And so I will not break with tradition here. Despite its name, VFI does not require that *all* values be excluded from science; cognitive (or epistemic) values concerning, e.g., theory-evidence relationships (such as accuracy) are permitted to enter into scientists' judgements. However, other values – noncognitive values, such as social or religious values – have no legitimate role to play in scientific inquiry. There will, of course, be some disputes among various views with regard precisely which values are to count as 'cognitive' or 'noncognitive.' But this is the sense in which VFI holds up value-freedom as an ideal: science should be free from noncognitive values.

Although the details remain to be filled in, it is not difficult to see what VFI gets right. Even among those who vehemently (and sometimes, derisively) reject VFI, many will admit that *some* aspects of scientific inquiry ought to exclude certain kinds of values. It would be troubling if, for example, a scientist rejected an empirical theory on the grounds that it conflicted with her religious values. Or, to give an historical example, during the "Lysenko Affair" in early 20th century Russia, social and political values (backed by power) determined which theories were accepted as scientific knowledge, regardless their (lack of) evidential support. Most will agree, consonant with VFI, that values should not influence science in this manner. Thus, what is needed is not a wholesale refutation of the value-free ideal, but a comprehensive account of the multifarious ways in which values should and should not be involved in scientific inquiry.

Hugh Lacey has taken the lead by developing just such an account, beginning with his book, *Is Science Value Free?* (1999). Just to be clear, Lacey's answer to the titular question of his book is a resounding 'No.' According to Lacey's view, values play a variety of indispensable roles, penetrating virtually every stage of scientific inquiry. In particular, he argues that social values (and other noncognitive values) help to shape a plurality of scientific approaches. He holds that these values interact not only with the aims of inquiry, but with the methodologies and practices at the very core of science. However, Lacey retains one aspect of VFI; namely, what he calls the "thesis of impartiality," according to which noncognitive values have *no* legitimate role to play in the context of theory appraisal. While he rejects other aspects of VFI, by upholding the thesis of impartiality Lacey's approach avoids the sorts of problematic cases (like the Lysenko Affair) which everyone agrees are not characteristic of good scientific practice.

Lacey does not provide a name for the kind of view he proposes. I will call views of this kind, views that distinguish the kind of distinct roles for cognitive and noncognitive values proposed by Lacey, *Dialectical Empiricism* (DE). This name captures two essential features of such views. They are *dialectical* because they identify crucial dialectical interactions between values and scientific investigations.

Such views are a form of *empiricism* because they maintain that empirical methods (and their attendant cognitive values) are central to the acquisition of scientific knowledge.[1] DE, on any interpretation, endorses the thesis of impartiality, which encapsulates the view that what scientific theories (or applied models) we come to accept as true (or probably true) must be empirically grounded.

The purpose of the present chapter is to provide an overview of Dialectical Empiricism, with emphasis on clarifying its relation to the traditional value-free ideal for science. I proceed, in §I, to provide an account of VFI and explain the taxonomy of values in science deployed by DE. In §II, I present Lacey's account of the various stages, which he calls 'moments,' that are constitutive of scientific practice, and I delineate his account of the proper and improper roles for values at each 'moment' of scientific inquiry. I also explore some of the important ways that the 'moments' are intertwined, and argue that they are indeed apt conceptual tools. Finally, in §III, I revisit the central tenets of the traditional value-free ideal – autonomy, neutrality, and impartiality – in order to clarify what DE retains and rejects from the traditional view. Throughout I defend DE by responding to some common (or anticipated) objections to DE, but I forestall responses to the specific critiques given by Helen Longino and Heather Douglas for later chapters (Ch. 2 and 3).

I. The Value-Free Ideal

The following discussion requires a basic understanding of the traditional account of VFI. The name "value-free ideal" is misleading, since it does not preclude *all* values from playing legitimate roles in science. On the traditional account of VFI,

[1] I emphasize that "empiricism" should be understood broadly. I do not mean to evoke, for example, certain anti-realist interpretations of science to which Lacey is not committed.

values of any kind may be appropriate in the so-called ‘context of discovery,’ in which scientists choose an area of research and conceive of hypotheses. In this context values are considered harmless, or sometimes even good sources of inspiration. However, *only cognitive values*[2] are permitted in the ‘context of justification,’ in which theories or hypotheses[3] are tested and their empirical support assessed.

An early account of this sort of analysis is found in Carl Hempel’s *Aspects of Scientific Explanation* (1965) in which he considers the relationship between science and values. He draws the distinction between the contexts of discovery and justification and points out that values may properly enter into the former:

> […] when a scientist chooses some particular topic of investigation, these choices will presumably be determined to a large extent by his preferences, i.e, […] by the importance he attaches to the problems he proposes to investigate. In this explanatory, quasi-causal sense the scientific *activities* of human beings may certainly be said to presuppose valuations. [Hempel 1965, p. 90]

Hempel thinks it is obvious and uncontroversial that values may influence the conception of theories in the context of discovery. However, he is clear that values may not properly enter into the context of justification: “The grounds on which scientific hypotheses are accepted or rejected are provided by empirical evidence, which may include observational findings as well as previously established laws and theories, but surely no value judgments” (Hempel 1965, p. 91). He points out that value judgments bear no “logical relevance” to hypotheses in the way that empirical evidence does (*ibid.*).

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[2] For now it suffices to think of cognitive values as theoretical virtues – i.e., as good qualities of scientific theories (e.g., empirical adequacy).

[3] For simplicity’s sake I will use the term ‘theories’ to refer to scientific theories and hypotheses (and scientific claims in general, unless otherwise noted).

Later in the same essay Hempel explains that "the standards governing the inductive procedures of pure science," in other words, standards aligned with cognitive values such as empirical adequacy (i.e., a theory's quality of fit with the data), "reflect the objective of obtaining a certain goal, which might be described somewhat vaguely as the attainment of an increasingly reliable, extensive, and theoretically systematized body of information about the world" (Hempel 1965, p. 93). This echoes the view that cognitive values are conducive to the goals of science. He goes on to say that if the goals of science were different, for example, if we hoped to "form a system of beliefs or a world view that is emotionally reassuring or esthetically satisfying to us," then scientists may need to adopt an entirely different set of standards (*ibid*.). He concludes that "[t]he standards of procedure must in each case be formed in consideration of the goals to be attained; their justification must be relative to those goals and must, in this sense, presuppose them" (*ibid*.). Hempel's sentiment, which lives on in the value-free ideal, is that given the aims of modern science, only cognitive values are appropriate in the context of justification. If the goal of science were, instead, to develop a picture of the world that conforms to our preferences about the way the world should be, then some other set of values might be appropriate.

Most readers will find Hempel's position familiar, if not commonsensical. Lacey points out that the traditional conception of the value-free ideal "is readily recognizable, and its attraction is obvious" (Lacey 1999b, p. 60). Indeed, contemporary supporters of VFI spend most of their efforts defending it against criticism rather than explaining why it is legitimate in the first place. There are exceptions, however. Gregor Betz explains that VFI "derives, straightforwardly and independently, from democratic principles […]: As political decisions are informed by scientific findings, the value free ideal ensures—in a democratic society—that collective goals are determined by democratically legitimized institutions, and not by a handful of experts" (Betz 2013, p. 207). The role of science is therefore to provide objective theories (objective in the sense of being unbiased), or "just the facts" (as Joe Friday might say), so that they can be used as seen fit by society. In contrast, value-*laden* science threatens to undermine democratic ideals by providing theories that carry social value presuppositions or support particular ideologies (held by the 'experts'). The worry is that, consequently, value-laden science may illicitly help to entrench the views of a dominant social group or to delegitimize the views of a marginalized group. Certainly if the value-free ideal enables us to avoid such situations it is a worthy ideal.[4]

In order to clarify his position in the dialectic, Lacey identifies three senses of value-freedom associated with VFI, which he calls the theses of *autonomy*, *neutrality*, and *impartiality* (Lacey 1999, Ch. 1; Lacey 2005, Ch. 1.2). The thesis of *autonomy* concerns the isolation or independence of science from external influences (e.g., social or political influences). This conception of autonomy evokes the image of science as occurring in a bubble with scientists seeking knowledge for its own sake, answerable only to norms and interests internal to the scientific community. The thesis of *neutrality* states that scientific claims do not entail any particular value commitments and do not support or undermine the values held by individuals or groups (given that those values are minimally consistent with such claims). In addition, if science is neutral, then it is "evenhanded" with respect to any set of values one might hold,

[4] We will see that this claim is contested. For example, Sandra Harding thinks VFI leads precisely to those consequences that Betz thinks it avoids (e.g., Harding 1998).

meaning that scientific knowledge serves the interests of everyone equally (Lacey 1999, p. 239). Finally, according to the thesis of *impartiality*, only cognitive values may serve as grounds for theory acceptance. Other kinds of values – personal, social, religious, etc. – are not proper grounds for judgments in this context. While DE accepts impartiality, it rejects autonomy and neutrality. In §II I will present the structure of DE in detail; then, in §III, I explore reasons for rejecting the theses of autonomy and neutrality.

Cognitive and Non-Cognitive Values

DE is concerned with identifying proper roles for values in science. So, I begin with an account of the different kinds of values under discussion. For our purposes, we can think of a value as a quality or ideal, where the manifestation of that quality (or an approximation to the ideal) is deemed "partly constitutive of a 'good' ø" (Lacey 1999, p. 28; c.f. Lacey 2005, pp. 60-61). The nature of ø indicates the kind of value we take that quality to be. For example, the welfare of citizens is a quality whose manifestation is deemed partly constitutive of a good *society*; therefore, the welfare of citizens is a *social* value. Obviously much more can be said about values in general, but this characterization will turn out to be sufficient to help clarify the relevant distinctions among different kinds of values.

In the values-in-science literature, the terminology used to classify values is diverse, so I cannot account for the full range of views here. However, those who work in this area commonly distinguish between *cognitive* and *noncognitive* values. Cognitive values are qualities of good scientific theories, relative to the cognitive aims of science, e.g., that of acquiring reliable empirical knowledge about the world. The nature of cognitive values thus depends on precisely how one articulates these aims, and there are sure to be different viable ways of doing so. Nonetheless, while there is no definitive list of cognitive values, there is much overlap among those found in the literature. Some commonly cited cognitive values are simplicity, explanatory scope, and predictive power. Noncognitive values constitute all other kinds of values (e.g., ethical, social, religious, aesthetic, etc.), which are *not* related to the cognitive aims of science.

Any viable version of DE is obliged to give some account of what cognitive values are, and how they are distinct from other values that may play a role in the scientific enterprise. On Lacey's account, a value is a *cognitive* value if and only if it meets the following conditions: First, it must be "needed to explain (perhaps under idealization or rational reconstruction) theory choices that are actually made;" second, its status as "an indicator of sound scientific understanding [must] be well defended" (Lacey 1999, p. 91). In these two conditions Lacey hopes to capture the explanatory and normative aspects that must be present in cognitive values, given that they "play their roles in a context that not only makes genuine contact with scientific practice, but also recognizes that scientific practice is open and, in the long run, responsive to rational criticism" (*ibid*.).

As previously mentioned, there are many ways one might spell out the cognitive aims of science. I believe this is why Lacey leaves open the nature of a "sound scientific understanding" in the above second condition. A scientific realist might well maintain that only a true (or approximately true) theory can provide a sound understanding. For constructive empiricists, perhaps a sound understanding is found in an empirically adequate theory. In any case, cognitive values are taken to be constitutive of good theories, and these values can be manifested to greater or lesser degrees. They are the criteria according to which the acceptability of theories is to be assessed; only theories that manifest cognitive values to a high degree (where these values are spelled out appropriately for a given version of DE) should be accepted into the corpus of scientific knowledge. I address the details of Lacey's treatment of theory acceptance for DE in §II.

Lacey's incomplete[5] list of cognitive values includes "empirical adequacy, explanatory power, power to encapsulate possibilities, internal consistency, consonance, source of interpretive power, and rejection of ad hoc features" (Lacey 1999, p. 109). Many of these are among the usual suspects on such lists, but even those that sound unfamiliar can be understood in more or less familiar terms. It will be useful to survey some of the standard interpretations of the cognitive values Lacey identifies.

The first three values concern theory-evidence relationships. An *empirically adequate* theory is one that fits well with the data; it is often used interchangeably with *accuracy*. It is no accident that empirical adequacy is the first item listed; whenever cognitive values are ranked by their importance or priority, empirical adequacy is inevitably at the top. *Explanatory power* has to do with the range and diversity of phenomena encapsulated by the theory, though this can be interpreted in a variety of ways. Lacey specifies two possibilities: "Wide-ranging" explanatory power characterizes explanations that unify phenomena across domains under one or a few basic principles; whereas "full" explanatory power relates to the ability to account for

[5] Lacey mentions additional cognitive values somewhat sporadically in his work. It is clear that he does not attempt to give a definitive list.

all (or many) of "the aspects and the dimensions of phenomena, all their causes and effects, responsive to particularity, concreteness and uniqueness" (Lacey 1999, p. 59). It might be helpful to think of wide-ranging explanatory power as 'generality' or 'breadth of scope' and full explanatory power as 'comprehensiveness' or 'depth.' The *power to encapsulate possibilities* is Lacey's take on predictive power, including the ability "to identify novel, low probability and unrealized possibilities" (Lacey 1999, p. 60).

Consonance and *source of interpretive power* involve inter-theoretical relationships. It will suffice to think of consonance as 'external consistency,' or consistency with accepted theories, although a more nuanced interpretation is possible (e.g., involving the quality of fit with other theories rather than mere consistency). Interpretive power is the ability to account for the successes and failures of previous theories (where appropriate). The last items on the list, *internal consistency* and *rejection of ad hoc features*, speak for themselves. Lacey excludes some notable candidates from his list such as *simplicity* and *fruitfulness*, though he is not alone in doing so (c.f., Laudan 2004 and Douglas 2009). Still, the rejection of *ad hoc* features might be understood as a kind of simplicity (a notoriously slippery value!). For now we will set aside the more controversial cases; they will return to focus in Ch. 2 and 3.

It is important for DE that there be a meaningful distinction between cognitive and non-cognitive values. This distinction is at the heart of the thesis of *impartiality*, which is what DE retains from the traditional value-free ideal. However, the cognitive/noncognitive value distinction need not commit one to other aspects of VFI. Lacey is clear about this for his version of DE: "My defense of the distinction, unlike the traditional viewpoint, does not support keeping social values entirely out of the core of scientific activity" (Lacey 2005, p. 60). Lacey articulates the various roles for values in terms of what he calls "five moments of scientific activities" (Lacey *Forthcoming*, p. 2).

II. Five 'Moments' of Scientific Activities

DE distinguishes five distinct logical 'moments' that are constitutive of scientific inquiry.[6] I will treat each of these in turn. However, first I should assuage any concern that might be raised by the very idea of carving up the complex and variegated practice of science into neat categories. Although according to DE the five moments are distinctive and constitutive of the scientific enterprise, Lacey acknowledges that in practice they are often deeply entangled. This is why he frequently emphasizes that they do *not* have a strict temporal or causal order; still, they do different work, and so can be distinguished conceptually. Perhaps a better term would be "the five factors" or "five elements" of the scientific enterprise, avoiding the misleading temporal connotations. But I will stick with Lacey's terminology, which will cause less confusion when citing what he says about each of these factors. Their identification enables analysis of the roles values ought to play throughout scientific inquiry. Once they are laid out I will explore the intermingling of the different moments.

M_1: *Strategy Adoption.* The first moment is characterized by the adoption of a "methodological strategy" or "constraint/selection strategy" (henceforth, "strategy") in light of the values and objectives of inquiry (Lacey 1999, pp. 68-69; Lacey 2005, pp.

[6] In his earlier work Lacey only distinguishes between two (1999) and three (2005) 'moments' of science, but all along he has acknowledged that each moment could be further subdivided, depending on the purpose of one's analysis.

28-30; Lacey *Forthcoming*, pp. 2-3). A strategy sets *constraints* on theories, delimiting the kinds of problems that are to be solved and the kinds of answers to be sought, and it *selects* the types of data that are relevant to solving these problems. Strategies vary depending on the objectives of inquiry – i.e., the phenomena and possibilities of interest and the kind of understanding one seeks (e.g., nomological, teleological, or intentional). The strategy one chooses to adopt is influenced by the way one conceives of the objectives of science, but also the associated values one holds. The importance of the links between strategies and values cannot be overstated. Lacey maintains that, "generally, settling on a particular strategy is linked with its mutually reinforcing interactions with particular social values" (Lacey 1999, p. 109). I examine specific examples of these links in detail in Part II.

On Lacey's account of DE the primary aim of most work in modern science is to gain an understanding of the natural world in terms of "underlying structure, process, and law" (Lacey 1999, pp. 68-69; Lacey 2005, pp. 30-35; Lacey 2009, p. 843). The strategies associated with this conception of the aim of science are "materialist strategies" (*ibid*.). Materialist strategies constrain the theory-space to those which can be understood in terms of "underlying structure, process, and law" and select as relevant data those containing "materialist terms," which are "generally quantitative and mathematical, applicable in virtue of measurement, instrumental intervention and experimental operations" (Lacey 1999, p. 68).[7] For this reason Lacey often emphasizes the "decontextualizing" aspect of materialist strategies, since they are not suited to describe phenomena "considered as an integral part of daily life and social practice"

[7] There are substantive alternatives to materialist strategies that constrain theories in other ways. These will be explored further in Chapter 2 and in Part II.

(*ibid*.). Furthermore, he believes materialist strategies are connected with a particular set of social values that have become predominant, which I will refer to as the 'values of modernity'; they include the values of *control over nature*, *technological progress*, and *the market* (Lacey 1999, Ch. 6; I elaborate these values in Ch. 5). If this is right, then modern science – far from being value-free – is underpinned by social values that need not be essential to the aims of science.

Perhaps the most important aspect of how DE departs from the value-free ideal is its pluralism regarding strategies. Strategies are determined by the objectives of inquiry, and a diversity of values and objectives supports a diversity of strategies. One might be disinclined to admit that science is subject to a multiplicity of objectives; in particular, it may be difficult to imagine a science whose objective is *not* restricted to that of understanding the world in terms of underlying structure, process, and law. However, anticipating such concerns, Lacey provides an example of a contemporary alternative strategy, "agroecological strategies," which, "frame research on the relations and interactions between organisms and their environments [and] provide knowledge that can inform agroecological farming" (Lacey 2005, p. 113). While agroecological strategies, like materialist strategies, rate control over nature as an important value, a key difference is that for agroecological strategies the value of control over nature is subordinated to other social values, such as *ecological stability* and *local well-being* (Lacey 2005, pp. 110-111). Thus, while scientists utilizing agroecological strategies may reasonably make use of knowledge and technology gained under materialist strategies, these will not be sufficient for the kind of research and understanding they seek.

The contrast between materialist and agroecological strategies helps to illuminate the moment of strategy adoption because they share a stake in the same domain – research on seeds used for farming. Under materialist strategies, the seed is considered as an object to be modified and controlled, whose possibilities of interest (e.g., maximizing crop yield under ideal conditions) can be studied without investigation of the social or ecological context (Lacey 1999, Ch. 8; Lacey 2005, Part II; Lacey 1999a, pp. 30-31). This is exemplified by the fact that the relevant research can (in this case) be conducted in an experimental setting, so that scientists need not step foot outside the laboratory. In contrast, under agroecological strategies, seeds are considered as integral parts of local cultures and ecosystems, whose possibilities of interest (e.g., providing a sustainable crop that promotes local well-being) cannot be researched without investigation of the social and ecological contexts. The objectives of inquiry, viz., the phenomena and possibilities of interest, are settled at the moment of strategy adoption, serving to constrain the theory-space and select the relevant data. Crucially, the possibilities of interest are influenced by noncognitive values – the *values of modernity* in the case of materialist strategies; values such as *ecological stability*, *local well-being*, and *social justice* in the case of agroecological strategies. According to DE, the influence of noncognitive values is proper at M_1; in fact, if a wide range of values leads to a variety of strategies being adopted, then so much the better for science (Lacey 1999, p. 212).

Finally, it is worth emphasizing a necessary condition for strategic pluralism: a theory's fit with a particular strategy is *not* a cognitive value. So, for example, it is not a cognitive value of a theory that it conforms to the constraints developed under materialist strategies. Otherwise strategic pluralism would be precluded, since it may not be possible for a single theory to satisfy the constraints of different strategies. Since the general cognitive aims of science do not pick out a unique set of theoretical constraints, there must be a plurality of viable strategies (Lacey 1999, pp. 107-109).

$\underline{M_2}$: *Research.* The second moment of the scientific enterprise is characterized by research activities, understood in a robust sense, including:

> […] determining priorities; theoretical innovation, elaboration and critique; development of mathematical (and computer) models and methods; construction of instruments (for measurement or intervention) and experimental apparatus; experimental/observational activity; analysis of data; ethical conditions and restrictions. [Lacey 2009, p. 840]

Lacey does not focus much on M_2, perhaps because the influence of values within this moment has already received a fair share of attention. However, the scientific activities that characterize M_2 help to clarify different cognitive attitudes scientists may take. For instance, the cognitive attitude of "adopting" is relevant at M_2; to *adopt* a theory *T* is "to prefer T to its competitors for the sake of giving direction to on-going research in that area" (Lacey *Forthcoming*, p. 6). The adoption of a theory can be thought of as a commitment to pursue certain lines of research made intelligible by that theory (Lacey 1999, p. 14). An adopted theory is considered the primary target of research and testing – it is adopted for the purpose of thoroughly investigating its implications, with any eye to using those implications to test it. One may adopt a theory based on considerations of cognitive or noncognitive values.

A theory may be adopted in light of cognitive considerations, e.g., because it is more comprehensive than its competitors (even if they are already well-established). For example, scientists could have adopted the theory of relativity at M_2 because it extended to domains left unexplained by Newtonian theories. On the other hand, noncognitive values may also play a proper role in judgments of theory adoption, similar to the role they play in strategy adoption at M_1 (Lacey *Forthcoming*, p. 6-7). That is, if a research agenda framed by a theory promises to help further projects aligned with certain social values, then holding those values may be sufficient for the adoption of that theory in M_2 (but *not* for its acceptance in M_3; adoption and acceptance are distinct cognitive attitudes).

M_3: *Theory Appraisal.* The third moment is that of theory appraisal, in which the evidential support for a theory ultimately leads to its acceptance or rejection. While the other moments are shot through with noncognitive values, Lacey insists that in the moment of theory appraisal the roles for cognitive and noncognitive values must be kept distinct. Central to M_3 is the *thesis of impartiality*, according to which cognitive values are the only acceptable criteria for theory choice. A theory is accepted in accordance with impartiality only if it "manifests the cognitive values to a very high degree according to the highest recognized standards of evaluation" (Lacey 1999, p. 72). The crucial point is that acceptance appeals only to cognitive values; noncognitive values cannot serve as grounds for theory acceptance. The thesis of impartiality is what DE retains from the traditional value-free ideal; it maintains that at M_3, concerning judgments of theory acceptance, science ought to be free from noncognitive values.

There are several contentious issues that arise regarding DE's account of impartiality and the moment of theory appraisal. First, the distinction between cognitive and noncognitive values has been challenged. Without this distinction, an essential feature of DE's approach to values collapses. Second, the very idea of theory ‘acceptance’ is not uncontroversial, and some critics argue that, in particular, Lacey’s view relies on a conception of acceptance that is implausibly narrow. I address these issues in the following chapters: In Chapter 2 I explicate and expand on the cognitive/noncognitive distinction, focusing on Helen Longino’s critique of the distinction. In Chapter 3 I defend Lacey’s conception of theory acceptance from objections raised by Heather Douglas. In addition, I explore the possibility of formulating an account of DE that does *not* rely on the notion of theory ‘acceptance.’

M_4: *Dissemination of Results.* The fourth moment of science involves the dissemination of scientific knowledge and the results of research (Lacey *Forthcoming*, p. 2). Lacey mentions the importance of such activities in the context of science education (Lacey *Forthcoming*, p. 6), but he has yet to treat M_4 at length. I will not attempt to elaborate what little Lacey has to say about M_4.

M_5: *Theory Application.* The fifth moment of science encapsulates the application of scientific theories. Obviously ethical considerations can enter into decisions concerning theory application, e.g., when the intended consequences of application would be harmful, as in the case of military and weapons technology. However, values also enter into M_5 in the context of what is called “inductive risk.” Discussions of inductive risk trace back to Richard Rudner’s influential article, “The Scientist Qua Scientist Makes Value Judgments,” (1953) in which he argues that scientists must appeal to noncognitive values in judgments of theory acceptance. In particular, when determining acceptable levels of inductive risk (e.g., the risk associated with accepting a false theory or rejecting a true one), he says, scientists may legitimately appeal to *social* values. If policies or actions informed by a theory would have (say) *dire* consequences should the theory turn out to be false (where 'dire' is relative to social values), then scientists have a moral responsibility to subject the theory to higher standards of testing prior to its acceptance.

Although noncognitive values cannot serve as grounds for theory acceptance, they may legitimately influence the *standards* by which theories are evaluated. While M_3 is characterized by scientists' judgments of theory *acceptance*, M_5 is characterized by scientists' judgments of theory *endorsement*:

> To endorse [a theory] T is to judge that T has sufficient cognitive value […] that the possibility of future research leading to its rejection […], and the possible consequences […] of acting on it if this were to happen, should not be considered good reasons not to engage in actions informed by T. [Lacey 2005, p. 79]

To put it another way, if a scientist endorses a theory, she judges that either it has been soundly accepted (accepted in accordance with impartiality) and therefore been tested in accordance with the highest recognized standards, or it has not satisfied the standards required for acceptance but has been tested to standards high enough to warrant its application.[8]

Heather Douglas challenges Lacey's conception of theory acceptance on the basis of arguments concerning inductive risk (Douglas 2009). I defend DE from Douglas's objections in Chapter 3. Here, in the next section, I show that the five moments of scientific activities are interrelated in various subtle ways, which will be important in addressing Douglas's objections. It is worth reiterating that even though Lacey believes the five moments are conceptually distinct, he is well aware that they are

[8] I will return to the topic of the *standards* for theory appraisal in Chapter 3. Also, see (Lacey 1999, pp. 62-66).

a tangled mess in some ways (as the actual world is messy, generally). Nonetheless, these moments serve legitimate uses in his analyses.

Interactions among the Five Moments

Nearly every occasion on which Lacey introduces the different moments of scientific activities he is sure to point out that the moments are logical or analytical, and that they do *not* carry with them a strict causal or temporal order. For this reason, it is somewhat misleading for him to call them 'moments.' However, while the five moments are not temporal or causal, and although they are deeply intertwined, there is a logical order to be found among them. An examination of the order and interconnections of the five moments will help clarify DE and provide answers to some superficial problems that arise.

Although there is no strict causal or temporal ordering of the moments, there are regularities to be found. For example, it should not be surprising that *theory appraisal* (M_3), *dissemination of results* (M_4), and *theory application* (M_5) are generally preceded by *research* (M_2). Similarly, scientists must *adopt a strategy* (M_1) – that is, have an understanding of what kinds of theories they will pursue and what kinds of data will be relevant – prior to *research* (M_2). Perhaps choice of a methodological strategy is sometimes taken for granted (especially given the ubiquity of materialist strategies); however, some choices can also be made explicit, e.g., in research grant applications. The numerical indexing of the moments generally coincides with a common sense understanding of how science works. For this reason, one might worry that this view is too simple to capture the actual complexity of scientific inquiry. However, according to DE, the moments of science are deeply interconnected.

While the common sense ordering of the moments is readily grasped, the possibility of alternate causal or temporal orderings is less obvious. One complicating factor is that not every moment is necessary in the life of a given theory, depending on the aims of research. Consider a case in which scientists choose a methodology that prioritizes expedited results over accuracy, and so leads to results which are suitable to serve as a basis for action, even though they have not been subjected to the highest possible standards of evaluation. One would not necessarily want to accept 'expedient' theories into the body of scientific knowledge, but they can still serve as a basis for action, e.g., given a situation where acting *now* on a theory with a high degree of uncertainty is better than acting later with a low degree of uncertainty (or not acting at all). In such cases a theory goes from *research* (M_2) to *application* (M_5), effectively skipping the moment of *theory appraisal* (M_3), in which theories are accepted or rejected. The key to understanding this possibility is the distinction between the cognitive attitudes of 'acceptance' and 'endorsement,' which is the focus of Chapter 3.

Another possibility that defies the common sense understanding of the five moments is that circumstances may lead to a theory traversing a moment more than once. Suppose a theory is accepted at M_3 and has moved on to M_5 where it is undergoing risk assessment prior to its practical application. Further, suppose scientists identify a serious risk associated with the theory, which then motivates them to develop novel ways to test the theory to higher standards (seen as necessary for the theory's safe and responsible application). At this point the scientists return to M_2, pursuing new lines of research which, they hope, will lead to the theory's sound acceptance at M_3 and eventual application at M_5. In this example, there is a 'backwards' influence from *application* (M_5) to *research* (M_2), since the risk assessment led to new forms of testing that the theory should undergo prior to its acceptance and application.

As a final example, a possibility for 'backwards' influence within the moments of science rests in the relationship between *strategy adoption* (M_1) and *theory appraisal* (M_3). DE does not recognize any methodological strategy as nonviable *a priori*; the viability of a strategy is an empirical question to be settled over time by assessing a strategy's fruitfulness, understood as its ability reliably to produce soundly accepted theories (Lacey 1999, pp. 108, 175-178; Lacey 2005, p. 87). Thus, if over time few (or no) theories under a given strategy are soundly accepted at M_3, then scientists utilizing that strategy should go back to the drawing board at M_1 to adopt a new strategy that may prove more fruitful. In this example, the scientists traverse the moments: $M_1 \rightarrow M_2 \rightarrow M_3 \rightarrow M_1$. This demonstrates once more that the moments can be causally or temporally related in complex ways.

Now it is clear that the moments of science support a complex picture of scientific activities. It is therefore not the case that DE misrepresents science naïvely or simplistically. Moreover, the fact that the five moments enable a discussion of the complexity of the scientific enterprise demonstrates their value as analytical tools.

III. The Theses of Autonomy, Neutrality, and Impartiality

Lacey identifies three theses associated with the traditional value-free ideal: *autonomy*, *neutrality*, and *impartiality*. Although DE retains the thesis of impartiality, and so maintains that science should be (noncognitive) 'value-free' in the moment of theory appraisal, it is important to understand that it is not thereby committed to the view that noncognitive values have no legitimate roles within the core of science. In order to emphasize this point it will be helpful to examine the reasons for rejecting the theses of autonomy and neutrality.

According to the thesis of *autonomy*, science should not be influenced by external factors – e.g., government agencies and other institutions that fund research. On this conception of autonomy, scientists should seek knowledge for its own sake and only concern themselves with norms and interests internal to the scientific community. The idea of autonomy in science has deep roots; for example, consider Thomas Kuhn's *The Structure of Scientific Revolutions* (1962). Kuhn emphasizes the "insulation" of science from society, noting that there is no other field in which "individual creative work is so exclusively addressed to and evaluated by other members of the profession" (Kuhn 1962, p. 164). Moreover, Kuhn thought the autonomy of science enabled scientists to progress via "puzzle-solving" – to attend to problems framed by a particular paradigm without being influenced by social or political issues (*ibid*.). Heather Douglas, a critic of the value-free ideal, recognizes the importance of *autonomy* to VFI: "Once we reject the presumption of an isolated scientific community, the ideal of value-free science in its current form crumbles" (Douglas 2009, p. 46).

Clearly autonomy so conceived is not always manifested in science today, which is often influenced by political interests and the aims and values of the institutions that fund research. Furthermore, according to DE most of modern science is influenced by social values in virtue of operating under materialist strategies – namely, it is influenced by the *values of modernity*. Still, these examples demonstrate only that autonomy is not always achieved; they do not show that autonomy *as an ideal* should not be pursued. Perhaps the strongest reason against adopting autonomy as an ideal is that scientists ought to be responsive to social values regarding the possible consequences of theory application. This line of reasoning is pursued further in Chapter 3.

Yet, even setting aside concerns regarding theory application, autonomy is ultimately unrealizable. For research (of any kind) is a goal-directed pursuit, and so can only meaningfully be conducted under a strategy for attaining the desired goals. The research goals are always value-laden. Even the pure pursuit of truth is subject to issues regarding which truths are valued. (A simple description of the shapes and locations of all the grains of sand on a beach will provide a long list of truths, if truth is your only value!) So, the adoption of a strategy cannot be settled only by appeal to the cognitive aims of science. The cognitive aims of science underdetermine the values of the truth to be pursued, and so underdetermines the choice of a strategy. No phenomena can be ruled out *a priori* as viable candidates for systematic empirical inquiry. Furthermore, while there are potentially many fruitful strategies, they cannot all be pursued at once. Lacey explains that "[s]ince research requires material and social conditions, and the conditions needed for research under different strategies may be mutually incompatible, even to conduct research exploring one class of possibilities may preclude probing exploration of another class" (Lacey 2002, p. 8). Thus, considerations external to the cognitive aims of science inevitably enter into decisions about which strategies to adopt, rendering autonomy an unrealizable ideal.[9]

Finally, consider the thesis of *neutrality*, according to which scientific claims do not entail any particular value commitments and do not support or undermine the values people may hold (given that they are minimally consistent with those claims). If

[9] Kristina Rolin (2015) recognizes that DE diverges from VFI, since DE permits a role for noncognitive values in the moment of strategy adoption (p. 158). This is noteworthy because DE is sometimes misidentified as a contemporary version of VFI (e.g., Douglas 2009; Steel 2011).

science is neutral, then it is "evenhanded" with respect to any set of values, so that scientific knowledge serves the interests of everyone equally (Lacey 1999, p. 239). In other words, the results of research are no more significant for one set of values than any other. As Lacey puts it, "[n]eutrality expresses the value that science does not play moral favorites – that scientific research provides, as it were, a menu of soundly accepted theories, among the items of which (in principle) each value-outlook may have its tastes (good or bad) for application catered to" (Lacey 2005, p. 26).

According to DE, neutrality (in particular, 'evenhandedness') is a viable ideal, but it has not been realized in practice. This is because the results of research and the range of applications yielded under materialist strategies are of special significance for the values of modernity:

> We might expect that a theory would be especially amenable to application in projects framed by the social values with affinities to the constraint/selection strategies under which it was developed and consolidated. Then we might even speak of these social values also being manifested in theory. Impartiality does not preclude this; it just denies that it should have anything to do with the grounds supporting a judgment of [acceptance]. [Lacey 1999a, p. 92]

This point is well-illustrated in the contrast between materialist and agroecological strategies. When conducted under materialist strategies, research on seeds is conducive to applications such as the production of high-yield monocultures, which aligns with the values of modernity (i.e., control over nature, technological progress, and the global market). However, these applications do not further (and may hinder) aims informed by values associated with agroecological strategies, such as ecological stability, local well-being, and social justice (see Lacey 1999, Ch. 8).[10] Thus the products of research under materialist strategies are not *neutral*, even if they adhere to *impartiality*.

Sandra Harding confirms that neutrality is not currently within our grasp. She explains that scientific approaches which purport to conform to the value-free ideal (i.e., most of modern science) are not really neutral:

> [Modern science] may well 'work' in the sense of enabling prediction and control. However, this obvious fact does not provide evidence that the representation of nature's order in such a science is free of culturally local values and interests. A scientific account of one set of nature's regularities may at the same time obscure or draw attention away from other regularities and their causes, and from ones that would suggest other possibilities for human projects of organizing nature and social relations. [Harding 1998, p. 132]

Harding has doubts that modern science is *neutral* because the knowledge gained not only 'draws attention' away from other ways of understanding the world, but also does not serve all interests equally well. Kevin Elliott makes a similar point:

> […] our values do not exist in a vacuum; they are influenced by our scientific understanding of the world and of ourselves […]. Thus, by collecting some forms of knowledge and not others (and making some forms of knowledge salient to powerful decision makers rather than others), we can either consciously or unconsciously advance some political interests and value orientations rather than others. [Elliott 2013, pp. 341-342]

Still, DE recognizes the possibility of *increasing* the neutrality of science via the proliferation of a plurality of strategies, linked with a broad range of social values. Thus, not only does strategic pluralism bolster the cognitive credentials of science (e.g., by extending research to different domains of phenomena and leading to the investigation of novel possibilities), it is conducive to 'evenhandedness' and so helps to increase the neutrality of science. Notably, neutrality in this sense *relies* on a range of values influencing the practice of science. In contrast, on the traditional view of VFI,

[10] Specific links between values and approaches to agricultural science are explored in detail in Ch. 5.

neutrality results from the *value-freedom* of science. So even though DE supports an increase of neutrality in science, it recognizes that doing so requires the inclusion of a diversity of strategies and values, rather than an absence of values (as prescribed by VFI). Consequently, DE rejects the thesis of neutrality understood as a form of value-freedom in science.

In summary, of the three central theses comprising the traditional value-free ideal, DE only retains *impartiality*. It rejects *autonomy* chiefly because the adoption of strategies requires considerations external to the cognitive aims of science – in particular, consideration of social values. Moreover, concerns about pressure from the institutions that fund research and issues surrounding the social and moral implications of theory application render autonomy an unrealistic ideal. DE also rejects *neutrality* in its traditional form, so that an increase in neutrality actually requires a role for noncognitive values at the core of science. In light of DE's position regarding these issues we can see that it is *not* merely an updated version of the traditional value-free ideal. Quite to the contrary, DE maintains that cognitive *and* noncognitive values play many indispensable roles throughout scientific inquiry.

Conclusion

I hope it is now clear that Dialectical Empiricism is *not* a contemporary incarnation of the value-free ideal. Even though DE retains the thesis of *impartiality*, it rejects the theses of *autonomy* and *neutrality*. Furthermore, the dialectical interaction among strategies and social values at M_1 reveals that DE does not represent science as value-free, even at its core. More generally, the interplay of values throughout and between the different moments of scientific activities demonstrates a thorough rejection of the idea of science as value-free.

The next two chapters will serve to further distance DE from VFI while also responding to some serious objections. In Chapter 2 I defend DE from Helen Longino's critique of the cognitive/noncognitive value distinction. I also show that DE (once again departing from VFI) is able to legitimize the use of *feminist* values in science (as presented by Longino) without thereby compromising the integrity of science. In Chapter 3 I defend DE from objections raised by Heather Douglas concerning the role for noncognitive values in assessments of "inductive risk" in science. While VFI rejects this role for noncognitive values, DE explicitly endorses it – despite Douglas's claims to the contrary.

Chapter 2: Feminist Values in Science

According to Dialectical Empiricism (DE), while noncognitive values play significant roles throughout scientific inquiry, they have *no* legitimate role to play in judgments about theory acceptance (at M_3). The distinction between cognitive and noncognitive values is therefore essential to DE; after all, if noncognitive values cannot be identified unambiguously, then it is unclear how they can safely be excluded from the evaluation of scientific theories.

Helen Longino, a critic of the value-free ideal (VFI), challenges the cognitive/noncognitive value distinction. Situated within her broader framework for science, Contextual Empiricism (CE), she argues that *feminist* values may play a legitimate role at the core of scientific inquiry by functioning as cognitive values in the context of theory appraisal. To be clear, Longino does not claim that feminist values *are* cognitive values (which would presuppose the distinction), but that they can legitimately serve the same purpose as traditional cognitive values. Further, they can play this role even though feminist values are (partly) motivated by feminist *political* aims. Longino asserts that a comparison of feminist and traditional cognitive values reveals that the latter, too, sometimes have a noncognitive (e.g., social or political) dimension. If she is right that (putatively pure) cognitive values may be permeated by noncognitive values, then the cognitive/noncognitive value distinction is cast into doubt.

Although DE affirms the cognitive/noncognitive distinction, it does not deny that social values, including feminist values, play an important role in scientific inquiry. Thus, in what follows I draw on the resources of DE to defend the cognitive/noncognitive value distinction while retaining a legitimate role for feminist values in science. Both aims will be achieved by detailing Longino's account of feminist science recast in the framework of DE. First, in §I, I explicate Longino's conception of science, CE, as well as her account of feminist scientific values. Next, in §II, I present an interpretation of Longino from the perspective of DE, which includes an account of *feminist strategies* and the legitimate role for feminist values in science. Finally, in §III, I assess the propriety of this interpretation of Longino by applying it to a case that exemplifies CE (beyond the narrower concern for feminist values).

I. Contextual Empiricism and Feminist Values in Science

In *Science as Social Knowledge* (1990), Longino rejects the value-free ideal traditionally associated with the epistemology of the sciences, and presents her alternative, Contextual Empiricism, according to which science is an essentially social form of knowledge production. She calls her view 'Contextual' Empiricism, since she holds that "[e]vidential relevance of data is secured […] by background assumptions, with the consequence that the same data can in different contexts serve as evidence for different hypotheses" (Longino 1997, p. 39). Thus, her project begins with an emphasis on the problem of the underdetermination of theory by evidence. Longino explains that evidence only supports a theory in light of background assumptions that bridge the underdetermination gap, such that "[i]n the absence of any such beliefs no state of affairs will be taken as evidence of any other" (Longino 1990, p. 44). The objectivity of science is threatened, says Longino, when problematic background assumptions – e.g., those expressing "subjective preferences" – go unnoticed and are perpetuated in scientific theories without undergoing critical scrutiny (Longino 1997, p. 40).

In order to maximize scientific objectivity, Longino argues that science as a social practice should manifest certain specific features to the greatest degree possible. These include (1) the provision of "avenues for the expression and dissemination of criticism;" (2) "uptake of, or response to, criticism;" (3) critical evaluation of scientific claims in light of "public standards" developed within the scientific community; and (4) "equality of intellectual authority" (Longino 1995, p. 384). The manifestation of these features will lead to exposure and scrutiny of background assumptions and minimize the influence of subjective preferences. A theory must filter through the social processes of science exhibiting these features, undergoing what Longino calls "transformative criticism," in order to be objective scientific knowledge (Longino 2004, p. 134). Thus, scientific knowledge is *social* knowledge.[11]

Of particular importance are the "public standards," which include the values that guide theory choice. According to CE, politically motivated feminist values are among the public standards to which scientists may legitimately appeal in support of their judgments. The explication of particular feminist values is situated in Longino's argument against a sharp distinction between cognitive and noncognitive values, which draws on a context-free conception of cognitive values. She explains that a context-free cognitive value is "a quality of theories, models, or hypotheses that can serve independently of context as a universally applicable criterion of epistemic worth" (Longino 1997, p. 42). Her position is that even the traditional cognitive values are not context-free in this sense. Instead, cognitive values should be understood to reside

[11] Longino makes this point explicit: "[…] the application of scientific method, that is, of any subset of the collection of means of supporting scientific theory on the basis of evidential data, requires by its very nature the participation of two or more individuals" (Longino 1990, p. 67).

among the *context-sensitive* public standards used in theory appraisal (Longino 1997, pp. 40-41).

Longino's Challenge to the Cognitive/Noncognitive Distinction

Longino's argument against the cognitive/noncognitive value distinction proceeds by laying out a set of feminist values that *function* as cognitive values even though they have a "social/political valence" (Longino 1997, p. 55). Along the way she contrasts feminist and traditional cognitive values in order to show that the latter are no more free from the influence of noncognitive values than the former, thus raising doubts about the (supposed) purely cognitive nature of traditional cognitive values. Thus, Longino flanks the cognitive/noncognitive value distinction on both sides. On one side she demonstrates that social (feminist) values can serve as cognitive values; on the other, she argues that there can be social aspects to (traditional) cognitive values.

The list of proposed feminist values includes empirical adequacy, novelty, ontological heterogeneity, complexity of relationship, applicability to current human needs, and diffusion of power (Longino 1995, pp. 386-389).[12] What makes them specifically *feminist* values is their explanatory role in actual theoretical choices made by feminist scientists and their ability to serve a plurality of feminist aims (Longino 1997, pp. 44-45). For feminist values to function as *cognitive* values they must also be characteristics of good theories, relative to the aim of acquiring reliable empirical knowledge about the world. Let us now examine the feminist values, attending especially to how they may characterize good theories. At the same time, for each

[12] I will not deal with the values *applicability to current human needs* and *diffusion of power*, since they are "pragmatic" values and, on Longino's own account, do not play an important role in her argument against the cognitive/noncognitive value distinction (Longino 1997, p. 48).

feminist value we will see how their traditional cognitive value counterparts may fail to exhibit a ‘purely’ cognitive nature.

An *empirically adequate* theory is one that fits well with the data. Longino considers empirical adequacy to be a feminist value (in addition to being a traditional cognitive value) because it does important work for feminist inquiry by “revealing gender,” as she puts it, “either in the form of bias about the phenomena or as a phenomenon in the domain itself, or to reveal the activities of women or females in the domain” (Longino 1995, p. 391). For instance, feminist inquiry appeals to empirical adequacy to reveal bias in research on sex differences. A theory that purports to identify biological sex differences that are not supported by the data (e.g., that females are naturally inferior at math) fails to manifest the value of empirical adequacy. Thus, empirical adequacy does important work for feminist inquiry.

Although Longino confesses she is tempted to treat empirical adequacy as a ‘pure’ cognitive value, she points out that it, too, is context-sensitive in at least one sense. Empirical adequacy concerns the quality of fit between theory and data, but a theory is not expected to fit *all* the data or just *any* old data, but data of specified kinds. Thus, where noncognitive values influence the selection of data, they may permeate even the value of empirical adequacy (Longino 1995, pp. 394-395).

The value of *novelty* directs us to prefer theories that diverge significantly from accepted theories or address previously unexplored domains of phenomena. The traditional counterpart to novelty is consistency with accepted theories (external consistency, or consonance). Novelty can help reveal gender by rejecting, in part or whole, traditional frameworks that entrench sexist conceptions of human nature

(Longino 1995, p. 392). In contrast, if one adheres to external consistency, such conceptions (when present) are perpetuated, purposefully or not. Thus, Longino explains, "[t]he socio-political basis for the criterion for novelty is the need for theoretical frameworks other than those that have functioned in gender oppression by making gender invisible" (Longino 1997, p. 51).

Ontological heterogeneity stands in contrast to (ontological) simplicity. While simplicity recommends theories with the fewest basic entities, ontological heterogeneity recommends those with a diversity of basic entities. Longino points out that ontologically simple theories may explain away individual differences between entities by considering them deviations from the paradigmatic basic entity. If so, they may preserve simplicity on pain of overlooking real and significant differences. On the other hand, ontological heterogeneity recommends "[t]reating individual differences as important and not to be elided in abstractions or idealizations" (Longino 1995, p. 387). For example, a medical science that treats the human male body as the paradigmatic human body would wrongly overlook real and significant differences between male and female subjects, possibly leading to medical treatments ostensibly suitable for humans, but tailored only to human males. This kind of case supports "theories of inferiority," according to which "[d]ifference must be ordered, one type chosen as standard, and all others seen as failed or incomplete versions. […] Difference is then treated as a departure from, a failure to fully meet, the standard, rather than simply difference" (Longino 1995, p. 388). In contrast, ontological heterogeneity supports ontologies with different, but equally significant, basic entities.

The last feminist value is *complexity of relationship*, which contrasts with the traditional values of simplicity and scope (Longino 1995, p. 394). Complexity of relationship recommends theories which treat relations between entities as complex, or which posit interaction instead of unidirectional causation.[13] Complexity runs afoul of simplicity (for obvious reasons), but it is also in tension with scope in that less complicated models may accommodate a greater range of phenomena. One example of the way complexity of relationship can reveal gender is found in accounts of human reproduction. In one such account, the female gamete (egg) was characterized as passive, while the male gamete (sperm) was active. More recent accounts have shown that the process of fertilization is actually a complex interaction with both gametes playing vital and active roles (Longino 1997, pp. 47-48).

Longino takes these examples to demonstrate both that feminist values can characterize good theories (and aid in theory choice) and that the traditional cognitive values can serve to smuggle in or reinforce social values. Although they help to serve certain cognitive aims, both sets of values exhibit a "political valence." Longino concludes that "the traditional virtues cannot be taken as constitutive of 'best explanation' or of 'science' in some social-value neutral sense" (Longino 1997, p. 55). Thus, the cognitive/noncognitive value distinction appears to be in serious jeopardy.

Masculinist Values in Science

In an effort to bolster Longino's position (before responding to it), I now turn to an argument forwarded by Sandra Harding that aligns well with Longino's strategy. Consonant with Longino's concerns, Harding points out that some concepts commonly

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[13] Longino sometimes refers to this value as "mutuality of interaction" (e.g., Longino 1997, p. 47).

invoked in scientific contexts – concepts such as 'objective' and 'rational' – carry a masculinist bias:

> […] objectivity has been thought to require neutrality; neutrality is coded masculine; and masculinity as individual identity and as symbolic meaning is culturally formed in opposition to the 'feminine' and is continuously so maintained. Masculine is defined primarily by the absence of the traits attributed to the feminine. […] So how could women ever be perceived to be objective? This problem infects related concepts, also thought to require value-neutrality, such as 'rational' and 'scientific.' [Harding 1998, pp. 137-138]

In the exposition of the value-free ideal in Ch. 1 (§I) I quoted a passage from Carl Hempel that illustrates Harding's point. Hempel distinguishes 'scientific' standards for theory acceptance from those standards which would be appropriate given 'non-scientific' aims, such as the formation of "a world view that is emotionally reassuring or esthetically satisfying" (Hempel 1965, p. 93). It is clear enough that there are important differences among the aims Hempel discusses. What is significant is the particular selection of aims he chooses to use in his example. In this passage, terms like 'emotional' and 'esthetical' are contrasted with 'scientific rationality,' 'objectivity,' and 'reasonableness.' Indeed, Harding does not have a difficult time making the case that such terms are (and have been) commonly associated with femininity and masculinity (Harding 1998, pp. 137-140).

Although Harding does not target specific cognitive values, if she is right that concepts of scientific rationality sometimes contain masculinist bias, then cognitive values fall within the scope of her argument. After all, cognitive values are understood as the criteria for the 'rational acceptability' of theories. If traditional cognitive values are, in fact, aimed at realizing a *masculinist* conception of acceptable theories, then they are permeated by social values.

Not only does Harding's point serve as additional fodder against the cognitive/noncognitive value distinction, it lends credence to the need for (and legitimacy of) feminist values. This is because feminist values can serve as a counter-balance to masculine bias if and when it is present in traditional cognitive values. In Harding's idiom, the use of feminist values in the context of theory appraisal would be a step toward realizing "strong objectivity," which is her preferred alternative to the illusion of objectivity (or "weak objectivity") secured via the traditional cognitive values as deployed in the framework of VFI (Harding 1998, pp. 140-143).

In the next section I draw on the resources of DE to respond to the challenge issued by Longino, and fortified by Harding. My aim is to explicate an interpretation of Longino's feminist values that justifies the cognitive/noncognitive distinction *and* establishes a legitimate (indeed, crucial) role for feminist values in science within the framework of DE.

II. Dialectical Empiricism: An Interpretation of Longino's View

From the perspective of DE, Longino's feminist values may indeed characterize good theories. However, they do so without functioning as cognitive values. Instead, they play their role at the moment of strategy adoption (M_1), serving as constraints on the kinds of theories to be formulated and investigated via *feminist strategies*. This is crucially distinct from the role that (traditional) cognitive values play at the moment of theory appraisal (M_3). Using this framework, I argue that the 'cognitive nature' Longino attributes to her feminist values is best explained by their function at M_1, and that this function actually *depends* on the cognitive/noncognitive value distinction. I begin by pointing out an ambiguity in how ‘cognitive value’ is ordinarily conceived, which will help to bring the rest of the discussion into focus.

On Lacey’s account of DE, cognitive values are the criteria by which theories are assessed and either accepted or rejected. Competing theories are evaluated in terms of the degrees to which they manifest these values. On this view, cognitive values are *constitutive* of good theories. Still, other kinds of values may *lead* to good theories even though they are not constitutive of them. Let us say that such values ‘*have*’ cognitive value when they increase the degree to which a theory manifests (traditional) cognitive values.[14]

Kristina Rolin provides an apt example of something which *has* cognitive value, but which *is not* a cognitive value. In her article “Values in Science: The Case of Scientific Collaboration,” Rolin examines the ways that collaboration in science helps lead to the development of good theories (Rolin 2015). Collaboration *is not* a cognitive value; this makes sense because the degree to which collaboration is involved in the development of a theory has no bearing on whether that theory should ultimately become accepted, except in a derivative sense. The *reason* collaboration helps lead to good theories is because it helps to increase the manifestation of (traditional) cognitive values. For instance, in virtue of being scrutinized and tested by more than one scientist in the course of its development, a theory might be expected to be shown to be more accurate than it otherwise would have been. Thus, collaboration may lead to the increased manifestation of empirical adequacy.

[14] Daniel Steel makes a similar distinction between what he calls “intrinsic” and “extrinsic” epistemic values (Steel 2011).

Along similar lines, in Longino's examples we can see that (with the obvious exception of empirical adequacy, and other values that DE recognizes as cognitive values) feminist values *have* cognitive value, but that they *are not* cognitive values. Although feminist values may be manifested in good theories, only the traditional cognitive values ground the acceptance or rejection of a theory. In other words, in Longino's cases, the acceptance of a theory in favor of its rival is *not* justified in virtue of its being characterized by the feminist values she recommends. It is justified, instead, because it manifests various specific traditional cognitive values. Consider the feminist value, complexity of relationship. When choosing between theories about human reproduction, if the female gamete *does* have an active role, then we should indeed prefer the theory characterized by the feminist value, complexity of relationship. However, the *reason* we prefer it is that it manifests empirical adequacy to a higher degree than its rival (Lacey 1999, p. 214). Thus, the value complexity of relationship *has* cognitive value in this context because it ends up promoting empirical adequacy. Similarly, the rest of the feminist values can be understood as *having* cognitive value without *being* cognitive values.

This account explains the 'cognitive nature' of feminist values, thus addressing one prong of Longino's attack. Still, she implicates the traditional cognitive values in being permeated by certain social/political values, especially sexist values. Appealing once more to the framework of DE, I argue that in these cases the *improper* role of social values is the key factor.

According to DE, when theory choice is underdetermined by both the evidence *and* (traditional) cognitive values, then if a theory is nonetheless (non-arbitrarily) accepted over its rivals:

> […] a [noncognitive] value has played a role (improperly) *alongside* the cognitive values in making the judgment of acceptance. This does not preclude [noncognitive] values playing a proper role […] at the moment when the strategies function, interacting dialectically with them. [Lacey 1999, pp. 231-232]

During the selection of a strategy (M_1) social values interact *dialectically* with cognitive values. However, if social values serve as independent grounds for judgments concerning theory acceptance (M_3), then they are operating *alongside* the cognitive values. Only the former role, during the selection of strategy, constitutes a proper role for social values. So, for example, if sexist social values served as grounds (explicitly or not) in the judgment to accept the theory attributing a passive role to the female gamete, then that theory was not soundly accepted. That is, in virtue of sexist values operating *alongside* the cognitive values, that theory was accepted in violation of the thesis of impartiality. The fact that this went unnoticed – that a traditional cognitive value 'smuggled in' a noncognitive value – does not undermine the cognitive/noncognitive value distinction. In fact, diagnosing and remediating the problem in such cases *depends* on the recognition of the distinction.

From the perspective of DE, feminist values help to uncover bias or 'reveal gender' not by providing a new set of cognitive values, but by *forming the framework for an alternative strategy*. This generates a greater range of theories which can serve as rivals to theories developed under other strategies. Rivals developed under a diversity of constraints can help to reveal cases (e.g., involving sexist bias) in which noncognitive values functioned improperly *alongside* cognitive values (at M_3):

> Pointing to the role of bias in the support of a theory indicates that the theory is not acceptable in accord with *impartiality*, and it indicates avenues to follow for its more stringent testing – test it against competitors that are free from *that* bias. All of this makes sense because we expect our research projects to leave a residue in the stock of knowledge. If we did not expect this, we would just have the back and forth play of biases, with only power to settle the matter. [Lacey 1999, pp. 214-215]

The role alternative strategies can play in helping to identify the improper influence of noncognitive values also yields a response to Harding's claims about the masculinist bias sometimes present in conceptions of scientific rationality. First, it should be emphasized that not even Harding believes such conceptions are *inherently* biased, but contingently so, as a result of their cultural and historical development (Harding 1998, pp. 138-139). Similarly, Longino points out that:

> One cannot claim by looking at the theoretical virtues themselves that they are liberatory, oppressive, feminist, masculinist, or neutral. One must look instead at the grounds that are offered for treating them as virtues and the ways in which their deployment in particular scientific arguments and research programs resonates with conditions in the social and political context of the research. [Longino 1995, p. 396]

When one suspects or identifies bias in a particular context, then the deployment of alternative strategies and the development of rival theories can help to suss out and eliminate it. The cognitive/noncognitive value distinction is therefore vindicated because it makes the thesis of impartiality intelligible and therefore provides the normative force behind feminist (and other) critiques that point out bias in the context of theory appraisal. And, equally important, the distinction enables the analysis of the proper roles for values at the different moments of science, which in turn supports a legitimate role for feminist values as the constitutive values of *feminist strategies* at the moment of strategy adoption.

Feminist Strategies

In the idiom of DE, Longino is advocating *feminist strategies* as an alternative to mainstream (i.e., materialist) strategies. On Lacey's account, the objective of feminist science is "to identify (reliably) the possibilities open to human agency, the conditions for its enhancement and diminishment, and to discover means for bringing about more enhancing conditions" (Lacey 1999, p. 204). This objective, together with the associated feminist values, serves to *constrain* the class of theories to be considered, and *select* the kind of data that is relevant to confirm those theories. Feminist strategies may constrain the theories under consideration to those consistent with "the intentionality of human agency" (*ibid*.), and select as the relevant data observations of actions that "need not abstract from the intentionality involved" (Lacey 1999, p. 208).

On this proposal, although feminist strategies and feminist values are viable, there are no uniquely feminist *cognitive* values. While *empirical adequacy* is a cognitive value, the other feminist values on Longino's list are best understood as "constitutive" values that have a proper role only during the moment of strategy adoption (Lacey 1999, pp. 218-223). Crucially, they serve in this role as theoretical constraints, *not* as criteria for theory acceptance. Thus, Lacey explains that from the perspective of DE, when Longino criticizes ontologically simple theories:

> […] she is not denying the 'factual' claims of such theories on the grounds of their discord with her adopted values, but pointing out that such theories have not been tested against theories developed under constraints dialectically linked with this value, so that, therefore, they have not been appraised under appropriately high standards. [Lacey 1999, p. 220]

Similarly, DE identifies the other feminist values as constitutive values that motivate further research with higher standards – i.e., by requiring a higher degree of manifestation of cognitive values for sound acceptance and further testing against theories constrained by feminist strategies.

This interpretation demonstrates the need for the cognitive/noncognitive distinction while illustrating how feminist values can play a vital role in science. According to DE the cognitive aims of science require the strict separation of roles for cognitive and noncognitive values at the moment of theory appraisal (M_3). Yet, this does *not* preclude a legitimate role for feminist values at the moment of strategy adoption (M_1). From Lacey's perspective, not only are feminist strategies viable, they are "essential – *for cognitive reasons* – for appraising theories of human cognitive abilities and their exercise and legitimating their practical application" (Lacey 1999, p. 212).

Rival Theories and Competing Strategies

DE contends that the development and pursuit of alternative strategies can serve to bolster the cognitive credentials of science by raising the standards for theory acceptance. This is accomplished by producing rival theories under constraints linked with different social values. These rival theories (developed under different strategies) are special in that they may be, in a sense, "unthinkable" under the original theory's strategy due to the constraints in play (Lacey 2005, pp. 90-91). Thus, if a single strategy dominates scientific practice it will have cognitive blind-spots. The proliferation of competing strategies helps to fill those gaps. The desire to overcome the cognitive limitations of individual strategies drives the need for alternatives such as feminist strategies.

In order for theories to be *rivals* there must be a meaningful way to compare them, which raises the question how qualities of theories developed under different strategies can be weighed against one another. The answer is complicated by the fact that incommensurability can arise between strategies deriving from differences in the phenomena and possibilities of interest, as well as the terminology deployed in theoretical language and data reports (Lacey 2005, p. 85). Notably the incommensurability that can result among strategies is reminiscent of Thomas Kuhn's ideas about methodological and semantic incommensurability between paradigms; however, we will see that DE's views on incommensurability are not as extreme as Kuhn's. Comparing theories within the *same* strategy is not problematic, since they work with the same general aims, constraints, and data. However, methodological and semantic incommensurability can sometimes serve as barriers to the comparison of theories developed under different strategies.

Semantic incommensurability may arise between competing strategies when the lexicon of one strategy contains terms whose meaning cannot be adequately expressed via the lexicon of another strategy. For example, feminist strategies may deploy intentional language that resists translation into the lexicon of materialist strategies which tend to characterize phenomena (e.g.) in the language of laws of nature (Lacey 1999, p. 205). On the other hand, *methodological* incommensurability can arise when alternative strategies result in divergent interpretations of cognitive values such as empirical adequacy. An empirically adequate theory is one which fits well with the data, but prior to the adoption of a strategy it remains unspecified what kind of data a theory must fit.

This is not to say that there is *no* traction between feminist and materialist strategies; the kind and extent of incommensurability depends on the particular strategies and the context of their deployment. Even when theories developed under competing strategies have different interpretations of empirical adequacy it is possible to compare them, given that their constraints and data overlap to a sufficient degree (Lacey 2005, pp. 82-86). Such overlap is what enables, for example, rival theories about gametic fusion to be developed under feminist and materialist strategies. Although they are formulated with different constraints (e.g., the feminist theory is constrained by the value of complexity of interaction), both theories are meant to fit the same data.

In light of this discussion of empirical adequacy, it is important to see that DE actually *resonates* with Longino's claim that there are no "context-free" cognitive values. Since empirical adequacy concerns the relationship between theory and data, its interpretation *is* influenced by the noncognitive values that shape strategies. However, this does not threaten the cognitive/noncognitive value distinction deployed by DE, which does not preclude interaction among, or even co-instantiation of, cognitive and noncognitive values. This point recalls how the distinct 'moments' of science often play out in complex and 'messy' ways even though they are conceptually distinguishable. The cognitive/noncognitive value distinction is no different.

Returning to the issue of strategy choice, another comparison between DE and Kuhn will prove useful. Kuhn holds that the *fruitfulness* of paradigms provides a ground for choice among them, where ‘fruitfulness’ is here understood as the ability to solve scientific ‘puzzles’ (and the inability to do so leads to ‘anomalies’) (Kuhn 1962, p. 169). Similarly, DE views a strategy’s fruitfulness – the ability to produce soundly accepted theories – as a necessary condition for adopting it in favor of a competing strategy. However, while Kuhn took fruitfulness to be sufficient for the selection of a paradigm, for DE fruitfulness is necessary but *not* sufficient for the selection of a strategy. In addition to fruitfulness, a strategy must produce *significant* theories in order to be adopted in favor of a competing strategy (Lacey 1999, p. 173). According to Lacey, a theory is “significant for specified values if [it] is applicable to important phenomena of daily life and experience and/or is applicable in practical activities in ways that further (and do not undermine) the interests shaped [by those values]” (Lacey 1999, p. 15).

Feminist and materialist strategies can be compared not only in terms of fruitfulness, but also in terms of significance. If feminist strategies are applicable to aspects of the world that are important in light of particular social values (e.g., intentional aspects of phenomena), and materialist strategies are not applicable in that context, then holding those values provides a reason to adopt feminist strategies. I will illustrate this point with an example in the next section.

III. Affinities between Dialectical Empiricism and Longino’s View

The question remains as to whether Longino is likely to consider DE’s interpretation to be a “friendly” variation on her view. In order to determine this, let us consider another case forwarded by Longino in order to see how well it fits with DE’s account of feminist strategies. It should be kept in mind that under feminist strategies the possibilities of interest concern *agency* and *intentional* terminology is often central. And, in contrast, under materialist strategies the possibilities of interest are *material* possibilities and *materialist* terminology (e.g., accounts of phenomena in terms of *laws*) are deployed.

In *Science as Social Knowledge* (1990) Longino discusses theories concerning the "biological basis for behavioral sex differences" (Ch. 7), beginning with the "behaviorist" model:

> [...] in behaviorist psychology explanations must appeal to environmental stimuli. Explanations that describe behavior by means of agents' intentions or that treat states of consciousness as independent variables do not conform to this model and are ruled out by the behaviorist program. [Longino 1990, p. 135]

Here Longino's account fits well with the way strategies function to constrain theory-space according to DE. Intentional accounts of human behavior are not *rejected* on the basis of evidence, but simply elude consideration because they fall outside the constraints of materialist strategies.

Furthermore, Longino makes the important point that this lack of consideration (which, in the idiom of DE, arises due to a lack of alternative strategies) can lead to cognitive blind-spots:

> Proponents of the [behaviorist] model often claim that there is no plausible alternative explanation for the data they present. While they are correct to the extent that there is not currently an alternative biological explanation, they are not correct in supposing that there *could* not be. [Longino 1990, p. 143]

Backing up this claim, Longino presents an alternative 'selectionist' model, which she argues is (potentially) cognitively superior to the behaviorist model *and* serves feminist aims by providing a central role for agency in human behavior. It is notable that this accords well with DE by taking into account considerations of both cognitive value and significance (i.e., applicability to phenomena of social value). While feminist political aims are not legitimate grounds for theory acceptance (this would violate the thesis of impartiality), they *are* legitimate grounds for the adoption of feminist strategies.

There are three main approaches Longino utilizes to support the selectionist model in favor of the behaviorist model. First, she points to sexist bias that improperly influences the behaviorist model via assumptions, e.g., about gender dimorphism (Longino 1990, p. 171). Second, she challenges the legitimacy of 'animal models' used as evidence in the behaviorist approach by arguing that there are salient disanalogies between animals and humans – namely, that humans "exhibit a degree of intentionality not characteristic of the more stereotyped animal behaviors" (Longino 1990, p. 157). Third, she emphasizes the ability of the selectionist model to encapsulate possibilities characterized by intentional behavior, which is not in the purview of the behaviorist model. According to Longino, the selectionist model:

> […] postulates the continual alteration of neural networks in response to experience and action and a role for 'self-inputs,' […] which include not just past associations (memory) of externally generated signals but representations of self as well in the generation of action. […] Experience (including social experience) and self-image, therefore, play a primary role in the *biological* explanation of the behavior/action of species with a highly developed cortex. [Longino 1990, p. 148]

The first two approaches – pointing out sexist bias and challenging assumptions on cognitive grounds – fit well into the framework of DE. The first reveals that *impartiality* has been violated due to the implicit role of sexist assumptions, while the second issues a challenge to the model's cognitive acceptability (by questioning the behaviorist model's empirical adequacy). Notice that the force of these critiques depends upon the cognitive/noncognitive value distinction. The third approach might be understood as merely revealing a deficiency of explanatory power, but it can also be seen a challenge to the *significance* of the behaviorist model. From the perspective of DE, Longino's criticism (the third approach) is not that the behaviorist model fails to meet the constraints of feminist strategies (which in itself has no bearing on its cognitive acceptability), but that it fails to explain adequately intentional features of the phenomena and thus *lacks significance* (c.f., Lacey 1999, p. 221). The selectionist model, on the other hand, is highly significant in this regard, thus supporting the adoption of feminist strategies.

I hope it is now clear that Longino's approach is amenable to the interpretation given by DE. In addition to what we have already seen, there are several affinities between DE and Longino's broader views about the nature of science that bear mentioning. For instance, the characterization of the objective of feminist science that frames feminist strategies is formulated in light of Longino's own description (c.f., Longino 1990, p. 190). There are also poignant similarities between the *strategic pluralism* of DE and Longino's inclination towards *scientific pluralism*, understood as the possibility that different aspects of the world might require the development of different (perhaps incompatible) scientific theories (Longino 2002, ch. 8; Kellert, Longino, and Waters 2006; I return to the issue of theoretical pluralism in science in Ch. 4).

Still, the most striking affinities concern the role of feminist values in feminist strategies. Longino explains that her critique of the behaviorist model points to the "limitations on human capacity imposed by the explanatory model underlying such research" (Longino 1990, p. 190). It is exactly this sort of concern that motivates the need for alternative – in particular, feminist – strategies under which more adequate models can be developed. Finally, both views understand that inquiry guided by feminist values should not be 'responsive to our wishes,' but must be tempered by traditional cognitive values like empirical adequacy. As Longino points out, the selectionist model is supported by feminist values "in combination with the phenomena," thus justifying the 'empiricism' of Contextual Empiricism (Longino 1990, p. 193).

Conclusion

Seen through the lens of DE, Longino's challenge to the cognitive/noncognitive distinction is not so much false as it is a mischaracterization of the distinction. She is right that feminist values *have* cognitive value insofar as they may lead to the development of theories that manifest traditional cognitive values to a high degree, but mistaken in taking feminist values to *be* cognitive values. Furthermore, by formulating rival theories under feminist strategies, theories which are constrained by the feminist values she proposes, sexist (and other forms of) bias can be revealed. Instead of upholding the 'permeability' of the traditional cognitive values, feminist criticism is best understood as adducing cases in which noncognitive values played an improper role alongside cognitive values in the context of theory appraisal. Consequently, the cognitive/noncognitive value distinction proves vital in the success of feminist strategies to provide soundly accepted theories and expose theories that have been wrongly accepted on noncognitive grounds. Finally, by using DE to interpret Longino's case involving behaviorist and selectionist biological models, I have demonstrated that there is an excellent fit between her approach and an account of feminist strategies. The cognitive/noncognitive value distinction is therefore vindicated in light of its explanatory and justificatory role as understood in DE, and as applied in Longino's feminist approach to science.

Chapter 3: Social Values and Inductive Risk in Science

In her book, *Science, Policy, and the Value-Free Ideal* (2009) Heather Douglas argues that the value-free ideal for science is seriously flawed due to the fact that it misidentifies the proper roles for cognitive and noncognitive values in scientific inquiry. She forwards two lines of criticism in this vein. First, Douglas argues that VFI is "too restrictive" in its treatment of *non*cognitive values because it precludes a role for noncognitive values to inform scientists' determination of acceptable levels of "inductive risk." Inductive risk is the risk involved when one uses a theory as a basis for action, given that the theory could be mistaken (false or inaccurate). In Douglas's view, scientists must use noncognitive values to weigh the possible consequences of acting on a theory that turns out to be mistaken. Such considerations are required in order to responsibly assess the appropriate evidential standards for a theory's acceptance prior to its application. I refer to this position regarding the proper role for noncognitive values in the acceptance of theories as the "Inductive Risk Thesis" (IR). Douglas's second criticism is that VFI is "too permissive" when applied to a role for *cognitive* values. VFI allows cognitive values to serve as grounds for theory choice. In contrast, Douglas believes *no values* (neither cognitive nor noncognitive) should ground theory choice. I refer to this view as the "Epistemic Purity Thesis" (EP).

Douglas misidentifies Dialectical Empiricism as a contemporary version of VFI and therefore believes it is susceptible to both lines of criticism. That is, she believes (1) DE is "too restrictive" regarding noncognitive values (i.e., it wrongly rejects IR) and (2) that it is "too permissive" regarding cognitive values (i.e., it wrongly rejects EP). I maintain that DE is not vulnerable to either of these charges. In the most general of terms, Douglas is mistaken because DE does *not* reject IR, but it *rightly rejects* EP.

I begin, in §I, by presenting Douglas's view in more detail. In this section I explicate her taxonomy of values as well as her distinction between 'direct' and 'indirect' roles for values. I then explain her reasons for holding the Inductive Risk (IR) and Epistemic Purity Theses (EP), both of which she (correctly) takes to be inconsistent with VFI. In §II and §III I defend DE against each of Douglas's two lines of criticism:

In response to (1), I argue that DE *does* accommodate a role for noncognitive values in light of inductive risk (it *affirms* IR). I show that Douglas's criticism results from a failure to distinguish among the distinct cognitive attitudes identified by DE; namely, that of 'acceptance' and 'endorsement.' Whereas acceptance involves judgments about a theory's cognitive acceptability, endorsement involves judgments about whether a theory should serve as a basis for action. This distinction helps show that Douglas's view regarding IR actually aligns well with that of Dialectical Empiricism.

In response to (2), which does at least gain traction within DE, I argue that cognitive values should and *do* have an important role in the context of theory appraisal. I demonstrate that, in fact, this role is presupposed by Douglas's own account of the function of cognitive values, which is to help expediently reveal errors within theories. Thus, Douglas cannot coherently endorse EP.

I. Douglas's View

Recall that, in general, philosophers distinguish between values that are relevant to knowledge or understanding (cognitive values) and those that are not (noncognitive values). Some commonly cited cognitive values include simplicity, scope, and predictive power. Noncognitive values constitute all other classes of values (e.g., ethical, social, and aesthetic values), which are thought *not* to be of cognitive significance.[15] Douglas follows the standard use of 'noncognitive,' but draws a further distinction between cognitive values and what she calls "epistemic criteria." She points out that some traditional cognitive values, when manifested in theories, do not necessarily promote the truth or reliability of those theories; accordingly, she considers these to be cognitive *but not epistemic* values. Douglas believes simplicity, explanatory power, predictive precision, and fruitfulness fall under this description. She classifies the (traditionally cognitive) values that *do* have epistemic import as epistemic *criteria* – not epistemic *values* – because she takes them to be necessary conditions for acceptable theories, rather than ideals to be sought (Douglas 2009, pp. 93-94). Examples of epistemic criteria are internal consistency and predictive competency.

Douglas's arguments center around two theses about the proper roles for values in science, both of which run contrary to VFI. First, she advocates the Inductive Risk Thesis (IR), according to which noncognitive values (in particular, social values) can and should enter into the scientific context of theory appraisal, serving to help determine the level of error that should be tolerated for the acceptance of a theory. Second, Douglas argues for the Epistemic Purity Thesis (EP), which says that *no* values (cognitive or noncognitive) should serve as grounds for theory acceptance. Before turning to the details of the two theses, there is one more aspect of Douglas's position that must be explained. She draws a distinction between 'direct' and 'indirect' roles for values, which is central to her views on IR and EP.

[15] An overview of cognitive values is provided in Ch. 1, §I.

Direct and Indirect Roles

Douglas believes that advocates of VFI wrongly focus on the question of which *kinds* of values are proper for science. Instead, Douglas thinks we should inquire into the proper *roles* for values in science. Thus, she identifies two roles that values may play in science: direct and indirect. Douglas explains that if something serves as a "reason in itself" or as "direct motivation" for a decision, then it plays a *direct* role (Douglas 2009, pp. 98-103). For example, if a scientist chooses one theory over another because it is better supported by evidence, then evidence is playing a direct role in her decision. In contrast, the *indirect* role has to do with the inductive risk endemic to science. Inductive risk results from the possibility for error that is ineliminable due to the ampliative nature of the kind of inference to the (probable) truth of scientific theories (i.e., evidential support is a kind of ampliative inference, which cannot guarantee the truth of its conclusions). In the context of theory appraisal, this kind of error is associated with the possibility of accepting a false theory or rejecting a true one. Since every theory has some level of uncertainty, decisions must be made about what level of uncertainty is permissible. If some consideration serves to help determine the significance of error, and thus to aid in judgments about acceptable levels of uncertainty in the acceptance of theories, then it plays an *indirect* role (Douglas 2009, pp. 103-108). For example, suppose scientists are testing the theory (or hypothesis) that a particular drug has no lethal side-effects. If they decide to decrease the chance of wrongly accepting this theory in light of the high value placed on human life (a noncognitive value), then this value is playing an *indirect* role. In what follows we will see that

Douglas's distinction between direct and indirect roles is crucial for both the Inductive Risk Thesis and the Epistemic Purity Thesis.

The Inductive Risk Thesis (IR)

The Inductive Risk Thesis (IR) states that noncognitive values should play an *indirect* role in the context of theory appraisal (i.e., acceptance or rejection) – which lies at the very heart of science. Douglas rightly believes IR is incompatible with the traditional value-free ideal, which does not permit noncognitive values to enter into judgments about theory acceptance. According to VFI science is essentially autonomous with regard to all concerns beyond the truth of theories, and so decisions about theory application are considered external to science proper. On this view it should be of no concern to scientists, *qua* scientists, how their theories are put to use in social contexts or what level of risk is involved. What this overlooks, says Douglas, is that the social context of theory application should influence the evidential standards for theory acceptance. Consequently, she believes VFI is *too restrictive* in its treatment of noncognitive values.

Douglas's position concerning IR traces back to Richard Rudner's influential article, "The Scientist Qua Scientist Makes Value Judgments," (1953) in which he argues that scientists must appeal to noncognitive values when making judgments concerning theory appraisal. In particular, he holds that when assessing acceptable levels of inductive risk scientists may legitimately appeal to social values. If policies or actions informed by a theory would have (say) *dire* consequences should the theory turn out to be false, then scientists have a moral responsibility to subject the theory to *higher* *standards* of testing prior to its acceptance. A few issues need to be unpacked in this sort of claim.

First, the role of noncognitive values in such cases is located in the assessment of the consequences of applying the theory. Whether or not we consider a set of consequences to be ‘dire’ depends on our evaluation of those consequences relative to noncognitive (social) values. For instance, if the consequence would be that several philosophers feel annoyed, then relative to prevailing social values this would not constitute a ‘dire’ outcome. On the other hand, if the result would be the death of millions of people, then we clearly have a case of ‘dire’ consequences. So, noncognitive values influence scientists’ evaluation of the consequences of error and only then can the evidential standards for acceptance be determined.

This leads to a second issue: one may query what it means for a theory to be subjected to ‘higher standards’ of testing. It will suffice to characterize ‘higher standards’ as standards of testing that are more demanding or probative, perhaps involving additional replication of results, etc., than otherwise would have been required if the consequences of error were low or negligible. For example, in null-hypothesis significance testing, a typical standard (“significance level”) is set at .05, which, oversimplifying a bit, we can understand as permitting a 5% chance of error – a 5% chance of rejecting the null-hypothesis when it is true. In the case that the risks involved in theory application are very serious, the standard might be set higher, say, at .01 (permitting only a 1% chance of committing the error of rejecting the null-hypothesis when it is true).

Before elaborating Douglas's view, there is an important objection to Rudner's argument that should be addressed. In a direct response to Rudner's paper, Richard Jeffrey (1956) argues that scientists do not – or at least, need not – accept or reject hypotheses as Rudner suggests. Instead, they need only present the results of their work (e.g., in the form of probabilities) so that, with this information made public, others may interpret their findings and then determine whether the theory is sufficiently supported for a particular practical application.

Rudner's response to Jeffrey, which Douglas also endorses, is that this only pushes the problem back a level, since scientists must still – in a sense – 'accept' the results of their studies (perhaps in the form of probabilities) (Rudner 1953, p. 4; Douglas 2009, pp. 53-54; Jeffrey 1956, p. 246). So, scientists accept theories, or they accept a numerical representation of the evidential support for theories. Either way there are possibilities for error, and noncognitive values are required in order to assess acceptable levels of error. An example will help clarify what Rudner (and Douglas) have in mind.

Imagine a scientist presents policymakers with the degree of confirmation for a theory (for example, as a posterior probability of .99), which is being considered for a high-risk application. It is conceivable, in light of a very high cost of being wrong (the aforementioned 'dire' consequences), that the policymakers ask the scientist, for example, "How *sure* are you that the probability is .99? Is it possible to conduct further tests to confirm this number?" This reveals the sense in which Rudner and Douglas believe even Jeffrey's scientist must 'accept' her results and be open to the possibility of subjecting them to higher standards of testing in light of inductive risk.

These questions, however, show a misunderstanding of what the probabilities mean. Additional tests may raise or lower the posterior probability – they do not make the posterior probability itself more probable. Instead, one might ask, "How sure are you of the truth of the reported evidence (that resulted in this posterior probability)?" Indeed, there are several ways that evidence reports may be unreliable; for instance, the measuring devices could be malfunctioning. These kinds of issues about the reliability of the evidence impact the Bayesian model of theory testing the same way they impact the 'acceptance' model of theory testing. Rudner and Douglas's misunderstanding of this matter (i.e., their mistaken idea that scientists must 'accept' that a theory has, e.g., a posterior probability of .99), reveals that there are really two distinct senses of 'acceptance' at play in their arguments. One the one hand, scientists may 'accept' that a theory is true (or probably true); on the other hand, they may 'accept' that the theory has undergone sufficiently rigorous testing and evaluation to warrant its use as a basis for (some particular) action. This issue will be brought into focus in §II.

Still, if Rudner is right and noncognitive values enter into judgments at the very core of science, then this poses a serious challenge to the traditional formulation of the value-free ideal. Advocates of VFI argue that noncognitive values should not influence the results of scientific research – for if they did, a scientist could accept a theory merely because she wished it to be true or because it aligned with her favored noncognitive values (personal, social, religious, etc.). In response to this worry, Douglas sets out to explain how noncognitive values can play a role in the core of science without jeopardizing science's ability to produce unbiased and reliable knowledge about the world. She achieves this via her distinction between *direct* and *indirect* roles for values. The key is that while noncognitive values are allowed to enter into the context of theory appraisal (contra VFI), they are restricted to an indirect role. Consequently, they do not influence scientific results themselves and they do not serve directly as grounds for theory acceptance; instead, they only influence the evidential standards for acceptance. On this view, noncognitive values may lead to higher standards, but by being limited to an indirect role they are prevented from influencing the content of theories. Douglas's view therefore addresses the concerns raised by VFI without excluding noncognitive values entirely from the context of theory appraisal.

Since noncognitive values have an important role to play, and they can do so without compromising the integrity of science, Douglas concludes that VFI is *too restrictive* in its treatment of noncognitive values. In the next section I address Douglas's second criticism of VFI, according to which it is *too permissive* in its treatment of cognitive values.

The Epistemic Purity Thesis (EP)

According to Douglas's Epistemic Purity Thesis (EP) *no values of any kind* (cognitive or noncognitive) should play a direct role in the context of theory appraisal. It should be noted that, although they are not strictly considered to be values, Douglas's epistemic criteria are precluded from a direct role in this context as well. Theories must satisfy the epistemic criteria in order to be candidates for acceptance in the first place, but these criteria are never "reasons in themselves" for accepting a theory. EP is inconsistent with VFI, since on the traditional account of VFI cognitive values *should* serve (in what amounts to) a direct role in judgments of theory acceptance. On this view, the degree to which theories manifest cognitive values (such as explanatory scope or simplicity) properly serves as grounds for their acceptance (or rejection).

Douglas's critique of VFI on this matter – that it is "too permissive" in its treatment of cognitive values – begins with her conception of the aim of science, broadly construed. To wit, she believes that science aims at the production of "true or reliable" theories about the world. It is clear that she thinks of this goal as being purely epistemic and so maintains that the grounds for theory acceptance must have epistemic import. Thus, *empirical evidence* should play a direct role in theory acceptance, since a theory well-supported by evidence is more likely to be true (or reliable). However, Douglas emphasizes that cognitive values, when manifested in a theory, are *not* indicative of the theory's truth or reliability. For instance a simple theory (e.g., an *ontologically* simple theory) is not necessarily more likely to be true than a complex one; indeed, in some cases an increase in simplicity would seem to require a decrease in accuracy. But it suffices to point out that we have no reason to believe an increase in simplicity always correlates with an increase in the reliability of a theory or the probability that it is true. For this reason Douglas argues that cognitive values should not play a direct role in theory acceptance; after all, she says, "[w]e do not want scientists to reject theories solely because they are not simple, not as broad in scope as their competitors, or seem to have less explanatory power" (Douglas 2009, p. 102).

Now that Douglas's positions have been made clear, her objections to VFI can be summarized as follows: (1) VFI violates the Inductive Risk Thesis by *forbidding an indirect role for noncognitive values* in the context of theory appraisal, and (2) VFI violates the Epistemic Purity Thesis by *permitting a direct role for cognitive values* in the context of theory appraisal. In the remainder of this chapter I explain where Dialectical Empiricism stands regarding IR and EP, and then reassess Douglas's two objections as applied to DE.

In §II, I argue that Douglas's objection to VFI regarding IR (1, above) simply does not apply to DE, which *accepts* IR. However, Douglas correctly portrays DE as permitting a direct role for cognitive values in the context of theory appraisal, thereby rendering DE vulnerable to her objection regarding EP (2, above). Consequently, in §III, I defend DE by supporting its rejection of EP. Furthermore, I argue that a careful look at Douglas's characterization of cognitive values, along with her articulation of the distinction between direct and indirect roles for values, reveals that she should permit a direct role for cognitive values in theory acceptance. I therefore conclude that she should join DE in rejecting EP.

II. Dialectical Empiricism: In Support of the Inductive Risk Thesis

Douglas argues that DE does not accommodate an indirect role for noncognitive values in the context of theory appraisal, thereby violating IR. But before turning to Douglas's criticism, it will be helpful to clarify how IR is actually understood from the perspective of DE. What is crucial here is the distinction between the cognitive attitudes of 'acceptance' and 'endorsement,' which characterize judgments made during the 'moments' of theory appraisal (M_3) and theory application (M_5), respectively. On Lacey's account of DE, *accepting* a theory involves judging that it is supported to a degree that warrants its being included in the corpus of scientific knowledge. In contrast, *endorsing* a theory involves judging only that its support is sufficient to warrant its use as a basis for (some particular) action or application (Lacey 2005, p. 79).

Notably, the standards that must be met for endorsement can be weaker than those required for acceptance, so it is possible to endorse a theory without accepting it. For example, there are some contexts in which theory application carries a low risk and high reward, and therefore does not require a well-supported theory as a basis for action. In such cases one may justifiably endorse a theory (for some specific application) that has not been accepted. This may also occur when the urgency of acting in a situation outweighs the (perhaps even considerable) risks involved if the theory should turn out false. In fact, one might even choose a methodology that prioritizes expedited results over accuracy, purposefully leading to a theory that is suitable to serve as a basis for action (right away) even though it is not a particularly good fit with the evidence (Lacey 2005, p. 79).[16]

It is important to remember that the five 'moments' of science are not temporal moments, they are *logical* moments. So, although judgments of acceptance occur at M_3 and judgments of endorsement occur at M_5, this does not mean a theory is always accepted temporally prior to its endorsement. DE is therefore compatible with the possibility of endorsement without acceptance. However, logical connections between the two cognitive attitudes may yet be identified. According to Lacey's account of DE acceptance *implies* endorsement, since on his view acceptance of a theory requires that it is tested in accordance to the "highest recognized standards" of scientific evaluation. He explains that in order to deny endorsement of a theory one must hold that there is too much remaining uncertainty to support its application. However, on Lacey's view, if the theory has already been tested to the highest standards then there is no known way to reduce that uncertainty. If there *is* a way to reduce the remaining uncertainty by a

[16] See Elliott and McKaughan (2014) for an example and analysis of this sort of case.

significant degree, then that theory simply has *not* been tested to the highest standards. Thus, to deny endorsement of an accepted theory is *ipso facto* to challenge the legitimacy of that theory's acceptance (Lacey 1999, pp. 71-73).

Still, one might worry that a case may arise in which a theory is accepted in accordance with the highest available standards, and yet the consequences of applying the theory, should it turn out false, are so terrible that one nonetheless hesitates to endorse it. This seems to me an idle sort of skepticism. In actual high stakes cases, scientific standards have proven sufficient. For example, there were concerns that an atomic detonation could ignite the atmosphere, or that the particle accelerator at CERN could create an apocalyptic black hole. Both scenarios are about as catastrophic as one can imagine, but the available scientific standards quelled these concerns, even given the inevitable absence of certainty.

This does not capture the whole story, however. Suppose a critic, concerned by the possible worldwide catastrophe, was able to devise a specific test (which had not already been conducted) that would help determine to a greater degree of accuracy whether or not the particle accelerator at CERN would create a black hole. One imagines that upon its discovery the scientists at CERN would have eagerly conducted the novel test, and perhaps admitted that they had been too hasty in accepting (the theory) that the particle accelerator would not create a black hole. This helps reveal how noncognitive values could function to influence judgments of theory acceptance. They do so not by serving as *grounds* for acceptance or rejection of a theory, but by leading to the adoption of higher standards of testing prior to acceptance. Idle skepticism or alarmism regarding the possibility for error (rightfully) should not pose a challenge to theory acceptance. However, specific concerns (framed by social values) may play an important role by motivating scientists to seek out new evidence or develop innovative ways of testing a theory against existing evidence.

Bayesian Dialectical Empiricism: Acceptance vs. Warranted Degree of Confidence

The notion of "theory acceptance" may not be an essential feature of DE. In fact, other options might be preferable, given some of the peculiarities it generates in Lacey's view. On his account "sound acceptance" implies that a theory has been tested to the "highest possible standards." This paints an awkward picture in cases where new evidence is discovered or new tests are devised on an ongoing basis, *after* a theory has already been accepted. When this happens, as in the CERN example, or with ongoing discoveries in evolutionary biology (which, for example, lend further credence to accepted theories of human descent), Lacey is forced to say something along the lines that the theory "was not soundly accepted after all" or that "it *was* soundly accepted, but now (in light of the discovery of higher standards) is not soundly accepted."

Some Bayesian approaches to the evaluation of scientific theories maintain that theories are not to be 'accepted.' Rather, the best that empirical evidence can establish is that one theory is *much more likely* (much more highly confirmed by the evidence) than any recognized alternative theory. This stance toward scientific theories – the judgment that a particular theory (among the live options) is the "best available theory," and so warrants a high degree of confidence – could replace that of 'acceptance' in the context of theory appraisal. This view resonates well with the fact that theories are never proven true, and so should always be subject to additional testing. It therefore avoids the sorts of awkward cases that result from Lacey's notion of acceptance. On a

Bayesian account of DE, the moment of theory appraisal (M_3) involves the ongoing assessment of the evidential support for theories, rather than acceptance. In particular, one theory may be recognized to be much more strongly supported by the body of available evidence (made much more likely by the evidence) than any recognized alternative theory.

On this account, the cognitive attitude of *endorsement* needs no revision. Noncognitive values may still play a role in determining whether the degree of support for a theory is sufficient for it to serve as a basis for (some particular) action or application (e.g., take that action that, given the probabilities supplied by the evidence, is the act that maximizes expected utility), even if the theory should turn out false. I think that DE is consistent with such a view – i.e., a Bayesian version of DE is perfectly coherent. But the main interlocutors in the values controversy tend to think in terms of an approach that draws on theory acceptance, so I will continue to follow suit.

Whether DE invokes the cognitive attitude of *acceptance* or of *warranted confidence* in the best available theory, the role for noncognitive values within the framework of DE *affirms* the Inductive Risk Thesis. In so doing, DE diverges from the traditional value-free ideal for science. Nonetheless Douglas portrays DE as a contemporary version of VFI.

Douglas's Misconstrual of Dialectical Empiricism

Douglas characterizes Lacey as "another defender of the acceptable versus unacceptable values in science distinction" (Douglas 2009, p. 91). She claims he is among the proponents of VFI, "arguing for a strict distinction between cognitive […] and noncognitive […] values, and for the exclusion of the latter from scientific reasoning" (Douglas 2009, p. 17). Although any formulation of DE will maintain an important distinction between cognitive and noncognitive values, on *no* account does DE require that noncognitive values should be excluded entirely from scientific reasoning. As I have continued to emphasize, noncognitive values play crucial roles, especially at M_1 and M_5, both of which are central to scientific practice. Noncognitive values are only excluded at the moment of theory appraisal. Furthermore, as we have just seen, the moment of theory application can influence the standards for theory acceptance within the moment of theory appraisal. Although the roles for cognitive and noncognitive values are kept distinct, the different contexts of science are deeply interconnected.

Another of Douglas's objections concerns Lacey's definition of theory acceptance, which Douglas faults, since Lacey "defines acceptance of a theory solely in terms of whether or not more research needs to be done on it, ignoring the multiple pragmatic aspects of acceptance, such as whether to use a theory as a basis for decisionmaking" (Douglas 2009, p. 63). However, as should be clear by now, DE (on any version or variation) does not exclude the 'pragmatic aspects,' but distinguishes between cognitive attitudes relevant to theory appraisal at M_3 (e.g., *acceptance* or *warranted degree of confidence*) and theory *endorsement*, the latter of which encapsulates (pragmatic) judgments about "whether to use a theory as a basis for decisionmaking."

Douglas's objections trace to this distinction, but it is not clear what exactly Douglas finds objectionable about it. In fact, others have criticized Douglas for failing to make this very distinction in her work. For example, in his article "Direct and

Indirect Roles for Values in Science," (2011) Kevin Elliott explains why the distinction is needed:

> When philosophers argue that [noncognitive] values have a proper role to play in science, one must distinguish whether they are arguing that values have a role to play in *practical* decisions about what claims to *accept* as a basis for action or whether they are also addressing *epistemic* decisions about what claims to *believe* as true. [Elliott 2011, p. 23]

When the distinction is presented in this way, it is clear that Douglas's view is actually consistent with DE: noncognitive values should not play a role in decisions about what claims to believe as true, but they should play a role in decisions about what claims to accept as a basis for action.

Now it is clear that Douglas wrongly attributes the rejection of IR to Dialectical Empiricism. In fact, DE maintains that noncognitive values properly play a role in determining acceptable levels of inductive risk and that in doing so they may influence the standards for theory acceptance. Next we turn to the Epistemic Purity Thesis, which represents a genuine source of conflict between DE and Douglas's view.

III. Dialectical Empiricism: Against the Epistemic Purity Thesis

As explained in §I, Douglas believes that in the context of theory appraisal *no values of any kind* (cognitive or noncognitive) should play a direct role. DE aligns with Douglas's view that *non*cognitive values should not play a direct role. After all, if they were permitted a direct role in this context scientists' subjective preferences would be considered a legitimate influence on their judgments about the acceptance (or rejection) of theories. However, contrary to Douglas, DE maintains that cognitive values *should* play a direct role in this context. This role for cognitive values is justified because theories which manifest them are conducive to scientific aims.

Of course, various accounts of the aims of science are plausible. Indeed, no single formulation of them is widely accepted, so it is beyond the scope of this chapter to settle the matter here. However, a more tractable project is to identify and query certain features or presuppositions involved in a given formulation. This will be the key to understanding why Douglas holds EP, and also why she should ultimately reject it. There are at least two problems with an account of the aims of science that leaves *no* role for cognitive values. First, such an account is highly implausible in light of actual scientific practice, in which undeniably cognitive values sometimes serve in a direct role in the evaluation of theories. Strictly speaking, this fact does not invalidate Douglas's view, given that her account is a normative one. Nonetheless, there is reason to question her view if episodes of characteristically good scientific practice tend to violate the norms she identifies. The second, more serious problem is that such an account of the aims of science is incompatible with Douglas's own articulation of the nature of cognitive values. The latter problem is the focus of the remainder of this section.

The Role of Evidence in Theory Choice

First, we should be clear about Douglas's understanding of a 'direct' role in scientific inquiry. She says that in the context of theory appraisal, if values play a direct role, they "act as reasons in themselves to accept a claim, providing direct motivation for the adoption of a theory" (Douglas 2009, p. 96). She explains why a direct role is improper for values in this context:

> […] if values were allowed to play a direct role in the acceptance or rejection of scientific theories, an unpalatable theory could be rejected regardless of the evidence supporting it. […] These problems are not just limited to social values in a direct role; they hold for cognitive values as well. We do not want scientists

> to reject theories solely because they are not simple, not as broad in scope as their competitors, or seem to have less explanatory power. […] If we allow values to play a direct role in these kinds of decisions in science, decisions about what we should believe about the world, about the nature of evidence and its implications, we undermine science's ability to tell us anything about the world. [Douglas 2009, p. 102]

Here we begin to see that Douglas rejects a direct role for cognitive values because they would undermine what she considers to be a fundamental aim of science, its "ability to tell us about the world." Douglas goes on to explain that theories which manifest cognitive values are not more likely to be "true or reliable," so they should not serve as "reasons in themselves" for accepting a theory (Douglas 2009, p. 106-107). What is at issue, then, is the epistemic nature of cognitive values.

In fact, there are precedents for challenging the purely epistemic nature of cognitive values (e.g., Laudan 2004), which will be discussed below. But first it is important to understand Douglas's claim that *only empirical evidence* should play a direct role in judgments of theory acceptance. If a "reason in itself" is understood as a reason which serves as the sole motivation for a decision, then Douglas's conclusion seems inevitable. Certainly we do not want scientists to adopt a theory *solely* because it is simple or reject it *solely* because it is more complex than its rivals. Yet this worry seems unfounded, since in actual practice it is unlikely ever to occur. Still, perhaps scientists should base such decisions *solely on the evidence*; this, at least, has a ring of plausibility.

Although it sounds reasonable, a direct role for evidence as a reason in itself for theory acceptance gives rise to significant problems. One familiar problem stems from the underdetermination of theory by evidence, in light of which Douglas's ideal is *impossible* to realize, since if underdetermination is true even an abundance of evidence does not (and cannot) support *any* theory in the absence of background assumptions, auxiliary hypotheses, etc. (e.g., Longino 1990). Evidence therefore cannot serve as the sole motivation for theory choice in the sense that Douglas sometimes seems to intend. But perhaps this is to place too much emphasis on the notion of a "reason in itself," understood as the *sole* motivation for a decision. Douglas also characterizes items serving in a direct role as a "direct motivation for the adoption of a theory," and understood in this way evidence can serve in a direct role, even if it must be supplemented with background assumptions, etc., before it can be brought to bear on a theory.

A Direct Role for Cognitive Values in Theory Choice

Unfortunately, recasting direct roles in this way solves one problem by creating a new one. Evidence can provide "direct motivation" for the adoption of theories that are true and reliable, yet utterly useless and uninformative. To elaborate this point I will draw on remarks by Larry Laudan, who explains the need for "cognitive, but nonepistemic" values as follows:

> Bas van Fraassen famously argued that a theory does not have to be true to be good. We can add to that dictum a new twist: a theory does not have to be false to be bad. A theory may be bad because it fails the test of possessing the relevant nonepistemic virtues. In other words, we expect our theories to do much work for us, work of a sort that most merely true statements fail to do. However, we may cash out precisely what that additional work is, and when we do so, we will move beyond the epistemic realm into one I call cognitive but nonepistemic values. Such values are constitutive of science in the sense that we cannot conceive of a functioning science without them, even though they fail to be intelligible in terms of the classical theory of knowledge. [Laudan 2004, p. 19]

Laudan holds that a good theory should not merely be true, but should also have (e.g.) wide explanatory scope and predictive power. He argues that such cognitive, but nonepistemic values should serve (and have served) as legitimate reasons for choosing one theory over another (Laudan 2004, p. 17). Douglas agrees with Laudan that some items commonly cited as epistemic or cognitive values, when manifested in a theory, do not seem to indicate that it is more likely to be true or reliable (e.g., a simple theory is no more likely to be true than a complex one). Indeed, this very concern largely motivates Douglas's view that cognitive values should not play a direct role. But Laudan's point is that scientists do not seek *merely* true theories; while truth is always a scientific ideal, it alone is not sufficient for a *good* theory. In addition to truth, cognitive values (even if nonepistemic) are vital desiderata of scientific theories.

While Laudan holds that cognitive values are constitutive of good theories, Douglas takes them merely to be pragmatic. She argues that cognitive values are useful because "a theory reflecting cognitive values will be more likely to have its flaws uncovered sooner rather than later" (Douglas 2009, p. 107). Thus, she ties the significance of cognitive values to the uncertainty endemic to science, which suggests that they properly play an *indirect* role. Notably she does not say that scientists should *choose,* or even *prefer*, theories that manifest cognitive values, but that they "should care about cognitive values because of the uncertainties in scientific work, not because one really wants simpler, more broadly scoped, more fruitful theories for their own sake" (*ibid.*). This claim sounds plausible – indeed there must be something right about it – but the result is that cognitive values do not fit well into Douglas's presentation of the structure of values in science.

If cognitive values play an indirect role, then they enter into judgments concerning "the sufficiency of evidence, the weighing of uncertainty, and the consequences of error, rather than the evaluation of intended consequences or the choices themselves" (Douglas 2009, p. 103). But if we take seriously her characterization of cognitive values as "an insurance policy against mistakes" (Douglas 2009, p. 107), then cognitive values do not function in an indirect role. If the function of cognitive values is to help reveal errors – and *not* to weigh their importance, etc. – then to say that scientists should 'care' about cognitive values must mean that *they should prefer theories which manifest them*. If this is right, then cognitive values provide a direct motivation – though not the *sole* one – for accepting a theory. Consequently, the significance of cognitive values, given Douglas's own description, seems to place them squarely in a *direct* role.

Douglas resists this conclusion by emphasizing that we do not *really* want, e.g., simple theories "for their own sake." But as I pointed out earlier, it is hard to imagine that *any* scientist worth her salt would accept a simple theory *solely* because of its simplicity. Perhaps this is an uncharitable interpretation of Douglas – she might not mean to say that simplicity should not be allowed to be *the one and only factor* involved. Alternatively, she might have in mind a scenario in which one among empirically equivalent rival theories is chosen because it is simpler than the rest. Let us consider this possibility. Imagine a case in which rival theories are equal in all respects save their degree of simplicity. Further, suppose that scientists allow simplicity to serve as a 'tie-breaker.' While simplicity may provide a direct motivation in this case, it is still not the sole reason a theory becomes accepted. This is true even if it ends up being the *decisive* factor. Presumably, in addition to its simplicity, the accepted theory in this scenario manifests other qualities (such as empirical adequacy) and it necessarily satisfies Douglas's epistemic criteria (otherwise it would not have been a candidate for acceptance). Thus, there is no good reason to think that a theory accepted under such conditions would thereby fail to "tell us anything about the world," as Douglas worries.

The 'Cognitive' Goals of Science

The tension in Douglas's treatment of cognitive values stems from her assumption that the goals of science are purely epistemic. If the goals of science *were* purely epistemic, then it would make sense to adopt an ideal according to which judgments of theory acceptance only involved epistemic values. However, as Laudan points out, the goal of science is not merely the production of true beliefs. If cognitive values are characteristics of theories that promote a broader sense of scientific understanding, then those values *are* "reasons in themselves" for theory acceptance. In her vague claim that scientists should 'care' about cognitive values, Douglas has already placed her view in a position that makes it difficult to deny a direct role for cognitive values. Since her assumptions about the narrowly epistemic nature of the goals of science are untenable, her view is better served by the adoption of cognitive values to play a direct role in the context of theory appraisal.

Permitting a direct role for cognitive values need not lead to the sorts of problematic cases that are Douglas's chief concerns. In his article, "Values in Science beyond Underdetermination and Inductive Risk," (2013) Matthew Brown suggests that what Douglas really wants to capture in her direct/indirect role distinction is the "lexical priority of evidence over values" (p. 834). By "lexical priority" he means that if there is a conflict between evidence and values, evidence always takes precedence. If scientists adopted a rule of lexical priority of evidence, then, for example, it would not be permissible to accept a simple theory when a complex but much more well-confirmed rival theory is available. Thus, cognitive values could play a direct role as long as scientists give priority to empirical adequacy, a high degree of confirmation, etc., so that the evidence always 'wins' in competition with cognitive values. I do not mean to suggest a principle of lexical priority is necessarily the best option for Douglas; what is important is that there are ways for her to allow a direct role for cognitive values without compromising her account of the integrity of science.

Conclusion

Douglas argues that within the context of theory appraisal DE (along with VFI) is *too restrictive* because it does not allow a role for noncognitive values (in opposition to IR). She argues that DE (and VFI) are *too permissive* because it allows a direct role for cognitive values (in opposition to EP). Now it should be clear that both of Douglas's objections fail. DE *does* accommodate an indirect role for noncognitive values in its treatment of inductive risk, and so affirms IR. Indeed, consonant with Douglas's position, DE holds that noncognitive values can both enter into judgments about theory endorsement and influence the standards required for theory acceptance. Furthermore, while DE does allow a direct role for cognitive values, it is *wrong* to thereby characterize this view as being "too permissive." Given that the aims of science are not purely epistemic – truth is not the sole aim of scientific theories – cognitive values *should* play a direct role in theory acceptance. The tension in Douglas's views reveals that in fact it is *her* account that is too restrictive because it excludes a direct role for cognitive values in theory acceptance. Dialectical Empiricism is therefore vindicated on both counts.

Chapter 4: Complexity, Values, and Pluralism in Science

Dialectical Empiricism (DE) is compatible with a broad range of metaphysical and epistemological views about science. For instance, we have seen (in Ch. 3) that it can accommodate different epistemologies within the context of theory appraisal, including the notion of "theory acceptance" as well as the Bayesian notion of "warranted confidence." DE permits such variations as long as they are consistent with values playing their proper roles throughout the different 'moments' of scientific activities. So, for example, whether one adopts the 'acceptance' or Bayesian variant of DE, the thesis of impartiality must be upheld; that is, noncognitive values must not serve as grounds for the evaluation of theories in the moment of theory appraisal.

Along similar lines, Lacey notes that his version of DE is consistent with realist as well as anti-realist interpretations of science. In the introduction to *Is Science Value Free?* (1999), he writes:

> Throughout the book, I have used a *realist idiom*, and, except when explicitly noted, I discuss the objectives of science in *broadly realist terms*. My intention is not so much to endorse realist interpretations of science, as to show that, even with realist interpretations, which are usually bearers of the idea of science as value free, important criticisms of it can arise. I am confident that my arguments can readily be restated within, for example, empiricist perspectives [Lacey 1999, pp. xv-xvi]

Lacey uses a "realist idiom" because he thinks a scientific realist is more likely (than, e.g., a scientific instrumentalist) to advocate VFI. In fact, perhaps serving as evidence that he is right, this tactic may have backfired by leading some of his readers to believe that DE is nothing more than a contemporary version of the value-free ideal (e.g., Steel 2011; Douglas 2009). His goal, however, was to make the strongest possible case for

DE by showing that even a realist version of it makes good sense of the role of noncognitive values in science.

In this chapter I probe the boundaries of DE with respect to its relation to different interpretations of scientific theories; namely, theoretical monism and theoretical pluralism. I treat theoretical monism as the view that only one theory about any given domain of phenomena should be accepted. Theoretical pluralism I treat as the view that more than one theory – *possibly incompatible theories* – can be accepted for a domain of phenomena, given that they have different explanatory goals. I chose a form of theoretical pluralism that admits the possibility of accepting incompatible theories for the same reason Lacey chose a realist idiom. If DE is compatible with *this* version of theoretical pluralism, then it is compatible with all (or most) versions.

However, as may have been the case with Lacey, this tactic could backfire. It might seem that realism entails the *unity of science* thesis (as a gloss: the view that we can discover a 'theory of everything'), which then precludes the kind of pluralism I explore in this chapter. Thus, certain assumptions might lead one to jump to the conclusion that realism entails theoretical monism (just as one might assume all realists advocate VFI). Thus, I want to be very clear that *pluralistic realism* (theoretical pluralism conjoined with scientific realism) *is viable*, even though a monistic interpretation is perhaps the norm. In short, even though there is just *one world,* it may not be possible to represent the one world with only *one theory*. The realist may not feel at home with theoretical pluralism, but she is nonetheless welcome to it.

I argue that DE is compatible with both theoretical monism and theoretical pluralism, but that a version of DE which incorporates the latter is especially promising.

This is because there are affinities between the *strategic* (or methodological) pluralism of DE and *theoretical* pluralism. I will focus on Sandra Mitchell's version of theoretical pluralism, which she calls "Integrative Pluralism" (IP). The 'pluralisms' of DE and IP result from the way the world's complexity (of specific kinds) reveals explanatory limits for scientific inquiry. For DE, complexity among *values* generates explanatory goals that call for a plurality of *strategies*. For IP, complexity among *biological phenomena* does the same for a plurality of *theories*, a plurality that DE can also accommodate. Furthermore, the sense in which each 'calls for' pluralism is *pragmatic*; that is, pluralism may not be the only choice, but it is the prudent choice given the present state of scientific knowledge.[17] Building upon these similarities, I argue that an approach to DE that incorporates (something like) IP bears further exploration.

First, in §I, I establish that DE is compatible with theoretical *monism*. This requires explanation given that DE, on any account, maintains strategic *pluralism*. Next, in §II, I explain theoretical pluralism in detail, focusing on Mitchell's "Integrative Pluralism," in order to lay the foundation for comparison between DE and IP. Along the way I examine several other versions of theoretical pluralism to clarify Mitchell's position. I also establish that DE is compatible with theoretical pluralism, despite the fact that some versions of DE hold *external consistency* as a cognitive value (which seems to contradict the possibility of accepting incompatible theories). Finally, in §III, I elaborate on the affinities between DE and IP, and provide some reasons why a version of DE that incorporates IP bears further investigation and development.

[17] Please note that being pragmatic about which truths to pursue, or how to pursue them, does not entail a purely instrumentalist view of science. In this sense, at least, realists can be pragmatists too!

I. Theoretical Monism

I will treat theoretical monism as the view that only one theory should be accepted for a given domain of phenomena. The basic motivation for monism is quite straight forward. As Sandra Harding puts it, on the monistic view "there exists just one world [and] one and only one possible true account of it" (Harding 1998, p. 166). Intuitively, when faced with a choice of two inconsistent theories at most one should be accepted. After all, the acceptance of both would mean that at least one false theory has been accepted. On the other hand, if two theories are consistent they should be open to unification as part of the 'one true account' of the phenomena of interest. Thus, whether or not the theories are consistent, ultimately only one theory should be accepted.

On this view, it is good to entertain and investigate a plurality of competing theories. Only one should be accepted, but by considering a wide range of theories we increase our chances of discovering *the* right one. As Sandra Mitchell quips, "The slogan here might be 'Pluralism: the Way to Unity'," since pluralism in this sense is "temporary and strategic, but ultimately eliminable" (Mitchell 2002, p. 57). In such cases pluralism can be eliminated, for example, by reducing 'higher level' to 'lower level' theories. While it may be useful to investigate phenomena from a plurality of perspectives, they are all still aimed at the 'one true account' of the world.

Dialectical Empiricism is consistent with theoretical monism as long as it does not preclude values playing their proper roles at the different 'moments' of scientific activities. In the moment of theory appraisal Lacey identifies external consistency as one of the cognitive values in light of which theories are assessed. Moreover, in his formulation of the thesis of impartiality it is made explicit that if a theory is accepted for a domain of phenomena, then inconsistent rival theories should be rejected (Lacey 1999, p. 234). Thus, the monist's insistence that contradictory theories cannot both be accepted is easily accommodated. Still, the moment of strategy adoption might be expected to conflict with theoretical monism, as it supports strategic/methodological pluralism.

The question is whether a plurality of strategies is consistent with the view that ultimately only one theory should be accepted for a given domain. On this matter I defer to Helen Longino, who provides an excellent explanation of why pluralism about strategies/methodologies is in fact consistent with theoretical monism:

> The approach I advocate requires pluralism for the conduct of inquiry, but a characteristic of a process need not be a characteristic of its product. This approach does not preclude the possibility of some single all-encompassing theory being the final result of inquiry nor does it preclude the possibility of multiple accounts. It is logically possible that all social and material cognitive interactions should come to an end in a single unified story, but also possible that they should end with a multiplicity of partially overlapping, but noncongruent accounts. [Longino 2002, p. 140]

Longino's claim in this passage is actually much stronger than is required given my characterization of theoretical monism. Whereas I am arguing that strategic pluralism is consistent with the view that only one theory should be accepted *for a domain of phenomena*, Longino maintains that it is consistent with the view that ultimately only one grand unified theory should be accepted (full stop).

II. Theoretical Pluralism

Pluralism in science comes in many forms, one example of which is the strategic/methodological pluralism of DE. However, the question at hand concerns pluralism regarding scientific theories, a kind of pluralism which itself can be understood in different ways. I will focus primarily on two specific versions of theoretical pluralism, Sherman's "Levels of Analysis" and Mitchell's "Integrative Pluralism," which I argue are ultimately compatible. Exploring these different versions of theoretical pluralism will lay the foundation for the comparison of DE and IP in §III.

Sherman's Levels of Analysis

Paul W. Sherman gives his account of theoretical pluralism in his article "The Levels of Analysis" (1988). Building on the prior work of Mayr (1961) and Tinbergen (1963), Sherman explains that theories within biology can be categorized into explanatory 'analytical levels' which are differentiated by the types of questions they address. For example, at the ontogenetic level there are "proximate" questions about *how a trait develops* and at the evolutionary level there are "ultimate" questions about *why a trait evolved* (Sherman 1988, p. 616). Sherman maintains that theories at different levels are never in competition since they answer distinct questions. On the other hand, he thinks it is possible for theories within a level to compete. In short, Sherman's view is that "competition between alternatives appropriately occurs within and not among levels" (*ibid*.).

Sherman's goal is to resolve conflicts between developmental and evolutionary biologists who disagree about which theory – ontogenetic or evolutionary – is *the* correct explanation for a given trait. They can *both* be correct, on his view, since a trait "can be explained from several different, but not mutually exclusive, perspectives" (Sherman 1988, p. 616). Notably, Sherman often adds that no analytical level "supersedes" or is "inherently superior" to other levels (Sherman 1988, p. 618; Alcock and Sherman 1994, p. 61). Thus, the claim that, e.g., ontogenetic theories give 'better' answers than evolutionary theories is fundamentally mistaken because theories at each level provide answers to different questions. Still, perhaps the scientists engaged in such disputes really mean to say that some *questions* (or analytical levels) are more important or more valuable than others. If so, Sherman's arguments do not directly address the issue. The most he can say is that we do not generally need to choose between theories at different levels, so the alleged 'superiority' of one level or another is irrelevant regarding theory choice.

Theoretical Pluralism and Inconsistent Theories

Michael Dickson (2006) provides another example of pluralism resulting from distinct explanatory contexts, but in the realm of quantum physics. To be clear, his view is not identical to Sherman's *Levels of Analysis*, which only targets pluralism in biology (though it is easily extended to other domains of inquiry). A brief tangent is worthwhile because Dickson explains how some explanatory contexts call for a pluralist approach that condones *contradictory* theories. As I explain in more detail later in this section, while Sherman makes the case that theories at different levels do not compete with one another, he does not say much about whether they might nonetheless exhibit inconsistencies.

In his essay, Dickson discusses the development of dynamics within quantum mechanics for properties of individual systems, rather than for probability distributions of a system's quantum state (Dickson 2006, loc. 1093). He explains that there are many possible equations for dynamics that satisfy the general constraints of quantum theory and are consistent with its empirical content (Dickson 2006, loc. 1207). However, there are different explanatory contexts that call for dynamics, and there are different constraints involved in these contexts. Dickson points out that in some contexts scientists must appeal to the principle of relativity and in others the principle of stability. However, no dynamics can satisfy the constraints of both principles (Dickson 2006, loc. 1175). He therefore recommends a pluralist approach, which recognizes the need for different – even contradictory – dynamics in light of the different explanatory goals within quantum physics.

As we see in Dickson's example, some forms of theoretical pluralism are open to the possibility that there are inconsistent or incompatible theories which may legitimately be accepted for a domain. This defies the intuition that at most one among inconsistent theories should be accepted. Regarding this point, Kellert, Longino, and Waters, proponents of what they call the 'pluralist stance,' explain this position:

> A pluralist would draw this conclusion in some cases [i.e., that only one of two inconsistent accounts should be accepted], but a pluralist is also open to the possibility that the situation is such that it is impossible to accurately represent all aspects (even all aspects of interest) with a single model. [Kellert, Longino, and Waters 2006, loc. 129]

The incompleteness or partial nature of scientific representations helps justify the need for inconsistent or incompatible theories. This is an important theme in pluralist thought and plays a central role in Sandra Mitchell's view, to which we now turn.

Mitchell's Integrative Pluralism

Sandra Mitchell maintains that scientific theories are always *partial* representations of phenomena. She explains that "[a]ny representation is at best partial, idealized, and abstract [...]. These are features that make representations usable, yet they are also features that limit our claims about the completeness of any single representation" (Mitchell 2009, p. 12). This incompleteness is exemplified, for example, in theories (ideal models) that focus on single isolated causes; such representations inevitably exclude or obscure some features (e.g., via simplifying assumptions) in order to focus on the features that are salient given one's interests. It is in virtue of the incompleteness of scientific representations that Mitchell rejects reductionist arguments that scientific accounts of higher-level phenomena are *in principle* reducible to some lower, perhaps fundamental, level of analysis or description (Mitchell 2009, p. 30).

Whatever level we take as fundamental, representations at that level are still partial and incomplete. To put it simply, there is always a difference between a scientific representation and that which it represents – if there were no difference the representation would be explanatorily inert and useless as a guide to interaction. This is true even in the realm of physics, since "[t]he local, contingent components of every causal process are just not included in the scope of physical theory or its abstract language" (Mitchell 2009, p. 32). Both lower and higher-level representations are inevitably incomplete, so there is no guarantee that they are incomplete *in the same way* such that one is reducible to the other (Mitchell 2003, pp. 182-183).

The incompleteness of scientific representations is important because it means theories can be incompatible (i.e., they might omit or obscure different aspects of phenomena) while nonetheless accurately encapsulating certain other aspects. This can be true even of theories aimed at explaining the same phenomena. For example, Mitchell examines three models explaining the division of labor in social insects, each of which focuses on distinct causal factors: genetic diversity, learning diversity, and architectural diversity (Mitchell 2003, p. 210-213).

According to the *genetic* diversity account, genetic variation influences the threshold at which stimuli (available tasks) lead an individual insect to begin working. Thus, genetic diversity yields a division of labor as insects responding to stimuli at different thresholds end up specializing in different tasks (Mitchell 2003, pp. 211-212). According to the *learning* diversity account, individuals share the same 'learning algorithms,' but as workers are born at different times they will be at varying stages of learning when they encounter stimuli. Thus, division of labor is understood as a result of the learning algorithm and age distribution of individuals, rather than genetic variation (Mitchell 2003, p. 213). Last, on the *architectural* diversity account, division of labor is explained in terms of nest architecture. On this model, individuals are born in one location (the 'brood pile') and then seek out the nearest available task. A pattern emerges where tasks near the brood pile tend to be performed by younger individuals, while more distant tasks are performed by older individuals. Thus, the architectural diversity account explains division of labor in terms of the age of individuals and the proximity of various tasks to the brood pile (Mitchell 2003, p. 212).

These models are incompatible in the sense that each isolates a particular cause and ignores other possible causes. According to Mitchell, this incompatibility is acceptable at the theoretical level. She maintains that a plurality of theories is sometimes the best (and perhaps only) way to grapple with *complexity* in the world. Regarding the models of division of labor Mitchell points out that "the diversity of the 'solutions' to adaptive problems and the historical contingencies influencing those variable paths" renders unification of the three models implausible (Mitchell 2003, p. 217). In other words, the monist's goal of unification is sometimes foiled by complexity (evolutionary complexity, in Mitchell's example). When theoretical unification is not forthcoming, theoretical pluralism becomes a salient option.

Although models (such as those in Mitchell's example) can be incompatible at the theoretical level, Mitchell believes it is crucial that this theoretical pluralism does not lead to incompatibilities *in application*. When applied in specific contexts, theories must be amenable to integration, which means their empirical content must be consistent. Mitchell explains:

> One expects neither that the actual processes in need of explanation will directly reflect the content of the models nor that the theoretical models will be modified to incorporate the details of all potential causal factors in a general framework. Rather, the actual configuration of complex processes resulting from [a variety of sources] must be determined on a case-by-case basis. Thus, pluralism at the theoretical level does not entail pluralism with respect to the explanations of specific cases. [Mitchell 2003, p. 207]

Mitchell's conclusion in this passage is well-illustrated by her example case. Each of the accounts of division of labor – genetic, learning, and architectural diversity – focuses on a single causal factor while obscuring others, which results in incompatibility and pluralism at the theoretical level. However, in application to concrete cases no model has the resources to account for all the causal factors involved. Thus, an integrated account which draws upon several models will be necessary for an adequate explanation. This integrated account must be consistent, since "there is only one causal history that, in fact, has generated the phenomenon to be explained" (Mitchell 2003, p. 216).

<u>Mitchell's Critique of Sherman's View</u>

Mitchell presents *Integrative Pluralism* as an incompatible alternative to Sherman's *Levels of Analysis* approach. The source of the dispute is about how theories can conflict or compete with one another. Mitchell presents arguments against Sherman's view, which she takes to be that (1) theories at different levels 'cannot conflict' and (2) theories within a level 'can conflict' (Mitchell 2003, p. 214). An immediate concern arises due to the terminology Mitchell deploys here. Sherman consistently talks about 'competition' and not 'conflict' among theories. This will have to be kept in mind throughout the discussion of Mitchell's criticism.

Mitchell's first point is that theories at different levels *can* conflict because they are not explanatorily closed (i.e., their explanations may overlap). By this she means that a theory at one level may have empirical content that imposes constraints on the range of possible explanations at another level. For example, there are developmental (ontogenetic level) constraints on the range of possible evolutionary explanations for particular traits (Mitchell 2002, p. 59; Mitchell 2003, pp. 214-215).[18] Mitchell provides an example involving social insects:

> […] some emergent features of a social insect colony arise 'for free' just by the interaction of the individuals. By appealing to natural selection, evolutionary explanations require ancestral variation in a trait. The possibility that natural selection is the major source of the prevalence of a trait in a population can be challenged by proximate explanations that give evidence for the implausibility of variation having occurred in the past. [Mitchell 2009, p. 112]

Her example shows that even though theories at different levels are answering different types of questions, their explanations can overlap in some areas thereby creating room for conflict.

Mitchell's second dispute with Sherman is about conflict among theories within a single level. Central to Mitchell's view is that theories are *partial* representations. Consequently, a plurality of theories at the same level can differ in virtue of

[18] Bateson and Laland (2013) examine additional ways that theories at different levels can interact.

representing different aspects (e.g., different causal features) of the same phenomena. I will call this a difference of *focus*. Mitchell maintains that theories within a level *with different focuses* cannot conflict. This is because theories focusing on different causal features "abstract away the operation of other compounding factors, and hence make no incompatible claim about their operation" (Mitchell 2003, p. 205-206). She takes Sherman's position to be incompatible with Integrative Pluralism because she thinks he is committed to the view that competition *can* occur between theories within a level that differ in focus.

Assessing Mitchell's Arguments

First consider Mitchell's claim that, contrary to Sherman, theories at different levels can conflict. It is true that according to the *Levels of Analysis* view theories about the same phenomena at different analytical levels are not in *competition*. However, this does not mean that the different levels are entirely autonomous or necessarily free from *conflict*. Sherman appears to be aware that there is explanatory overlap between levels; he notes that "there is a general awareness that it is helpful to use evolutionary theory to inform proximate [ontogenetic] research, just as it can be productive to use knowledge about internal mechanisms and ontogenies to raise interesting ultimate [evolutionary] questions" (Alcock and Sherman 1994, p. 61). Indeed, this 'awareness' was evident in the works of Mayr and Tinbergen on which Sherman's view is based. In his discussion of different subfields within biology, Mayr explains that there are "many points of contact and overlap" and that "[a]ny biologist working in one of these fields must have a knowledge and appreciation of the other field" (Mayr 1961, p. 1501). Similarly, Tinbergen, purporting to contribute a bit of "plain common sense," points out that “[t]here is, of course, overlap between the fields covered by these questions […]” (Tinbergen 1963, p. 411).

Although Sherman does not directly address the issue of theoretical *conflict*, in one passage he explains that the truth of a theory at one level “does not *necessarily* jeopardize any hypotheses at other analytical levels” (Sherman 1988, pp. 617-618, my emphasis). The key word here is ‘necessarily,’ which implies that it is indeed possible for noncompeting theories at different levels to conflict. The distinction between ‘conflict’ and ‘compete’ is at the crux of the matter. Even if it is true that theories at different levels can conflict in the way that Mitchell describes, it is not clear that such conflict entails that they are competing theories. ‘Competition’ implies that the theories share the same explanatory goals, which I take to be part of what is captured by distinguishing kinds of questions at different levels of analysis.

An ontogenetic theory is aimed at explaining ‘how’ a trait develops in an individual organism and an evolutionary theory is aimed at explaining ‘why’ a trait develops in a particular species. Given their disparate goals, they are not in competition. Nonetheless, if their empirical content contains contradictions, they may be noncompeting but mutually exclusive theories. This must be what Sherman means when he says that a theory at one level might ‘jeopardize’ theories at other levels. If Sherman held that theories at different levels cannot ‘conflict,’ then Mitchell’s criticism would have been right on target. But while lack of explanatory closure guarantees that *conflict* is possible, it does *not* guarantee that *competition* is possible (another necessary condition for competition is shared explanatory goals). Mitchell’s criticism thereby misses the mark, as it is consistent with Sherman’s view.

Now consider Mitchell's second claim that, contrary to Sherman, theories within a level *with different focuses* cannot conflict with one another. Mitchell correctly says that on Sherman's view "competition among explanations rightly occurs within a level" (Mitchell 2002., p. 63). However, she goes on to say, "[t]hus we would expect that the three self-organizational models [of division of labor in social insects], all located at the ontogenetic level, would be mutually exclusive alternatives" (*ibid*.). This step is not warranted; nowhere does Sherman say that any two theories at the same level *must* compete (or conflict). There are obvious reasons to think that inhabiting the same level of analysis is not *sufficient* for competition among theories. Consider scientific theories at the ontogenetic level. These theories answer questions about how traits in an individual organism develop within its lifespan, but clearly not *all* theories at this level are in competition. It would be odd to think, say, that a theory about the embryonic development of a bat's wings and a theory about the development of a lion's mane are in competition. Although they are at the same level of analysis no one would be surprised if they did not conflict in any way. Certainly Sherman would not insist that they are competitors.

I find it exceedingly unlikely that Mitchell would attribute such an implausible view to Sherman. Instead, Mitchell must think that on Sherman's view any two theories that *answer the same question* within the same level of analysis are competitors. Indeed, in her example about social insects all three theories are at the ontogenetic level *and pose answers to the same question*, though with different focuses: "What explains the self-organization of division of labor in social insects?" However, it is not clear that these theories answer the same question in the sense that is relevant for Sherman. This can be demonstrated by rephrasing issues of focus in terms of the questions being asked. In the case of division of labor in social insects, the questions might be formulated as follows: "What is the role of individual learning in the self-organization of division of labor in social insects?" "What is the role of genetic variation…?" and "What is the role of nest architecture…?" Scientific questions can be classified by focus – what we might call 'causal levels' – just as they can be classified by Sherman's analytical levels. Consequently, I believe Sherman can coherently maintain that Mitchell's examples are not competitors even though they occur within the same level, since there is an important sense in which they answer distinct questions (or have distinct explanatory goals).

Turning the Tables on Mitchell's Criticism

On careful inspection, Mitchell's arguments leave Sherman's view intact. Nonetheless her critique involves some important insights. I think she is clearly right that conflict can occur between theories at different levels. Although theories at different levels are about different things – answering distinct questions – they may lack explanatory closure. Theories that are not explanatorily closed have overlapping explanations, which means it is possible for them to conflict.

The inverse of this claim also seems plausible: if theories are explanatorily closed (i.e., their explanations do not overlap), then it is impossible for them to conflict. Indeed, Mitchell appears to have this in mind when she points out that theories focusing on different causal features "abstract away the operation of other compounding factors, and hence make no incompatible claim about their operation" (Mitchell 2003, p. 205-206). She explains that "[c]ontrary to what one might expect on the levels of analysis picture, the three self-organization hypotheses although located at the same 'level' of analysis do not directly compete, since they describe only what would happen in *non-overlapping ideal worlds*" (Mitchell 2002, p. 64, my emphasis). Thus, while explanations *for concrete phenomena* cannot guarantee explanatory closure because of the complexity involved in real world contexts, ideal models (that vary in focus) can guarantee explanatory closure by referring to "non-overlapping ideal worlds."

A possible problem with this line of reasoning is that while some ideal models may indeed exhibit explanatory closure, it is not clear that this can be established *a priori* for *all* ideal models. If some ideal models lack explanatory closure, then the most Mitchell can claim is that such models do not *necessarily* conflict (in contrast to her claim that they *cannot* conflict). This may be her best option since there are reasons to question whether explanatory closure is a feature of all ideal models. In *Scientific Pluralism* (2006), Kellert, Longino, and Waters conceive of how it is possible for ideal models to be inconsistent:

> In many complicated situations, investigation is not feasible unless investigators parse causes. […] Some parsings are advantageous for explaining (and/or investigating) some aspects of the situation, other parsings are advantageous for accounting for other aspects. In such cases, we could say that each account emphasizes some causal aspects of the situation while obscuring others. In fact, an acceptable scientific model might describe some facets of the situation extremely well (e.g., the potential causal influence of some factors) while actually distorting other facets (the potential causal influence of other factors). If this is the case, and if two models distort some of the same aspects, they might distort these aspects in different ways, giving rise to inconsistencies. This is just one kind of situation in which a plurality of inconsistent approaches might be defended. [Kellert, Longino, and Waters 2006, loc. 129]

If this scenario is possible, then while Mitchell may be correct that her particular examples exhibit explanatory closure, this does not seem to be a foregone conclusion for all ideal models that differ in focus.

Notwithstanding the issues of explanatory closure, I think Mitchell's view is basically correct. In fact, were she to accept it, the conflict/competition distinction may work to her advantage. Given this distinction, even if some idealized models (that differ in focus) do somehow *conflict*, Mitchell can consistently maintain that they do not *compete* since they do not share explanatory goals. The advantage of this view is that it supports investigating a plurality of theories all answering the same question, but with different focuses. Such investigations might reveal conflict, but neither conflict nor competition is a *necessary* result of pursuing multiple theories at the same level about the same phenomena (e.g., "What explains the self-organization of division of labor in social insects?"). This is all that is required for Integrative Pluralism to get off the ground.

Are Levels of Analysis and Integrative Pluralism Compatible?

I believe Mitchell and Sherman's views are compatible once the conflict/competition distinction is taken into account. They can agree that theories do not necessarily conflict (regardless the levels they inhabit) and that conflict is possible between any two theories whose explanations overlap. Thus, their views about *conflict* are consistent. But do they agree about *competition* among theories?

In her characterization of Sherman's view, Mitchell explains that "[s]ince the answers [at different levels] are not directed at the same target, they are not competing alternatives but, rather, compatible components of a multidimensional body of biological knowledge" (Mitchell 2003, p. 200). In the context of this passage, it is clear Mitchell thinks Sherman's approach has merit. In particular, she agrees that identifying distinct questions can help clarify whether theories share explanatory goals. She notes that the relevant details are sometimes implicit, but can be spelled out (e.g., by identifying the intended contrast class of a given question)[19] (Mitchell 2003, p. 201). But Mitchell goes on to assert that what scientists need is "a classification of answers, as well as a classification of questions," where 'answers' are understood as "appeals to distinct abstract models of causal processes and the specifications for application to a given case" (Mitchell 2003, p. 202). While it is clear enough that she is referring to theories differing in focus, the claim that scientists should be concerned with classifying 'answers' and not just 'questions' is a bit puzzling.

At this point I suspect the metaphor of 'answers' and 'questions' has outlived its usefulness. That is, I am not sure it matters whether we think of Mitchell and Sherman's views as classifications of 'answers' or 'questions.' The different levels of analysis are classifications of theories according to their explanatory goals. Differences in focus can be understood likewise. Sherman's approach takes goals in biology (e.g., explain trait A) and subdivides them according to certain explanatory goals of interest (e.g., explain trait A's development; explain trait A's evolution; etc.). Mitchell's examples take the circumscribed goal of one level and further subdivides it according to even more precise explanatory goals (e.g., explain the causal role of X in trait A's

[19] The idea is that the questions "Why A (rather than B)?" and "Why A (rather than C)?" can have distinct and compatible answers. For example, consider the question, "Why did Nicole tend the garden on Monday?" The appropriate answer is dictated by the contrast class; i.e., it depends on whether the intended question was, "Why did Nicole tend the garden on Monday, *rather than doing something else*?" or "*...rather than on Sunday*?" If the former, the answer might be that the tomatoes were ripe and needed to be picked; if the latter, it might be that it was raining on Sunday. What matters is that the answers need not be mutually exclusive. Once the contrast classes are distinguished they can be seen as compatible answers to distinct questions.

development; explain the causal role of Y in trait A's development; etc.). The specification of goals is important, since shared explanatory goals are necessary for competition (but not conflict!) between theories. If it is established that two theories have significantly different explanatory goals, whether in terms of *level* or *focus*, that is sufficient to show that they are not in competition. Thus, once we shed the question/answer metaphor, I think it is clear that Mitchell and Sherman agree about competition among theories.

So far there are no incompatibilities between Mitchell and Sherman's views, but there is one last complication. Mitchell believes that conflict among theories must also be considered in the context of theoretical *integration*. She emphasizes that an integrated account must be consistent, since "there is only one causal history that, in fact, has generated the phenomenon to be explained" (Mitchell 2003, p. 216). Sherman says nothing to contradict this claim and, as mentioned previously, he is aware that the different levels of analysis are not autonomous. While he does not address the issue directly, there is nothing in Sherman's view that precludes him from adopting Mitchell's approach to integration.

Interestingly, Tinbergen, whose work is the foundation for Sherman's view, shows some strong affinities with Integrative Pluralism. For example, Tinbergen explains that overarching questions in biology (e.g., "What explains trait A?") require "a complete answer in which [the] partial answers are contained" (Tinbergen 1963, p. 427). Similarly, in another passage he explains that "[…] it is useful both to distinguish between [levels] and to insist that a comprehensive, coherent science […] has to give equal attention to each of them and to their integration" (Tinbergen 1963, p. 411). In fact, in a recent retrospective on Tinbergen's work, Bateson and Laland conclude that scientists have generally overlooked Tinbergen's call for integration (Bateson and Laland 2013, p. 717). However, given that his view relies heavily upon Tinbergen's work, I do not think Sherman is guilty of this oversight. The fact that he does not directly address integration is probably explained by the fact that his primary concern is to resolve and prevent *disputes* among scientists; perhaps the goal of motivating collaboration seemed too lofty or unrealistic.

While Mitchell and Sherman's views have different emphases, the conflict/competition distinction helps reveal that they are consistent, despite Mitchell's criticisms. Given that their views are compatible, I believe they should be seen as complementary approaches under the umbrella of theoretical pluralism. With an understanding of theoretical pluralism in hand, we can now investigate its relation to Dialectical Empiricism.

Dialectical Empiricism and Theoretical Pluralism

I maintain that both theoretical monism and theoretical pluralism are compatible with Dialectical Empiricism since they are consistent with values playing their appropriate roles at the different logical 'moments' of scientific activities. Recall that theoretical monism is compatible with the moment of theory appraisal; i.e., it is consistent with the cognitive value of external consistency and the thesis of impartiality. It is less obvious how theoretical monism would fit with strategic pluralism at the moment of strategy adoption, but, as Longino aptly puts it, "a characteristic of a process need not be a characteristic of its product" (Longino 2002, p. 140). It is possible that a plurality of strategies could give rise to a unified theory.

In contrast, theoretical pluralism fits easily with strategic pluralism in the moment of strategy adoption, but potentially runs into trouble with the moment of theory appraisal. One point of difficulty is that theoretical pluralism appears to jettison the cognitive value of external consistency as a criterion for theory acceptance. For example, according to Integrative Pluralism it is permissible for models to be *incompatible* at the theoretical level by focusing on different causal factors. However, Mitchell is explicit that in application to particular cases an integrative account must not contain inconsistencies, so she supports a version of external consistency. Although external consistency is a cognitive value (on at least some accounts of DE), its interpretation is not settled *a priori*; an interpretation like Mitchell's is not ruled out. Similarly, in Dickson's account of quantum dynamics inconsistencies are permitted among theories involved in distinct explanatory contexts. Nonetheless, inconsistencies are not permitted within one and the same explanatory context. Thus, theoretical pluralism allows cognitive values to play their role at the moment of theory appraisal, though with varying interpretations of external consistency.

A second difficulty appears with the moment of theory appraisal given Lacey's formulation of the thesis of impartiality. He maintains that if a theory is accepted for a domain of phenomena, then *inconsistent rival theories should be rejected* (Lacey 1999, p. 234). There are two ways in which theoretical pluralism can accommodate this position. First, a theory is always accepted 'for a domain of phenomena,' so there is room for a plurality of theories concerning different domains. Second, it is never spelled out just what constitutes a 'rival' theory, which is open to an interpretation like Sherman or Mitchell's according to which rival/competing theories must have shared explanatory goals. Thus, it is consistent with DE that a plurality of theories can be accepted given that they are applied to different domains or pursue different explanatory goals.

III. Affinities between Dialectical Empiricism and Integrative Pluralism

In both DE and IP, complexity in the world influences how we conceive of the explanatory goals of science. As we have just seen, according to IP complex biological phenomena are not always amenable to explanation via a single theory. This complexity thereby leads scientists to identify more precise classifications of explanatory 'questions,' which a plurality of theories can address. On the other hand, according to DE, values interact with the aims of inquiry and lead scientists to investigate and explain certain phenomena (or certain aspects thereof) and not others. Thus, what we might call 'value-complexity' results in explanatory goals which are not always amenable to investigation under materialist strategies, thus making salient the possibility of alternative strategies and, hence, strategic pluralism.

To put it another way, in both DE and IP, complexity (among biological phenomena and values) exposes limitations on what can be explained via scientific inquiry. These are limits of strategies (for DE) and theories (for IP), and pluralism enables us to overcome these limitations. In the case of DE, strategic pluralism arises because *not all aspects of the world can be investigated (and not all interests can be equally well-served) by a single methodology* (Lacey 1999, pp. 96-100). For example, materialist strategies do not have the tools to investigate the social and ecological aspects of seeds for crops, thus revealing the need for other approaches such as agroecological strategies. On the other hand, theoretical pluralism arises because *not all* *aspects of the world can be represented adequately by a single theory*. Mitchell's example of the division of labor in social insects illustrates this point well.

Neither DE nor IP takes these limitations to be *necessary* features of scientific theories and methodologies; they view explanatory limitations pragmatically. It is conceivable that such limitations eventually can be overcome via monistic approaches. Thus, both DE and IP admit that it may strictly be *possible* to devise something like an *all-encompassing* strategy or a *unified* theory (respectively), but they also recognize that these are not currently within our grasp (and perhaps are not even on the horizon). If we want to overcome these limitations in the near (or at least, foreseeable) future, then we should pursue pluralism.

So, Lacey and Mitchell both express serious doubts that the monistic ideals can be realized (the *all-encompassing* strategy or *unified* theory), but they do not preclude the possibility entirely. In this vein Lacey discusses the question whether materialist strategies may be able to produce acceptable theories pertaining to 'intentional phenomena.' As discussed in Chapter 2, *feminist* strategies show potential in this regard. But materialist strategies run into trouble, for example, because intentional phenomena (e.g., involving human agency) are not easily explained in terms of causal laws. Lacey remarks that while attempts have been made to formulate theories developed under materialist strategies that are applicable to the domain of human agency, the hope that such theories will eventually provide adequate explanations is "simply an anticipation with slender empirical warrant" (Lacey 2005, p. 71). Yet, he does not think it is out of the question; in the same passage he admits that "[t]he conjecture that adequate understanding of human agency can be produced under [materialist strategies] is worth exploring and a priori I do not rule out its eventual confirmation" (*ibid*.).

Mitchell expresses a similar sentiment regarding the possibility of unification among theories concerning complex biological phenomena:

> There is no prima facie reason to reject [the aim of unification]. Indeed, theoretical consistency among various causal models is certainly a scientific virtue. However, to date no grand theoretical unification has developed. […] at least at present, there is no strong indication that a unified theory is in the wings [Mitchell 2003, p. 207]

What this reveals is that Lacey and Mitchell's views have a shared and distinctive *pragmatic* motivation. To wit, the pluralist features of their views enable us to grapple with the limitations exposed by complexity – limitations we cannot *currently* overcome via monistic approaches (even though they might, in principle, be sufficient).

This line of reasoning may sound reminiscent of Chapter 3, in which I pointed out the possibility of a theory being endorsed, but not accepted, on the ground of expedience. In such cases we can settle for lower quality theories – e.g., by permitting lower standards of testing – because we need information on which to act sooner rather than later. However, this is *not* an apt analogy for strategic or theoretical pluralism because in neither case is adopting the pluralist view 'settling for less' than what might be achieved on the monistic view. A plurality of strategies can contribute to s*cientific knowledge*. A plurality of theories can be *true* (or approximately true, or reliable).

Given that pluralism may allow us to overcome explanatory limitations exposed by complexity, the following outcomes are possible: (1) Strategic pluralism may enable the production of reliable and well-confirmed empirical theories that are not (currently) within the purview of materialist strategies; (2) a plurality of theories may enable us to grasp truths that are not (currently) amenable to theoretical unification. In fact, both of these outcomes have already been realized. Chapter 2 touched on the first possibility by exploring feminist strategies. This is also the topic of Chapters 5 and 6, in which I apply DE to actual scientific practices, focusing on contemporary developments in scientific approaches to agriculture and nutrition. The second possibility above has been demonstrated in this chapter, using examples within the discipline of biology (e.g., the division of labor in social insects).

Conclusion

If they are indeed possible, the discovery of an *all-encompassing strategy* or a *grand unified theory* would be magnificent – it would unify our understanding of a domain of inquiry. But until that day comes, an approach that combines DE and theoretical pluralism may provide the most comprehensive approach to scientific knowledge within our grasp. Thus, while DE is consistent with theoretical monism, a version of DE that incorporates IP bears further investigation, since it may provide unique tools to overcome present and persistent explanatory limitations in science.

Part II: Application

Value-freedom is supposed to be the hallmark of modern science. It is considered essential to scientific objectivity. As such, it is deeply enshrined in the scientific community's self-conception. Accusations that a scientist is not being objective or that she is being influenced by values are not just criticisms, they are insults. When values impact one's research, this is not seen as a mistake on par with a mathematical miscalculation, it is perceived as a vice or character flaw. This is because value-freedom is at the core of the scientific identity, so much so that 'value-laden science' probably has the ring of an oxymoron to most people. This needs to change.

The scientific community and the societies in which it is embedded need to be disabused of the notion that value-freedom is essential to the scientific identity. The value-free ideal for science is problematic both philosophically and socially. *Philosophically* the idea of science as value-free is untenable. I argue that it generates a kind of paradox because the methodology of 'value-free' science is dialectically linked with a particular set of social values (values associated with the aims and methodologies of scientific inquiry), thus leading to the contradiction that *value-free science is not value-free*. *Socially* the value-free identity of science can be disastrous.

This may sound hyperbolic, even to those sympathetic to the idea of value-laden science. To be clear, I am not claiming that the conception of science as value-free always leads to bad consequences, but that it often blinds us to very real but unrecognized value-laden assumptions, and to alternatives to those assumptions that may legitimately ground alternative scientific approaches. Feyerabend captures this sentiment well: "The appearance of objectivity that is attached to some value judgments comes from the fact that a particular tradition is used but not recognized. Absence of the impression of subjectivity is not proof of objectivity, but an oversight." The misapprehension that modern science is value-free leads to the mistaken belief that all other approaches are value-laden and therefore illegitimate. When in this vein we marginalize or overlook alternative approaches, we miss out on possibilities for gaining scientific knowledge that are inaccessible from the 'value-free' approach. When those overlooked possibilities hold the promise to significantly improve people's lives, our blindness results in both a cognitive and a moral failure.

In Chapters 5 and 6 I apply DE to two case studies in contemporary sciences – agriculture and nutrition – in order to establish a pattern that demonstrates three theses: first, that there are consistent links between the aims and methodologies of 'value-free' science and a particular set of social values; second, that there are legitimate alternative approaches linked with alternative values; third, that these alternative approaches reveal cognitively and morally significant possibilities that are occluded by or inaccessible from the myth of the 'value-free' approach.[20] In light of these claims, I conclude that we should jettison value-freedom from the scientific identity for both cognitive and moral reasons.

The Paradox of Value-Free Science

Quine (1980) distinguishes three kinds of paradoxes: veridical paradoxes, falsidical paradoxes, and antinomies of reason. Paradoxes wreak havoc on our intuitions, but some of them can ultimately be explained, while others cannot. Veridical paradoxes are those which upon analysis are confirmed to be true, even though they are

[20] Henceforth I will omit the scare quotes when discussing 'value-free' science. When I refer to "value-free science" or "value-free methods," etc., it should be understood that these are *purportedly* value-free.

initially counter-intuitive; their paradoxical nature can eventually be explained away. Falsidical paradoxes are those which turn out to be false, often involving some sort of trick or illusion that makes them seem true at first glance. Antinomies of reason are 'genuine' paradoxes which seem to defy explanation entirely. As I see it, value-free science involves two paradoxes, both of which appear to imply a contradiction: *value-free science is not value-free*.

One way the idea of value-free science generates a paradox is that value-freedom is itself a value in science. This is a *veridical* paradox because upon closer inspection we find that there is no contradiction involved in the scientific community adopting value-freedom as a central value – no more so than there is a contradiction in the claim that a classroom (with no one in it) is empty even though it contains tables and chairs. The classroom is empty because it contains none of the *relevant* things (people); likewise, science is value-free as long as it contains none of the *relevant* values. In the context of the scientific identity, 'value-freedom' means freedom from noncognitive values such as personal, religious, or social values. Thus, the contradiction is resolved. However, this resolution might be problematic since it presupposes that value-freedom is a cognitive value.

Indeed, the distinction between cognitive and noncognitive values has generated its fair share of debate (see Ch. 2). Sandra Harding, an important critic of the value-free ideal, argues that "trying to maximize cultural neutrality, as well as claiming it, expresses a culturally specific value" (Harding 1998, p. 61). In terms of my resolution of the first paradox, Harding is claiming that value-freedom is itself one of the 'relevant' values that it aims to exclude from science. In other words, she understands value-freedom as a *noncognitive* value, which itself expresses as an ideal that there should be no noncognitive values in science. Clearly, if Harding is right, value-freedom is a paradoxically self-defeating value.[21] However, even if we were to grant that value-freedom *is* a cognitive value, it turns out that value-free science is *still* not free of the relevant noncognitive values. Consequently, the arguments I present in Part II cannot be bypassed merely by rejecting Harding's view and maintaining that value-freedom is a legitimate cognitive value. Thus, we turn to the second paradox of value-free science.

While the first paradox involves a conceptual contradiction (value-freedom is a value in science), the second paradox involves a manifest contradiction: in practice, value-free science is not value-free. I am *not* claiming merely that some instances of science that purport to be value-free are actually influenced by values – no one denies this. The paradox is that even sciences that are usually taken to be the epitome of value-freedom cannot be value-free. I demonstrate this with case studies that exemplify a central insight of Dialectical Empiricism: the methodologies of value-free science, even at its best, are "dialectically linked" with a set of social values, which I will refer to as the *values of modernity* (see Lacey 1999, Ch. 6). These values include the ability to exert control over nature, to promote technological progress, and to produce marketable commodities.[22] By claiming that the methodology of value-free science is dialectically linked with these values, I mean that they stand in "*mutually reinforcing relations*" with each other (Lacey 1999, p. 109; Lacey 2005, pp. 23-28). The following case studies

[21] See Chapter 1 for an account of how this issue is handled within the framework of Dialectical Empiricism. In short, the thesis of impartiality, which is the sole aspect of the value-free ideal retained in Dialectical Empiricism, does not exclude itself as an ideal for science.

[22] The term 'modernity' as I am using it here is more generally associated with the social and scientific values that emerged in Europe in the 16th and 17th centuries, and the resulting emergence and development of industrialization, urbanization, individualism, and political democracy. It is associated with the scientific values of rationality, objectivity, and the ability of the experimental method to uncover and evidentially support true hypotheses and theories about the nature of world.

will reveal that the knowledge acquired via value-free science is especially suited to inform projects that manifest the values of modernity, and the greater manifestation of these values in turn supports research utilizing the methodologies of value-free science (Lacey 1999. pp. 236-247). This may sound a bit vague, but before we dive into specific examples and case studies I need to expand on what I mean by "the methodologies of value-free science."

Decontextualizing Approaches to Scientific Inquiry

The goal of value-free science is to investigate phenomena in ways that ensure values do not improperly influence scientific inquiry. One way this might be achieved is by constraining the theories (or hypotheses) that are candidates for investigation to those which concern only the *material* properties and possible states of things. Consequently, value-free methodologies aim to identify possibilities of phenomena strictly in terms of the "underlying structure, process, and law" (Lacey 1999, p. 68). On this approach, phenomena are abstracted away from their social (and other value-laden) contexts and typically are studied in a meticulously controlled laboratory setting. When phenomena are *decontextualized* in this way, they are "stripped of all links with value" (Lacey 1999, p. 68; see also, Lacey 2014, p. 5 and Shiva 1988). After all, a brick of gold may have social and economic value in the context of the global market, but this value is not to be found in its molecular structure or in the laws governing the motion of its subatomic particles. Decontextualizing methodological approaches appear safely cloistered from the realm of values, and they are therefore the ideal choice for value-free science.

Now let us reconsider the claim that value-free methodologies – *decontextualizing* methodological approaches[23] – stand in mutually reinforcing relations with the values of modernity. Techno-scientific innovations represent the pinnacle of humankind's ability to exert increasingly sophisticated control over nature, and these technologies are the outcome of value-free sciences, such as fundamental physics. Thus, *value-free science leads to the manifestation of the value of control over nature* via its technological products. Furthermore, among these technologies are the increasingly complex experimental apparatuses (e.g., electron microscopes and particle accelerators) necessary for the progress of modern science. The research, development, and building of such devices are so expensive as to require government funding. Governments do not spend the money required to develop and build such equipment merely for the sake of intellectual curiosity. They expect such investments to produce technological payoffs, almost always in the form of control of some aspect of nature. Thus, *manifestations of the value of control over nature support the advancement of value-free science*. This is perhaps the most obvious and simplistic (but by no means trivial) example of the way value-free science is dialectically linked with social values.[24]

I hope the second paradox of value-free science is beginning to become clear. In the case studies that follow, I highlight systematic links between value-free science (even at its best) and the values of modernity. Once this pattern is established, there is no denying that value-free science is not really value-free after all. The second paradox

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[23] I am adopting this terminology from Shiva (1988; 1991, p. 68; 1993, p. 137) and Lacey (2014). In Chapter 6 I will expand on the reasons for this choice of terminology, particularly in contrast to the more widely used idiom of 'reductionist' or 'reductive' scientific methodology.

[24] See Harding (1998, p. 11) for a similar account of the connections between value-free methodologies and the manifestation of technological values.

is *falsidical;* it is unlike the first (veridical) paradox, whose contradiction can be resolved conceptually. The manifest contradiction, that in practice value-free science is not value-free, can only lead to the conclusion that *there is no such thing as value-free science*. This does not mean that we should never pursue science via decontextualizing methodologies. Instead, we need to recognize that even these approaches are value-laden and, as I adduce in the case studies, there are other possible methodological approaches linked with other values. And, crucially, we need to realize that these alternative approaches can guide legitimate scientific practices that can generate scientific knowledge and lead to beneficial applications of that knowledge that would otherwise be overlooked.

The analyses of the case studies that follow present some terminological challenges. First, even when every instance of the term 'value-free' is assumed only to be *purportedly* so, this term is a distracting misnomer. The alternative approaches under discussion are not alternatives in virtue of being value-laden, they are alternatives because their methodologies are dialectically linked with values which are *different from the values of modernity*. Second, while 'decontextualizing' methodologies tend to abstract away from messy real-world contexts, their distinguishing feature is that they avoid explicitly value-laden contexts, which supports the illusion of value-freedom. It turns out there are decontextualizing methodologies (so understood) that are, in a more ordinary sense, context-sensitive (this is explained in Ch. 6, §I). In order to avoid confusion, from this point on I will refer to sciences dialectically linked with the values of modernity as *value-blind* sciences (approaches, methodologies), instead of referring to them as 'value-free' or 'decontextualizing.' I believe this nomenclature is fitting, since the problem with modern science is *not* that it is value-free or value-laden, but that it is *blind to its own links with values*.

Chapter 5: Values in Agricultural Science

The analysis in this chapter broadly follows Lacey's application of his version of Dialectical Empiricism to agricultural science in his book, *Values and Objectivity in Science* (2005). My aim is to explicate some of the particular methodologies (not discussed by Lacey) that are used in the sciences of biotechnology and agroecology, and to draw out some of the most prominent links between those methodologies and their associated social values. Subsequently, in Chapter 6, I extend the analysis to the discipline of nutrition, which is interconnected with the issues arising in this section (but which has not been addressed by Lacey).

I. Biotechnology in Agriculture: The Green Revolution and Transgenics

The first case study concerns the value-blind[25] sciences of agriculture; specifically, biotechnology in the Green Revolution and the science of transgenics. Beginning in the 1940's, the Green Revolution involved a series of initiatives aimed at relieving hunger (and, generally, at promoting 'development') via the transfer of modern agricultural science and technology to 'third world' countries. By the late 1960's the Green Revolution succeeded in increasing worldwide crop production by utilizing modern techniques, such as the farming of monoculture crops, and biotechnologies, such as chemical fertilizers and the hybrid 'miracle' seeds developed by Norman Borlaug. Borlaug's hybrid seeds significantly increased yields (under ideal conditions) compared to conventional seeds; by some estimates, many regions targeted

[25] Again, I emphasize that they are 'value-blind' in virtue of failing to recognize their links with the values of modernity.

by the Green Revolution tripled food production in the course of about 30 years (Lipton 1989, p. 1).

First developed in the 1980's, transgenic seeds are the result of genetic engineering whereby genes from different organisms are inserted into seeds so that the resulting plants will express certain desirable traits. For instance, 'golden rice' was developed by inserting genetic material from daffodils (among other things) into the genome of rice seeds so that the rice crop would contain increased levels of beta-carotene (a precursor to vitamin A). Other crops have been modified to produce pesticides (e.g., Bacillus thuringiensis, or Bt) or to be resistant to herbicides (e.g., glyphosate). Transgenics already make up more than 10% of the world's crops, and their usage is steadily increasing (James 2013). Genetically modified crops may prove to be a key piece of the solution for meeting the world's nutritional needs; indeed, it is the natural continuation of the aims and practices of the Green Revolution. Golden rice could help alleviate malnutrition, especially for children in poor regions where vitamin A deficiency is widespread; and pest or herbicide resistant crops could dramatically increase crop yields in order to feed the ever-increasing global population.

Clearly these biotechnological research programs are aimed directly at the realization of certain social values (e.g., human well-being). Nonetheless, the science of biotechnology is regarded as value-free, under the assumption that there is a sharp distinction between scientific research and application. The idea is that scientists merely produce knowledge and technology, which is then handed over to society to be applied. The outcomes of research are supposed to be neutral, and so able to inform projects consistent with *any* set of social values (Lacey 1999, pp. 74-82). The standard line is that *(value-free) science establishes the facts, which can then be used for good or ill*. This is an important aspect of the conception of science as value-free that, I argue, needs to be rejected.

I do not deny that knowledge gained through the study of transgenics can be incredibly useful and beneficial in a variety of contexts; but I maintain that it is *not equally well-suited* to serve projects informed by *any* set of social values (Lacey 1999. pp. 236-247). This is because sciences pursued via value-blind methodologies, which includes transgenics, are dialectically linked with the values of modernity. Specifically, transgenics are especially well-suited for (and supported by) projects that aim to manifest the values of control over nature, technological progress, and the market economy. Furthermore, transgenics are *not* well-suited for projects that aim to manifest values which conflict with the values of modernity (Lacey 2005, Part II).

The Values of Modernity: Control Over Nature

One might surmise that virtually all science is concerned to some extent with the ability to exert control over nature, and I certainly agree. Scientific knowledge is power. The same is true to varying degrees about the values of technological progress and the market. However, the values of modernity form a coherent whole, what Lacey calls a 'value-outlook' (Lacey 1999, Ch. 2), a distinguishing feature of which is that its constitutive values are generally treated as *lexically prior* ("not subordinated") to any other social values (Lacey 1999, p. 113). This leaves open the possibility for alternative value-outlooks which still rank control over nature as an important value, but subordinate it to other social values.[26]

[26] I present alternative value-outlooks and methodological approaches at the end of each case study.

Biotechnology – transgenics in particular – may be unparalleled when it comes to the ability to exert control over nature. It is incontestable that research in transgenics manifests this value to a high degree, e.g., in experiments involving genetic modification. Similarly, the proposed applications of transgenics (like golden rice) are clearly aimed at exerting sophisticated control over a range of natural objects and processes. Furthermore, we see in the research and application of transgenics that the values of modernity are not subordinated to other values, such as *environmental integrity*. There is a consistent pattern whereby environmental harms are downplayed, considered 'side-effects,' and remedied (when possible) in an *ad hoc* manner (Lacey 2005, p. 187; Hecht 1995, p. 7). Vandana Shiva argues that this mindset, which is characteristic of biotechnological approaches, systematically "offers technological fixes to complex problems, and by ignoring the complexity, generates new ecological problems which are later defined away as 'unanticipated side effects' and 'negative externalities'" (Shiva 1993, p. 109). We will see in the case of nutritional science that this kind of tunnel-vision is not unique to transgenics or biotechnology; it is a result of the values of modernity and the value-free scientific identity.

The Values of Modernity: Technological Progress

The values of control over nature and technological progress are deeply interrelated, as is evident in the example of fundamental physics and its high-tech experimental apparatuses. But there are subtler ways in which these values are related. The values of control over nature and technological progress help to frame the way value-blind sciences perceive the problems they aim to resolve: *problems are presumed to have technological solutions* (Lacey 2005, Ch. 9.2; Shiva 1991, p. 98). The invention of golden rice is evidence of this mindset; even malnutrition has a technological solution.[27] Related to this, approaches which attempt to resolve problems without a central place for technology are often labeled as unscientific or 'anti-science' (e.g., Borlaug 2000).

The values of technological progress reinforce the characterization of risks merely as potential 'side-effects,' since any problems that arise are assumed tractable via further technological innovation. But there are important questions concerning risk that should be asked throughout scientific inquiry, including questions concerning choice of methodology. In particular, scientists should investigate whether there might be alternative methodologies that could achieve the same aims (e.g., feeding the world's population) while posing fewer or less significant risks (Lacey 2005, Ch. 9). One effect of the values of modernity, in concert with the value-free scientific identity, is to blind us to the very possibility of alternative, potentially less risky approaches. Transgenics, in virtue of its technological sophistication, is assumed to be the best option; this assumption is reflected both in the dearth of research on the risks involved[28] and in the absence of (and resistance to) the investigation of alternative approaches. This theme will be highlighted below, both in the exposition of the risks and consequences of transgenics and the presentation of an alternative approach, agroecology.

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[27] Perhaps nothing better illustrates this point than the fact that advocates of biotechnology are calling for a second (or third) green revolution – the transgenic revolution – to solve the problems created by the (first) Green Revolution. When I address the aims of biotechnological approaches to agriculture, I explain how this pattern has played out in the case of 'golden rice.'

[28] In 2004, less than 4 percent of biotechnology research budgets were dedicated to risk assessment (Altieri 2004, p. 53).

The Values of Modernity: The Global Market

The values of the market encompass neoliberal ideals of globalization and development, privatization, and capitalism. For instance, we see that value-blind science is well-suited to the production of marketable products, as evinced in the technological innovations that increasingly pervade our daily lives. But the values of the market also shape how we perceive the objects of scientific inquiry. For example, transgenic seeds have become patented and protected by intellectual property rights, reflecting our perception of them as commodities as opposed to, e.g., integral parts of ecosystems or farming communities.

Regarding this point, there is a predictable objection that might go as follows: science is not at fault for the social and legal implications for transgenic seeds; after all, the commoditization of seeds is attributable to the actions of corporate lawyers, judges, and global organizations, *not* scientists. However, the point is not that scientists are producing technologies like transgenic seeds for the express purpose of creating marketable products (although this is sometimes the case), but that value-blind methodologies are linked with the values of modernity. Susanna Hecht, a leading researcher in sustainable agriculture, explains that the methodology used in conventional agriculture "tends to atomize research questions" and that this one-dimensional approach is reinforced by "an agricultural commodity focus" (Hecht 1995, p. 7). The reason for this is that the value-blind methodology, which *decontextualizes* the objects of inquiry, encourages us to think of seeds in isolation, abstracted from their local conditions, e.g., their ecological, social, and cultural contexts. In this value-blind conceptualization of seeds, there is *prima facie* no reason against treating them as commodities. But seeds are not merely objects with certain biological properties; they are also integral constituents of ecosystems and they serve as the livelihoods of farmers and farming communities (Lacey 2005, pp. 150-153). The social and ecological dimensions of seeds cast doubt on the legitimacy of treating them as commodities. The fact that this is overlooked from within the biotechnological approach shows that the value-blind methodology, dialectically linked with the values of the market, influences how we perceive and (therefore) treat the objects and products of scientific inquiry.

In the remainder of this section I analyze some of the risks and problems generated by biotechnological approaches to agriculture. The purpose is *not* to show that biotechnology, or value-blind methodologies in general, are necessarily bad; instead, the goal is to demonstrate that there is room for better alternative approaches. At the same time, I will continue to establish links between the value-blind approach and the values of modernity.

Risks and Consequences of Biotechnology: the Green Revolution

The Green Revolution undeniably increased food production and saved many lives, but it was not an unqualified success; it led to environmental and social harms, generating criticism even from the people it was designed to help. Hecht summarizes some of the harms that have been well established:

> First, [the Green Revolution] focused its benefits on the already resource-rich farmers, accelerating the differentiation between them and other rural inhabitants, so rural inequality often increased. Second, it undermined many forms of access to land and resources such as share cropping, labor tenancies, and access to water supplies and grazing lands. This reduced the diversity of subsistence strategies available to rural households and thus increased their dependence on the agricultural plot. With the narrowing of the genetic basis of agriculture, risk increased because crops became more vulnerable to pest or disease outbreaks and the vagaries of climate. In irrigated rice, the secondary

> pollution generated by the increased use of pesticides and herbicides often undermined the important source of local protein: fish. [Hecht 1995, p. 17]

It is important to see that these consequences were connected with the decontextualizing methodology of value-blind science. For example, in their analysis of agricultural methodologies, scientists Richard Norgaard and Thomas Sikor point out that many problems of the Green Revolution resulted from the assumption that "farming can be understood atomistically" leading to the development of "separate technologies for plant nutrition and insect control" (Norgaard and Sikor 1995, pp. 21-22). The problem with these technologies was that, having been developed exclusively under ideal conditions in a laboratory setting, they were not well-suited to the diverse social and ecological contexts of actual farms. The combination of "separately and individually derived technologies," were therefore significant contributing factors to the various social and ecological harms caused by the Green Revolution (*ibid.*).

In her book, *Monocultures of the Mind* (1993), Vandana Shiva offers a sharp critique of the Green Revolution that further reveals the influence of the values of modernity. Shiva condemns the farming of input-intensive monoculture crops in India for depleting water resources and soil nutrients to the detriment of the surrounding ecosystem and native biodiversity. She maintains that the Green Revolution was characterized by a mindset according to which there were no better alternative farming practices that could result in sufficiently high crop yields to meet people's needs. However, the view that there were no better alternative approaches relied heavily on certain concepts, especially that of 'high yielding varieties,' that were considered value-free when, in fact, they were permeated by the values of modernity (Shiva 1991, p. 68; Shiva 1993, p. 39).

Shiva discusses the 'miracle' crop eucalyptus, which she admits is indeed 'high yielding' with respect to the woody pulp that is a valuable commodity in the paper industry. However, she argues that this concept obscures the costs associated with eucalyptus, as it requires significant inputs (e.g., water and pesticides) to be productive and generates little ecologically valuable output. In contrast, certain indigenous plants yield not only pulp, but food for humans and livestock, nutrients for the soil, reeds for basket-weaving (a significant source of income for local women), and support for the diversity of organisms that serve vital purposes for the local ecosystem and community (Shiva 1993, Ch. 1). Miguel Altieri, a world leader in sustainable agriculture research, affirms Shiva's point:

> Traditional farmers are much more innovative than many agriculturalists believe. In fact, most productivity comparisons between Green Revolution and traditional agricultural systems have been biased and unfair, as they ignore the fact that traditional farmers value total farming system production and not just yields of one commodity as it is the case in a Green Revolution system. [Altieri 1995, p. 107]

Thus, the idea of a 'high yielding variety' is value-laden, as it focuses on only one dimension of plant production while excluding the rest. Arguably indigenous plants produce a higher yield than eucalyptus, when 'yield' is understood to include *all* the ecologically and socially valuable resources crops may produce.

What we see in Shiva's study of eucalyptus is the manifestation of the values of modernity, reinforced by a value-blind approach to agriculture. By abstracting the object of inquiry (e.g., eucalyptus) from its ecological and social contexts, the value-blind methodology supports the one-dimensional category of 'high yielding varieties,' which is linked with the values of the market. In §II we will see how agroecology, an alternative approach associated with a different set of values, has generated important insights into other agricultural concepts – insights which were systematically overlooked by the Green Revolution in virtue of its links with the values of modernity.

Risks and Consequences of Biotechnology: Transgenics

Turning to the issue of transgenics, it is clear that genetically modified crops will carry many of the same risks and benefits as the monocultures used in the Green Revolution in addition to novel environmental and health risks associated with genetic engineering. For example, there are risks involving the unintended transfer of the modified genetic material to unrelated organisms (Altieri 2004, p. 26), as well as concerns about the long-term impact of genetically modified foods on human health (Altieri 2004, pp. 29-33). These risks have generated highly publicized controversy. Many scientists maintain that genetically modified foods do not pose serious health risks, but their position has been challenged on the basis that there have been very few risk assessments conducted. Thus, the values associated with high crop yield (and the more general awesomeness of technology) have tended to lead researchers to downplay issues involving risk – issues which themselves should be assessed via properly tested hypotheses about risk, rather than by merely brushing aside such worries (Lacey 2005, Ch. 9.4).

Risk assessments are especially important when there is a lot at stake. Indeed, the risks involved with transgenics are sufficiently serious that many European countries ban or restrict their sale. If that is not telling enough, countries including Zambia, Malawi, Mozambique, and Zimbabwe have placed restrictions upon, or entirely refused, transgenics sent as food aid (Altieri 2004, pp. 56-57). Nonetheless, transgenics are given the presumption of safety, generally paired with the claim that there are *no better alternatives* when it comes to feeding the world's ever growing population. The values of control and technological progress reinforce the idea that risks are merely 'side-effects' that will be tractable via further technological innovation, should they arise.

Transgenics also continue the trend of increasing the manifestation of market values and the commoditization of seeds by granting them patent protection and associating them with intellectual property rights, which leads to various harms and benefits. Before addressing these consequences, it is worth pointing out another way in which the values of modernity are interrelated. Transgenic seeds are granted global legal protection and considered as the private property of agribusiness corporations because (to put it simply) these firms invested resources and capital in the development of these new technologies and could not generate profit (and so motivate further market innovations) without owning and selling the seeds. However, the very same seeds that are modified to produce this 'technology' are themselves the outcome of decades (or more) of careful and systematic seed selection and planting by local farmers.

Shiva argues that value-blind science is able to ignore this issue by "creating a framework that treats self-regenerative seeds as 'primitive' and as 'raw' germplasm" (Shiva 1993, p. 144); in other words, the value of technological progress creates a convenient blind-spot for those with interests in biotechnology. As a result, farmers, even though they have labored and modified these seeds for centuries, do not own the varieties they produce or generate profits from their sales.[29] Seeds, it seems, are converted into 'technology' and become private property in the hands of scientists and

[29] The term associated with the appropriation of farmers' seeds by agribusiness is "biopiracy" (Lacey 2005, p. 151; see also Altieri 1995, pp. 367-368).

not in the hands of farmers. If transgenic seeds were seen as biological objects, rather than as technological innovations, then it would be absurd to claim them as private property (it would be akin to claiming ownership over an entire species of algae or elephants). Thus, we see the values of technological progress and the market intertwined in value-blind science.

Setting aside the dubious justification for granting patents for seeds, let us turn to the consequences. The potential benefit of the commoditization of transgenic seeds is that their market value drives innovation, leading to better and more sophisticated technologies that could, in the best case scenario, *end world hunger*. No doubt the aims of transgenics are laudable, and their realization might justify some concomitant harm (again, assuming there are no better alternatives!). At the end of this section I will assess how well these aims have been met by biotechnological approaches to agriculture. Meanwhile, it is clear that the harm resulting from the commoditization of transgenic seeds is not merely potential.

Traditionally seeds serve as renewable resources for farmers, who could collect and save seeds after a harvest for the next year's crop. Transgenic seeds, however, must be purchased every year. In some cases these seeds are modified so that the resulting plants do not generate new seeds for collection. When patented seeds are capable of reproducing, it is illegal for farmers to collect and save them; farmers that do so are prosecuted. Thus, those who plant transgenic crops face a significant increase in overhead costs. Moreover, farmers who collect these seeds (as they have been doing for years prior) face the prospect of devastating litigation. In 2004, the penalty was about $3,000 per acre, enough to cost defendants their farms (Altieri 2004, p. 6). Given that agribusinesses dominate the seed market, these risks accrue to a huge number of farmers worldwide.

The Aims of Biotechnological Agriculture: Increasing Food Supply or Ending Hunger

The Green Revolution undeniably increased crop yields, and there is little doubt that transgenics have the potential to continue this trend. However a surplus food supply is not valuable in itself; the ultimate aim of the Green Revolution was to reduce or eliminate hunger and malnutrition in the poorest regions of the world (Borlaug 2000). Relative to this goal, and especially given the current state of the world, there are many critics who believe the Green Revolution was a *failure*.

Some of the reasons for the perceived failure of the Green Revolution were discussed in the previous sections. The destructive environmental effects of modern methods and technologies had the greatest detrimental impact on many of those it was supposed to help. Moreover, the increased food supply led to a reduction in market price, which meant that many poor farmers, who now had to spend more than ever on inputs like fertilizers and pesticides, did not see a significant increase in profits. Thus, Altieri explains, development via the Green Revolution led to “loss of food sufficiency, genetic erosion, loss of biodiversity and traditional farming knowledge, and permanence of rural poverty” (Altieri 2004, p. xiii). What was overlooked by those who saw the Green Revolution as promising an end to world hunger is that social, political, and economic forces are significant causes of hunger and malnutrition (Altieri 1995, p. 71; Altieri 2004, p. xii-xiii; Miller 2005, p. 287; Lipton 1989; Sen 1997). This is why in India, for example, during the period when the Green Revolution led to a rate of food production that outpaced population growth, “the incidence and severity of hunger hardly changed" (Lipton 1989, p. 10). Indeed, *there is enough food today to feed the entire global population, yet hundreds of millions starve* (Burlingame 2004, p. 585; Miller 2005, p. 286; Patel 2012, Ch. 1).[30]

Sometimes this point is sidelined because not only are many people hungry or malnourished, but the global population is growing *exponentially*. The food supply today appears insufficient to feed the population of tomorrow (or, say, 50 years from now, by which time the population could double!). However, positing a technology-driven increase in food production as a *solution* to this long-term problem is wrongheaded for at least two reasons. First, increased food production does not guarantee (or, given current realities, does not make it especially plausible) that fewer people will go hungry. Second, as a basic principle of population ecology, we know that an increase in the availability of food drives an increase in population. Far from being a solution, increased food production, especially in the absence of other measures, is a significant factor that leads to population increases, and thus even higher demand for food. An increase in population also corresponds to increased environmental destruction, resource consumption, disease vectors, and many other adverse effects (Hopfenberg and Pimentel 2001). Thus, it is clear that the single-minded pursuit of technological advances in food production is not a viable long-term solution for world hunger. This can be hard to grasp from the perspective of value-blind science, when alternative approaches effectively are rendered invisible.

The same theme is being played out again with Transgenics. Consider the development of golden rice, which was aimed at reducing vitamin A deficiency of poor

[30] It is understandable if this claim sounds outrageous, for outrage is the appropriate response. Altieri explains that "[b]y channeling one third of the world's production of grain from livestock to needy people, hunger would cease immediately" (2004, p. 2).

children in ‘developing’ countries. Is golden rice the only, or best, solution to meeting this need? It turns out that in some of the targeted regions there used to be plenty of vitamin-rich vegetables (wild and cultivated) that were wiped out by the implementation of monoculture crops – a legacy, or ‘side-effect,’ of the Green Revolution. While children would have to eat *two pounds per day* of golden rice to obtain sufficient vitamin A, they would only need a few ounces of the wild vegetables that used to be abundant. Furthermore, even if children managed to eat copious amounts of golden rice, they would not necessarily reap the benefits, since vitamin A is fat-soluble, and malnourished children seldom enjoy a fatty diet (Altieri 2004, pp. 9-10). In light of this information it is obvious that golden rice is not a very good solution, much less the only one. It is, however, a technologically sophisticated solution that provides a marketable commodity. I hope this line is starting to sound familiar. Next we turn to the science of agroecology, an alternative approach to agriculture that has the potential to better meet the goals of increasing food production *and* reducing hunger and malnutrition.

II. Agroecology: An Alternative Approach

There is a sense in which discussion of the harms and risks resulting from biotechnological approaches to agriculture would be purely ‘academic,’ given that there were no less harmful or less risky alternatives. Throughout the discussion so far I have sustained the criticism that there has been inadequate investigation of alternative approaches to agriculture which can better fulfill the stated aims of the value-blind approach, generating more benefits and less harm. The science of agroecology is precisely such an alternative, and we will see that the reason it has not been pursued traces to the value-free scientific identity.

Recall that biotechnological approaches use *decontextualizing* methods to determine the material possibilities of seeds, generated from their underlying structure. They impose constraints on theories (hypotheses) that preclude investigation into value-laden (social, cultural, etc.) contexts of seeds; the material possibilities determined by the seeds' underlying structure are supposed to be context-neutral. The result is a perspective which occludes "the perception of linkages" and leads to "a blind spot with respect to relational properties and relational impacts," as Shiva puts it (Shiva 1991, p. 21). In contrast, agroecology is deeply concerned with value-laden contextual factors, aiming not only at increasing crop yields (in terms of biomass), but doing so in ways that promote ecological stability and the well-being of local farmers and farming communities.

In light of the fundamental concern for local contexts, the predominant methodology of agroecology, 'farming systems research,' begins with the knowledge of traditional farmers who have been systematically refining sustainable agricultural practices for generations (Altieri 1995, p. 73). Norgaard and Sikor explain that, in contrast to conventional/modern methods, agroecology "puts relatively little emphasis on laboratory and experiment station research and gives considerably more emphasis to on-farm experiments, and it is more open to participation in the research process by farmers themselves" (Norgaard and Sikor 1995, p. 21). After all, no one is in a better position to understand the unique ecological and socio-cultural conditions in particular locations than the farmers who live and work in those very conditions, and who have inherited knowledge and practices from others who worked in the same (or similar) conditions for many years.

Miguel Altieri describes agroecology as "the discipline that provides the basic ecological principles for how to study, design, and manage agroecosystems that are both productive and natural resource conserving, and are also culturally sensitive, socially just, and economically viable" (1995, p. ix). It is immediately clear that the methodologies of agroecology are *not* decontextualizing methodologies; theories (hypotheses) are *not* constrained to those generable from the underlying structure, process, and law. Instead, theories are constrained to those which concern the possibilities of phenomena in relation to *agroecological systems*, which include the social and ecological contexts. Particular possibilities of interest concern 'sustainability,' as characterized by Altieri, which encompasses the following:

1. Maintenance of the productive capacity of the agroecosystem (productive capacity);
2. Preservation of the natural resource base and functional biodiversity (ecological integrity);
3. Social organization and reduction of poverty (social health); and
4. Empowerment of local communities, maintenance of tradition, and popular participation in the development process (cultural identity). [Altieri et al. 1996, pp. 367-368]

While value-blind approaches often rely on a sharp distinction between research and application, relegating sociological and ecological considerations (especially the risks) to the realm of 'application' (or public policy), agroecological approaches take these considerations to be integral. It is important to see that these issues frame questions that can be answered by systematic empirical inquiry; for example, "How can we produce enough wheat so that all the people in a given region will gain access to a well-balanced diet in a context that enhances local agency and sustains the environment?" (Lacey

1999, p. 194). The sciences of biotechnology, using decontextualizing methodologies, do not have the resources to answer such questions.

<u>The Values of Agroecology</u>

The tension between biotechnological and agroecological approaches does not arise because there is disagreement about the facts of agricultural science (though there is some disagreement, as we will see). By now it should come as no surprise that the fundamental difference pertains to values. Where biotechnology is linked with the values of modernity, agroecology is linked with values such as ecological stability, local well-being, and social justice (Lacey 1999, Ch. 8; Lacey 2005, p. 138).

Ecological stability is a feature of ecosystems that are robust or resilient with respect to fluctuations in the weather and climate, pest populations, disease, soil conditions, etc. Biodiversity is a key characteristic of stable ecosystems, supplying organic resources that enable ecosystems a degree of flexibility. Monoculture crops, the antithesis of biodiversity, are disaster-prone because a single pest, disease, or shift in soil conditions (among other things) can quickly lead to crop failure. In contrast, with polycultures, diverse plants can be selected so that they are not all vulnerable to the same pests (or diseases, etc.) so that the chances of losing an entire crop are slim.

The value of *local well-being* encompasses the physical, psychological, cultural, and social health of local farmers and farming communities. In the previous section we saw how the value-blind sciences of the Green Revolution and transgenics sometimes worked against local well-being, so understood. Where biotechnological agriculture is aimed solely at maximizing food production, agroecological methods are aimed not only at food production, but at feeding people and enabling them to lead the kinds of lives they want to live. Consequently, values penetrate agroecology to its core; in order to study farming methods that will promote good lives for local farmers, we will have to ask farmers (their families, their communities) about their values and about what a 'good life' amounts to for them.

The value of *social justice* is closely related to that of local well-being, especially regarding the last point. The Green Revolution was aimed at 'development,' which sounds unambiguously like a good thing. But it turns out that what the powers that be in the Western world see as development, many of its 'beneficiaries' see as well-meaning coercion (if not outright exploitation) in light of its many detrimental effects for local peoples and its many benefits for the West and global market interests (see Harding 2008). Furthermore, to the extent that it did help local people, there is evidence that modern development primarily helped those in positions of power, who occupy higher social classes, and are mostly male (Lipton 1989; Hecht 1995). In contrast, agroecology is aimed at helping those who are the most in need, on their own terms, and in ways that help remediate unequal global power relations and social injustices.

Agroecology may start to sound more like a social movement than a science, and with good reason.[31] While we may be used to thinking about science as a means for social change, we do so primarily in the context of the value of technological progress. Techno-scientific innovations are supposed to pull 'primitive' people out of the gutter and into modernity, where no one goes hungry and everyone is equal (right!).[32] Despite the fact that people go hungry and are not treated equally in the West, this mindset of

[31] For an analysis of the various dimensions of agroecology see Wezel, et al (2009).

[32] Sandra Harding provides an incisive critique of this view (Harding 1998, Ch. 2; Harding 2006, pp. 34-36).

modernity is pervasive. Value-blind science is so deeply embedded in the values of modernity that it does not perceive them (this is why I call them ‘value-blind’). If I may coopt an analogy from Melville, when engaging in ‘value-free’ inquiry, “we are too much like oysters observing the sun through the water, and thinking that thick water the thinnest of air" (Melville 2003, p. 41). How does one explain the glimmering sun to the oyster that considers itself water-free? A science such as agroecology that contests the values of modernity inevitably will seem radical. Nonetheless, agroecology has emerged as a science suited to investigate empirical questions within agriculture that are linked with values like ecological stability, local well-being, and social justice, and it has been quite successful.

The Benefits of Agroecology

One of the clearest benefits of agroecology is that it is able to meet the particular needs of farmers by actively involving them in research. For instance, in what is called “farmer-back-to-farmer” research, scientists begin by formulating specific problems to be solved in consultation with farmers (who actually face the problems in everyday life). An interdisciplinary team of scientists develops methods and technologies to address the problems, with the farmers contributing and helping to evaluate the proposed solutions. The farmers’ involvement can lead to novel insights, not only as to the local conditions, but how to evaluate the results. For example, when the farmer-back-to-farmer method was being used to develop improvements in potato farming, it was found that “what appeared to be losses to scientists were not necessarily losses to farmers, who had uses for shriveled or spoiled potatoes” (Altieri 1995, p. 79).

In taking the perspectives and interests of farmers seriously, agroecological methods help to reveal how 'value-free' concepts are in fact value-laden. We have already seen how the category of 'high yielding varieties' is value-laden, since that concept as deployed in the Green Revolution relied heavily on values of the market. The concept of a 'weed' follows a similar pattern (see Shiva 1993, pp. 22-27). From the view of Transgenics weeds are *bad*, which is why we need herbicide resistant crops that can be showered with glyphosate. But, as Altieri explains, traditional farmers do not think weeds are necessarily bad:

> In many areas of Mexico, for example, local farmers do not completely clear all weeds from their cropping systems. This 'relaxed' weeding is usually seen by agriculturalists as the consequences of a lack of labor and low return for extra work. However, a closer look reveals that certain weeds are managed and even encouraged if they serve a useful purpose. In the lowland tropics of Tabasco, Mexico, there is a unique classification of non-crop plants according to use potential on one hand and effects on soil and crops on the other. Under this system, farmers recognized 21 plants in their cornfields classified as *mal monte* (bad weeds) and 20 as *buen monte* (good weeds). The good weeds serve as food, medicines, ceremonial materials, teas, and soil improvers. [Altieri 1995, p. 297]

What seems like 'relaxed' weeding from the perspective of modern scientists might be well-described as 'strategic' weeding from the perspectives of local farmers. Agroecology, by being open to farmers' values, can develop approaches to farming that are resource efficient and both socially and culturally appropriate.

Last, it is worth expanding on the benefits of polyculture crops, which are a staple of the agroecological approach. As previously mentioned, polycultures support ecological stability, but they have many additional advantages. The specific crops of polycultures can be mutually beneficial; a classic example of this is the 'Three Sisters' of corn, beans, and squash. When planted together the corn stalk allows the bean to grow upward to reach the sun, the beans and corn provide shade for the squash, the squash reduces weeds, and the beans increase the nitrogen level of the soil (which benefits the corn). Not only do these (and other) ecological interactions make the Three Sisters resilient, it makes them more productive, even in the 'high yielding variety' sense of crop production. *A monoculture crop of corn produces a smaller yield than a polyculture crop of Three Sisters on the same amount of land* (Altieri 1995, p. 112; Liebman 1995, p. 206; Miller 2005, p. 278). Furthermore, while polycultures produce less quantities of each individual crop, their variety helps to ensure "a regular and varied food supply [and] a diverse and nutritionally adequate diet" (Altieri 1995, p. 108).

If polyculture cropping produces a high yield, is ecologically stable, and provides for the nutritional needs of farming communities, why are more than 90 percent[33] of farms still dominated by monocultures? The prevalence of monocultures can partially be explained by reference to the values of modernity. While the Three Sisters provide a *higher total yield*, a monoculture crop of corn produces *more corn*. Corn, for a variety of socio-economic reasons, has a very large market, so for those with interests in the *global* market monocultures of corn are of greater value than polycultures like the Three Sisters. It is important to emphasize that monocultures serve the *global* market because polycultures have the advantage for local farmers' markets. Polycultures can claim this advantage because they "enhance opportunities for marketing, [and ensure] a steady supply of a range of products without much investment in storage, thus increasing marketing success" (Altieri 1995, p. 113).

[33] Altieri 2004, p. xi.

But the values of the market are not the only ones at work; in particular, the values of technological progress encourage a biased conceptualization of traditional farming methods as 'primitive.' For example, horticulturist Jane Mt. Pleasant points out that "[m]ost people view the Three Sisters as a rather charming but primitive planting system. However, they find it hard to attribute value or agricultural productivity to a cropping system that mixes three species of plants in what appears to be a haphazard jumble" (Mt. Pleasant 2006, p. 529). Similarly, Hecht identifies a bias connecting 'primitiveness' with 'disorder':

> The transition of epistemologies [during the scientific revolution] shifted the view of nature from that of an organic, living entity to one of a machine. Increasingly, this approach emphasized a language of science, a way to talking about the natural world that essentially dismissed other forms of scientific knowledge as superstitions. […] This position, coupled with an often derogatory view of the abilities of rural peoples generally, and colonized populations in particular, further obscured the richness of many rural knowledge systems whose content was expressed in discursive and symbolic form. Because the ecological context was misunderstood, the spatial and cultivar complexity of non-formalized agricultures was frequently reviled as disorder. [Hecht 1995, pp. 3-4]

While Mt. Pleasant and Hecht highlight the 'haphazard' appearance, it is easy to see the resistance to the polyculture method as deriving also from its lack of technological sophistication; it is technologically 'primitive' and is therefore presumed to be illegitimate, ineffective, or unscientific.[34] This sentiment is clear, for example, in the evocative, if factually incorrect, rhetoric of Norman Borlaug:

> The affluent nations can afford to adopt elitist positions and pay more for food produced by the so-called natural methods; the 1 billion chronically poor and hungry people of this world cannot. New technology will be their salvation, freeing them from obsolete, low-yielding, and more costly production technology. [Borlaug 2000, p. 490]

[34] Sandra Harding highlights a similar theme, though in a wider context (Harding 2008, p. 139)

In light of the ecological elegance, superior productivity, and numerous additional benefits of the Three Sisters, it is clear that resistance to it (and similarly successful polycultures) does not have an empirical basis, but can be partially explained by the fact that it conflicts with the values of modernity. On the other hand, the Three Sisters is a perfect fit for the values and methods of agroecology.

Throughout this analysis of the values and methodologies of agricultural science, I have focused on the bad outcomes of biotechnology and the good outcomes of agroecology. However, I should emphasize that I do not believe biotechnology, or value-blind approaches in general, tend to have bad outcomes, or that they should not be pursued. Instead, my goal is to show that value-blind science is not value free, since it is linked with the values of modernity. I contend that the value-free identity of science blinds us to the influence of these values, leading us to believe that alternative approaches which do not purport to be value-free are *ipso facto* unscientific. When in this vein we marginalize or overlook alternative approaches, we miss out on possibilities for gaining scientific knowledge that are occluded or inaccessible from the value-blind approach. When those overlooked possibilities, such as agroecology, hold the promise to significantly improve people's lives, our blindness results in both a cognitive and a moral failure.

Competition between Biotechnological and Agroecological Approaches

Once we see that biotechnology and agroecology are both legitimate scientific approaches, the most obvious way forward might be to pursue both at the same time. In fact, they are both being pursued today; however, compared to biotechnology, agroecology has been seriously neglected and marginalized. I have already explained one reason for this – from the perspective of value-blind science, agroecology is primitive and unscientific. It does not receive much funding for research from the major scientific institutions. Neither does it receive much funding from private firms, since it does not promise to be very profitable. The systems and institutions in place that promote and fund science largely support, and are supported by, value-blind science. For this reason, agroecology has been developed and supported primarily by grassroots movements and non-governmental organizations with access to few resources.

In addition to the social structures that are biased in favor of value-blind science, the pursuit of agroecology is undermined by the material and social consequences of decades of research and development in biotechnology (Lacey 2005, Part III). During the Green Revolution many traditional farmers were displaced to urban areas when they could no longer afford input-intensive farming (Patel 2012, Ch. 1); the knowledge of traditional farming practices they took with them – knowledge which is foundational to agroecology – is now lost. Furthermore, the environmental destruction caused by the widespread implementation of monocultures devastated the ecosystems and biodiversity on which traditional farming practices depend (Shiva 1993, p. 53; Harding 2008, p. 139). Last, the commoditization of seeds, i.e., via patents claimed by agribusinesses, has the result that many traditional farmers no longer have access to seeds as regenerative resources. This undermines one of the central aims of agroecology, which is to manage farming systems in ways that are "culturally sensitive, socially just, and economically viable" (Altieri 1995, p. ix).

Thus, as it stands, the widespread pursuit of biotechnology systematically eliminates the social and material conditions necessary for agroecological research. While this is not a *necessary* consequence of pursuing biotechnology, it is a predictable one, given the mutually reinforcing links between value-blind science and the values of modernity. It would take large-scale social and political reforms in order to reshape biotechnological research in ways that would enable it to be pursued alongside agroecology. If such political and social reorienting is not feasible, the best hope for agroecology is that biotechnology ceases expansion, leaving room (however inadequate) for agroecological research (Lacey 2005, pp. 221-223). If, instead, there is a second or third Green Revolution (depending on who is counting), agroecology may have to be studied by historians instead of scientists.

Chapter 6: Values in Nutritional Science

The case studies in this section concern different approaches to the science of nutrition. Following the same pattern as in Chapter 5, I begin by discussing the conventional modern approach to nutrition, which deploys a value-blind (decontextualizing) methodology. It is important to remember that by 'value-blind' I certainly do *not* mean that these approaches are unrelated to social values (or the 'right' social values); indeed, they often acquire worthy aims (such as promoting individual health). What they are blind to is their links with the values of modernity. Moreover, this form of blindness is compounded: the failure to notice these linkages (which we are less likely to notice if we believe science is value free) blinds us to the possibility that there are other legitimate approaches linked with other values. Thus, throughout the first half of this chapter (§I) I will tease out the links between value-blind approaches to nutrition and the values of modernity.

Along the way I highlight some of the negative consequences which might be avoided by pursuing alternative approaches. This should not be taken as a condemnation of mainstream approaches, which have obviously improved people's lives in many ways. The point is to show that there is room for alternative approaches which may have different and perhaps more effective tools to achieve shared social aims. Finally, in §II, I turn to two distinctive alternative approaches, 'wholistic' nutrition and nutritional ecology, and explicate their benefits, unique methodologies, and links with values.

I. Reductionist Nutritional Research: A Value-blind Approach

Experiments aimed at discovering a treatment for scurvy in the 18th century were perhaps the earliest of modern research in nutrition. These investigations were simple, but fruitful. Scientists gave different foods corresponding to various folk remedies to sailors inflicted with scurvy and observed the effects. As is well known, the experiments indicated citrus fruits as an effective treatment. The College of Physicians in London, who believed that scurvy was "a 'putrid' disease" and that "animal tissues that went putrid became alkaline," thought the prominent acidity of citrus fruits must have been the source of the cure (Carpenter 2003a, p. 643). Unfortunately, they therefore concluded that other acidic substances could serve equally well as treatments for scurvy and recommended the use of "stable acids like sulfuric acid (diluted before use!) or vinegar" as substitutes that would be suitable for long-term storage (i.e., on long sea voyages) (*ibid*.). Of course, now we know that it was not the acidity, but the vitamin C that was doing the work.

Technological advances eventually led to the discovery of vitamins early in the 20th century, at which point most nutritional research could accurately be characterized as the search for 'one nutrient, one disease' relationships. In his account of the history of nutritional science, Kenneth Carpenter explains that "[t]hose who had begun their careers in the 1930's looked back on it as 'the golden age of nutrition,' with its rapid discoveries of one nutrient after another" (Carpenter 2003b, p. 3031). Carpenter's narrative closely follows the 'one nutrient, one disease' theme, as illustrated by his section titles, such as "Rickets and vitamin D" and "Beriberi and vitamin B" (*ibid*.). As explained in the following passage from *Nutrition in the Prevention and Treatment of Disease* (2012), this approach remains the standard:

> [N]utrition implicitly holds to a 'one-nutrient—one-disease' conceptual model. As commonly taught, *the* disease of thiamin deficiency is beriberi; *the* disease of niacin deficiency is pellagra […] and so on. Although there is no expressed objection by nutritional scientists to recognizing multiple systemic consequences of nutrient inadequacy, the hold of the one-nutrient—one-disease model continues to dominate both nutritional policy and the regulation of health claims. Specifically, it has been the organizing principle not only for nutrition scientists but also for food and nutrition policymakers. [Heaney and Roller 2012, p. 249]

Investigations using the 'one nutrient, one disease' (henceforth, N/D) model are value-blind (decontextualizing) approaches in virtue of studying phenomena abstracted from their value-laden contexts and formulating theories in terms of underlying structure, process, and law. In fact, this approach attempts to dissociate N/D relationships from virtually *all* contexts to such an extreme that it has attracted some criticism. However, it is important to see that while the narrow scope of this approach is often symptomatic of value-blind methodologies, it is not a necessary feature. One might criticize the N/D approach for being *too narrow* or for being *pursued single-mindedly* to the exclusion of most other approaches (in particular, excluding approaches *not* linked with the values of modernity). The latter is the central concern of this chapter.

The fact that nutrition is *taught* primarily using the N/D model confirms that such approaches are the norm in modern nutritional science. Professor and nutritional biochemist, T. Colin Campbell, explains that nutrition is taught as "the arithmetic summation of the effects of individual nutrients" as expressed in such aphorisms as "'[c]alcium grows strong bones,' '[v]itamin A is necessary for good eyesight,' and '[v]itamin E is a cancer-fighting antioxidant'" (Campbell 2014, p. 59). According to

Campbell, students are trained in the standard methodology which "identifies the component parts of food–the individual nutrients–and figures out exactly what each one does in the body and how much of it we need" (*ibid*.). It should not be surprising, then, that contemporary research in nutrition is mostly conducted via value-blind approaches.

Value-blind Methodologies and 'Reductionist' Science

Perhaps the most compelling evidence that mainstream nutritional science is dominated by value-blind methodologies is found in the 'reductionism vs. holism' debate within the discipline. The reductionism/holism distinction turns out to be more complicated than it is made out to be in the nutrition literature. For now it will suffice to think of reductionism as the mainstream approach to nutrition, and holism as the position often taken by critics of reductionist approaches. Among the most outspoken holists are Campbell and his son, Thomas M. Campbell (henceforth C&C), who received widespread attention (both acclaim and controversy) with their book *The China Study* (2006):

> [The] method of investigating details out of context, what I call reductionism, and trying to judge complex relationships from the results is deadly. […] The greatest danger is that reductionism [*sic*] science, standing naked from its larger environment, has come to be the gold standard. Indeed, I know many researchers who would even say that *this is what defines "good" science*. [C&C 2006, p. 287, my emphasis]

Right away we see the familiar pattern whereby the dominant methodologies are understood as the only 'scientific' option. This is symptomatic of the value-blindness which renders alternatives invisible. The holists are not only concerned with the limitations of reductionist approaches, they are troubled by the prevailing wisdom that reductionism is the *only* legitimate approach. I challenge this sentiment throughout §II, but first more needs to be said about reductionist approaches to nutrition.

Somewhat surprisingly C&C tend to frame issues in the idiom of philosophy of science; indeed, their criticism of 'reductionism' is often (quasi)philosophical. However, they are not always very careful in how they use philosophical terms. For example, C&C appear to switch between using 'reductionism,' on the one hand, to refer to a *methodology* (as in the previous quote) and, on the other hand, to refer to an *epistemology*. In the latter case, the sense of 'reductionism' at play is mereological; roughly, it is the view that a whole can be understood fully in terms of its parts. For instance, at one point C&C define reductionism as "the mistake of characterizing whole foods by the health effects of specific nutrients" (C&C 2006, p. 271).

C&C are not the only nutritional scientists who have identified what they call 'reductionism' in their discipline. The term occasionally shows up in the scientific literature, but, as in the case of C&C, it is not always used consistently. There are frequent shifts between using 'reductionism' to refer to a *value-blind methodology* and to a *reductionist epistemology*. I will refer to approaches that embody *both* of these methodological/epistemological aspects as 'reductionist approaches' or 'reductionist science,' etc. In cases where just one aspect is relevant, I will make it explicit.

The value-blind methodology used with the reductionist approach is encapsulated in the standard 'one nutrient, one disease' mantra. For example, nutritional scientists have characterized reductionism (*qua* value-blind methodology) as an approach which "combines a single micronutrient with a single health-related biological effect" (Fardet and Rock 2014a, p. 182), focuses exclusively on the "associations between single food compounds and single physiologic effects" (Fardet and Rock 2014b, p. 431), reveals "a linear cause-effect relation" (Fardet and Rock

2014b, p. 432), and generally proceeds by "emphasizing the role of a single chemical" (Messina et al. 2001, p. 1416). Sometimes it is made quite clear that the reductionist approach ignores important broader contextual factors:

> Nutrition and health are central dimensions of the way of life, which has been known since antiquity as *díata*. The present-day constriction of this broad meaning to the term 'diet' covers only the aspects of eating and drinking and is a symptom of the reductionism of our age. [Meyer-Abich 2005, p. 1]

> [Reductionist research] seems airtight because it deals with objective facts—reactions, genetic mutations, and carcinogenesis—as opposed to messy things like human behavior and lifestyle. Only by excluding messy and complex reality can we make linear, causal statements about biological chain reactions. [Campbell 2014, p. 63]

In other instances, sometimes even in the course of a single sentence, 'reductionism' is also used to refer to a reductionist epistemology. For example, it is explained that the reductionist "objective of science is to reconstruct reality by its parts" (Hoffmann 2003, p. 514S); reductionist scientists "believe that everything in the world can be understood if you understand all its component parts" (Campbell 2014, p. 47), and so they strive "to explain a phenomenon by dividing it into parts" (Fardet and Rock 2014b, p. 431).

What is not generally recognized in the nutrition science literature is that a value-blind methodology and reductionist epistemology can come apart. One can study an isolated N/D relationship without believing (or it being the case) that a whole can always be explained by its parts, so the adoption of a value-blind methodology does not require the adoption of a reductionist epistemology. Moreover, one who adopts a reductionist epistemology is not thereby limited to the study of isolated N/D relationships; the study of complex interactions among nutrients is not precluded by a reductionist epistemology (e.g., it could accommodate a 'two nutrients, one disease' model, etc.). Still, it is clear enough that these views are often linked in mainstream nutritional science; indeed, they seem to reinforce one another. Given that students – future scientists – are taught that *the* way to contribute to the body of scientific knowledge on nutrition is to study its isolated 'parts' (nutrients), perhaps it is reasonable for them to conclude that an understanding of all the individual N/D relationships is all there is to know about nutrition.

<u>Reductionist Approaches to Nutrition and the Values of Modernity</u>

It is clear that modern nutritional science is mostly pursued via value-blind methodologies, but so far not much has been said about values. I maintain that value-blind methodologies are dialectically linked with the values of modernity. It turns out that the critics of reductionist nutritional science are sometimes aware of these links. Although they do not deploy the language of values, Campbell and Campbell systematically identify connections between mainstream nutritional science and the values of modernity. For instance, they write at length about the interaction of market values and reductionist research:

> Corporate influence in the academic research world can take many forms […]. This influence does not need to be a crass payoff to researchers to fabricate data. That sort of behavior is rare. The more significant way for corporate interests to influence academic research is much more sophisticated and effective. […] scientists investigate a detail out of context that can be construed as a favorable message and industry exploits it for all it's worth. [C&C 2006, p. 299]

While C&C do not shy away from exposing corrupt research practices, they are clear that even well-meaning scientists are playing into the hands of corporations merely by engaging in reductionist research; that is, by investigating the effects of "single nutrients […] without seeking or understanding the larger context" (C&C 2006, p. 288). They condemn the marketing ploy that takes research out of context in order to make "sweeping assumptions about complex diet and disease relationships," and they fault scientists who engage in reductionist research for making marketers' jobs a cakewalk (C&C 2006, p. 286). This sort of condemnation of reductionist research is somewhat unfair, but it serves well to point out that, like it or not, modern science is not *neutral*. The knowledge and (as we will see) technology produced via reductionist nutritional research undeniably serves market interests especially well.

C&C also level criticism against reductionist science in virtue of its links with the values of technological progress. They even identify cases exemplifying the view that all problems have technological solutions. In a case reminiscent of transgenic 'golden rice' (presented as solution to vitamin A deficiency), C&C find fault with researchers who propose to modify foods by enriching them with anti-carcinogenic compounds. To be sure, such a project serves a laudable aim (preventing cancer), but the reductionist approach may not be the best way of achieving it. The proposal is to use sophisticated technological methods to enrich foods typical of the Western (affluent) diet – high in fat, carbohydrate, and sugar – with isolated compounds *found in ordinary fruits and vegetables*:

> Rather than avoiding bad foods altogether, these researchers are suggesting that we tinker with the existing, but problematic, foods to correct the problem. Instead of working with nature to maintain health, they want us to rely on technology […]. This faith in technological tinkering, in man over nature, is ever-present. [C&C 2006, p. 300]

In this passage C&C deploy a dismissive rhetoric that seems to express contempt for modern technology. I do not mean to endorse a Luddite, or anti-technology, approach to science (and I do not think C&C mean to either). However, this case exemplifies the way in which blindness to the role of values influences the way we think about science.

This sort of food modification fits a pattern whereby technological solutions are generally assumed to be the best, and perhaps only, option.

Illustrating a similar point, C&C cite an episode in reductionist nutritional research in which experiments revealed a connection between consumption of (whole!) tomatoes and a reduced risk of cancer. Somewhat predictably, researchers zeroed in on a particular chemical compound, lycopene:

> There is no evidence for a lycopene-specific effect on prostate cancer […]. Nonetheless the lycopene business is up and running. In-depth studies are underway to determine the most effective dose of lycopene as well as to determine whether commercial lycopene preparations are safe […]. Also, consideration is being given to the possibility of genetically modifying plants for higher levels of lycopene and other carotenoids. It is a real stretch to call this series of lycopene reports legitimate science. In my book, this is what I call technological tinkering and marketing, not science. [C&C 2006, p. 301]

C&C are overly hasty to disregard lycopene research as illegitimate science; the truth of such accusations could only be ascertained on a case by case basis.[35] Although they expose some questionable research practices, it is obvious that their criticism is driven in no small part by their dispute with the values associated with reductionist research. That their dispute is about values is supported by the fact that C&C propose a (purportedly) superior alternative approach to nutritional science, one which is linked with a different set of social values (I explore C&C's alternative view, 'wholistic nutrition,' in §II). Although their approach has gained some traction of late, C&C believe that progress has been slow because the dominant approach often "rejects, beforehand, the idea that it may not always be the best or only way to apprehend and measure reality" (Campbell 2014, p. 54). The rest of this section will help to motivate the possibility that (assuming there are other options) reductionist approaches are not

[35] Some recent studies support C&C's conclusions; see Fardet and Rock (2014b, p. 436) for some specific problems in lycopene research.

obviously 'the best way,' then in §II we will see that they are certainly not 'the only way.'

The Consequences of Pursuing Nutrition via Value-blind Methodologies

While reductionist research on nutrition has certainly provided invaluable scientific knowledge, its fruitfulness does not justify the exclusion of all other approaches. The discipline's tunnel-vision is egregious if there are alternative approaches with the potential to better achieve some of the goals of nutritional research. The cases and criticism presented in the remainder of this section aim to expose some of the negative consequences that result from mainstream research on nutrition, thus establishing room for better alternatives. It is wise to keep in view, however, that the greatest harm results not directly from decontextualizing research or its products, but from the way it interacts with the values of modernity (under the guise of value-freedom) and blinds us to alternative approaches.

There are two prominent and related problems caused by the pervasive reductionist approach to nutrition that relies on the N/D model. First, it encourages us to think of certain food compounds simply as 'good' or 'healthy' *in general*; in fact, however, when there are benefits (of isolated food compounds) at all, they only obtain in particular contexts and for particular purposes. I call this the problem of *oversimplification*. Second is the problem of *confusion*; value-blind approaches often yield contradictory and confusing results that muddle nutritional advice and public health issues. Both problems are perspicuous in the case of dietary supplements.

Dietary Supplements

The alleged benefits of multivitamin pills, omega-3 supplements, and the like, are established on the basis of reductionist approaches that focus on the effects of single nutrients (Burlingame 2004; Fardet and Rock 2014a, p. 182). However, recent studies have revealed that some of these supplements can lead to increased risk of cancer (beta-carotene, vitamins A and C), heart failure (vitamin E), and even mortality (Fardet and Rock 2014a, p. 182; Fardet and Rock 2014b, pp. 434-435; Campbell 2014, pp. 156-160). Moreover, some of the purported preventative effects of dietary supplements for chronic diseases have been debunked (Fardet and Rock 2014a, p. 182). Nonetheless, nutritional supplements remain in high demand. Part of the explanation for this is that we have latched on to the real benefits of taking vitamin supplements—benefits obtaining primarily in cases of vitamin deficiency—and taken them out of context. According to common wisdom *vitamins are healthy*. This *oversimplification* is encouraged by the kind of knowledge produced by reductionist research that tends to focus on isolated N/D relationships. But while we can excuse a child who believes "vitamins are good for you," we might think adults should be aware that the truth is not so simple.

Thus, we turn to the problem of *confusion*. It is not the case that the general adult population is gullible; rather, they are bombarded with contradictory nutritional advice which makes it genuinely difficult to decide what to believe about nutrition. In their article in *Advances in Nutrition*, Fardet and Rock argue that reductionist approaches play a central role in propagating confusion about nutrition:

> [Consider the] contradictory results of some observational studies concerning some diet-disease associations (e.g., dairy products and bone health, particularly

> osteoporosis). Some studies showed protective effects, others showed no significant effects, and some show negative effects. Such contradictions may be partly explained by an excessively reductionist approach, which does not consider some confounding factors, genetic polymorphisms of the studied population, or other unknown associated factors. [Fardet and Rock 2014b, p. 434]

If the science of nutrition is perceived by the public to be something less than scientific, it is partly because reductionist approaches have led to so many contradictory results. The reason for this, as Campbell puts it, is that "there's an inherent trade-off between that kind of certainty within a controlled environment and its applicability in the messy, noisy real world. […] The more perfectly controlled the experiment, the less it resembles reality" (Campbell 2014, p. 82). By minimizing confounding factors and, in general, abstracting away from the context as much as possible, the results of value-blind research (even when they reveal real and genuine isolated relationships) are often useless guides for nutrition in our daily lives. In nutritional science, the minimization of confounding factors is too often correlated with the propagation of confounding results.

It is worth pointing out that the phenomenon of dietary supplements is itself a manifestation of the values of modernity. For example, Fardet and Rock are cognizant of the links between dietary supplements and the market:

> Indeed, we must recognize that the large amount of marketed functional foods [e.g., vitamin-enriched foods] or nutritional supplements (e.g., vitamin(s), mineral(s) and/or plant-derived supplement(s)) has not really succeeded in arresting the development of chronic disease epidemics […]. Instead, the results were far from convincing, and more studies reported harmful physiological effects of nutritional supplements […]. It seems that this reductionist approach has mainly benefited the agro-food industry and sellers of nutritional supplements. [Fardet and Rock 2014a, p. 182]

Campbell, who draws many similar conclusions, also perceives links between dietary supplements and the values of technological progress; he supposes that the dietary supplement industry is founded upon a "techno-scientific fantasy that we can get all our nutritional needs met by powders, pills, or cubes" (Campbell 2014, p. 153). This may sound like an overstatement (who wants to eat a food-cube?); yet it is borne out in some recent trends.[36] In any case, the point is that reductionist research serves especially well those interests related to the values of modernity.

The Aims of Nutritional Science: Are We Healthier?

The central aim of modern nutritional science is to provide knowledge that can guide our dietary choices so that we are well-nourished, free of disease, and ultimately lead longer and healthier lives. So far in Western societies we are succeeding at leading longer lives, but we are *over*-nourished and prone to develop chronic diseases. Consequently, we have not clearly succeeded at leading healthier lives. Consider the success story of life-expectancy in the West. A recent study in France showed that life expectancy is more than 80 years on average, and increasing by 3 months every year (Fardet and Rock 2014b, p. 430). We are living longer than ever! Unfortunately, this triumph is perhaps on par with that of the Green Revolution (i.e., for increasing food production) – it is a story of quantity, not quality. While life expectancy is steadily increasing, the duration of 'healthy life years' is steadily *decreasing*, and *at a higher rate*. In France, the average duration of life with chronic diseases increased by a *full year* from 2008-2010, a one year average decrease in healthy life years. The same study reported an average of 20 years in a "chronic disease state" (*ibid*.).

Of course, there are a lot of complex factors that contribute to human health. As mentioned in Chapter 5 (§I), overpopulation contributes to a host of worldwide

[36] There are several 'meal-replacement' products on the market (e.g., powder drink mixes) that purport to meet every nutritional need for humans, and which are not very far removed from 'food-cubes.'

problems, including poverty and disease, both of which obviously impact human health. I certainly do not mean to implicate reductionist nutritional science as the sole or primary cause of the decrease in 'healthy life years' seen in France. However, the connection between diet and health is undeniable, and there seems to be room for improvement when it comes to the way we approach nutritional science. It is safe to say we have not fully achieved the aims outlined above; thus, the question is whether we have overlooked promising alternatives to mainstream approaches to nutrition.

Let me reiterate that I have purposefully focused on some of the bad consequences of the single-minded pursuit of value-blind approaches to nutrition. There have certainly been good consequences resulting from modern nutritional research; for example, it is uncontroversial that the knowledge gained regarding the treatment of diseases arising from vitamin deficiency has saved millions of lives (Fardet and Rock 2014b, p. 432). Again, I am not advocating that we cease research by conventional means. However, we would be remiss not to investigate the possibility of alternative approaches that may be able to help us achieve our aims. In the next section I highlight two such alternative, wholistic nutrition and nutritional ecology.

II. Wholistic Nutrition and Nutritional Ecology: Alternative Approaches

In order to be an alternative to mainstream nutritional science, an approach must deploy methodologies that are dialectically linked with values which exclude (or subordinate) the values of modernity. The alternative approaches considered in this section are linked with the values such as *sustainability* and *egalitarianism*. I emphasize the role of values because it is tempting to focus instead on the holism of these approaches. After all, it is true that neither wholistic nutrition nor nutritional ecology studies isolated N/D relationships. However, holism can be compatible with a value-blind approach; this is one reason it is important to see that a value-blind methodology and reductionist epistemology can come apart (§I). In light of this complication, the relationship between value-blind methodologies, reductionism, and holism needs clarification before I turn to the two alternative approaches.

Nutrigenomics: A Value-blind and Holistic Approach

One central motivation for pursuing diverse approaches to nutrition is the sheer complexity of the phenomena and processes that contribute to nourishment and health. Anyone who looks closely at the state of the art will stand in awe of the complexity of nutrition, but not everyone will draw the same conclusions about how to handle it. Some scientists maintain that the standard approaches alone cannot generate a comprehensive account of health and nutrition in the face of such complexity. Other scientists believe that we should keep investigating nutrition with a 'value-free' approach, and that we only need *more sophisticated technologies* in order to delve deeper, unlock the mysteries of nutritional complexity, and ultimately become masters of our nutritional fate. As an example of the latter view, we now turn to the field of nutrigenomics.

In the previous section, I introduced the holistic/reductionist debate in nutrition and pointed out that the reductionist approach utilizes a value-blind methodology; however, I did not fill in the details of the holistic approach. The reason for this is that, although it may sound paradoxical, some holistic approaches use a value-blind – decontextualizing – methodology and others do not. The alternative approaches we will be considering later in this section (wholistic nutrition and nutritional ecology) deploy holistic and *value-sensitive* (non-decontextualizing) approaches, so it will help to contrast these with nutrigenomics, which takes a holistic and value-blind approach.

The study of nutrigenomics analyzes nutrition at a genetic level; for example, it has demonstrated that nutrition can influence gene expression. One of the implications of this is that "depending upon the genotype of an individual, the metabolism of nutrients may vary and ultimately result in a different health status" (Corthésy-Theulz, et al. 2005, p. 355). Nutrigenomics requires a holistic approach due to the complexity of interactions involved in nutrition at the genetic level; it therefore employs the methods of 'systems biology,' which integrates the study of "genomics (DNA), transcriptomics (RNA), proteomics (protein), [and] metabolomics (metabolites)" (Corthésy-Theulz, et al. 2005, p. 359). While each of these areas are typically studied in isolation, the systems approach integrates them in mathematical models by utilizing 'bioinformatics,' advanced computing technologies that can store, integrate, and analyze enormous data sets (Corthésy-Theulz, et al. 2005, p. 363).

Notice that this approach is *holistic*; it rejects a reductionist epistemology, since the kind of system being analyzed "is not just an assembly of genes and proteins [and] its properties cannot be fully understood merely by drawing diagrams of their interconnections" (Kitano 2002, p. 1662; c.f., Sauer, Heinemann, and Zamboni 2007, p. 550). Nutrigenomics researchers adopt a holistic epistemology, which can be characterized as the view that understanding the individual parts of a system is *not* sufficient for understanding the whole system. Nonetheless, the methodologies of nutrigenomics are value-blind (decontextualizing) ones: Even while the approach is holistic (non-reductionist), its theories (hypotheses) are *constrained to 'value-free'* *contexts*, focusing on the *material possibilities* of nutrients and nutritional processes at the genetic level, described in terms of *underlying structure, process, and law*. Moreover, the methodologies of nutrigenomics are *dialectically linked with the values of modernity*.

The technology of nutrigenomics and systems biology is about as sophisticated as it gets. Nonetheless, it is widely recognized that we will need *even more advanced technology*, to unlock the full potential of nutrigenomics. This is because a fully integrated model of nutritional systems is so complex that it overwhelms today's bioinformatics technologies (Corthésy-Theulz, et al. 2005, p. 362). One of the most "comprehensive" and promising attempts at integration so far only managed to integrate *two* of the four targeted data-types (Sauer, Heinemann, and Zamboni 2007, p. 551). Yet researchers retain optimism:

> […] the complexity of the relationship between nutrition and health can be met by [systems biology]. At the moment this is little more than a dream […]. Many hurdles need to be taken, most of them in the field of bioinformatics, before this area matures. [*ibid*.]

Without a doubt, the rate at which technology progresses warrants a good deal of optimism in this area of research. However, there is a danger in being blind to the fact that the value of technological progress has a strong hold on us.

In the conclusion of one study, researchers suggest that their high-tech approach stands as an *ideal for nutritional science*: "Advancement of research in computational and analytical science will gradually transform nutrition into a more systematic and hypothesis-driven science" (Corthésy-Theulz, et al. 2005, p. 364). From their perspective technology holds the key to unraveling the complexity of nutritional systems and without technological advances there is little hope for a "systematic and hypothesis-driven science" of nutrition. The values of technological progress are so deeply ingrained that these researchers cannot even conceive of alternative approaches to nutrition that are 'systematic' and 'scientific' *without* being incredibly sophisticated technologically.

Now we have seen that value-blind approaches may be consistent with either a holist or reductionist epistemology. Finally, in the remainder of this section, I present two holistic and *value-sensitive* (non-decontextualizing) approaches which are dialectically linked with values like *sustainability* and *egalitarianism*, rather than the values of modernity.

<u>Wholistic Nutrition</u>

The first thing one might notice about "wholistic nutrition" is the peculiar 'w.' Campbell uses this unconventional spelling because he believes the usual spelling ('holism') has religious connotations and that it evokes thoughts of "'fairy-tale' belief systems" which he thinks prejudices mainstream (reductionist) scientists against holistic approaches (Campbell 2014, p. 48). However, we saw with nutrigenomics that some holistic approaches have gained traction despite the fact that reductionist approaches dominate nutritional science. I strongly suspect that nutrigenomics is able to gain traction because it does not depart from the hegemony of value-blind approaches to nutrition. Although it adopts a holist epistemology, the methodology of nutrigenomics is linked with the same values as the reductionist approach and so easily finds a home in mainstream nutritional science. Campbell's unconventional spelling of 'wholism' thereby serves an additional purpose by helping us distinguish between 'holism,' which refers to a holistic epistemology (which may or may not be paired with a value-blind methodology), and ‘wholism,’ which refers specifically to Campbell’s holistic and *value-sensitive* approach to nutrition.

I am inclined to think Campbell would welcome this added dimension to his terminology since it helps clarify his view. In fact, it helps to clarify critiques of reductionist nutritional science more generally. Consider how Campbell contrasts wholism and reductionism in the following passage: “wholistic ways of exploring reality […] don’t narrow cause and effect to the point where everything is airtight, completely repeatable, and measurable to the fifth decimal place, the way reductionist experimental design does” (Campbell 2014, p. 86). If Campbell were referring to holistic/reductionist *epistemologies*, then the field of nutrigenomics would be a clear-cut counterexample to his claim. Nutrigenomics rejects reductionism in favor of a holistic epistemology, but it *does* aim at modeling every detail of the nutritional system in a way that is ‘airtight, repeatable, and measureable to the fifth decimal place’ (and then some). However, if we interpret Campbell’s claim as contrasting wholism with the value-blind *methodological* aspect of the reductionist approach – *not* the reductionist epistemology – then rather than serving as a counterexample, nutrigenomics exemplifies the contrast perfectly well.

As we saw in §I, Campbell is not the only critic of reductionist approaches to nutrition. For example, Fardet and Rock observe that “the reductionist approach is beginning to reach its limits. The risk of maintaining only a reductionist view […] is obsessing over details while losing sight of broader and more important issues” (Fardet and Rock 2014b, p. 432). Once again, despite being a holistic approach, nutrigenomics appears to be just the kind of science targeted by critics of reductionism. This makes sense given that the dispute is not only about holistic/reductionist epistemologies; it is often centered on methodologies ('obsessing over details') and values ('losing sight of important issues').

<u>The Methodologies of Wholistic Nutrition</u>

In *Whole* (2014), Campbell specifies three methodologies that are associated with his wholistic approach. First, he highlights *ecological studies*, which "survey and compare populations as they already exist, and see what they eat and how healthy they are" (Campbell 2014, p. 78). The results of such studies are limited to 'correlational' associations since they do not provide neat cause/effect relationships for nutrients and diseases, but instead reveal broad associations between diet and health. Campbell believes this feature of ecological studies fuels a reductionist bias against them; he explains that "because conclusions about specific causes cannot be made in this type of study, it is considered by reductionists to be a weak study design" (Campbell 2014, p. 79). I see no reason to deny that this is a weakness for some purposes, but we will see that this methodology has strengths as well.

The eponymous centerpiece of C&C's *The China Study* (2006) utilized an ecological design which established an association between consumption of animal products and the gamut of so-called 'diseases of affluence' (e.g., heart disease, diabetes, and obesity). The scale of the China Study is noteworthy; it included 65 counties, 130 villages, and 6,500 adults (C&C 2006, p. 39). Campbell reflects on his work on this project:

> I wanted to determine, from a wholistic perspective, whether a particular diet led to markedly better health outcomes than other diets. One way to do this was to study the people in an entire ecosystem—the rural population of China—who ate in a way markedly different from populations in the West. Using the rural

> population of China allowed us to consider a large-enough number and variety of lifestyle factors and health and disease conditions to see the big picture. [Campbell 2014, p. 80]

C&C point out that the diets in the China Study varied from "diets rich in plant-based foods to diets very rich in plant-based foods" whereas most Western studies focus on diets that range from "diets rich in animal-based foods to diets very rich in animal-based foods" (C&C 2006, p. 75). While the China Study had much more dietary variation than the typical Western studies, it still included mostly plant-based diets. One reason this fits with the wholistic approach is because it provided much needed data for comparison of plant-based diets (acquired in the China Study) and animal-based diets (readily available from Western studies). Many of the most startling results of the China Study (discussed at the end of this section) yielded from such comparisons: "The difference between rural Chinese diets and Western diets, and the ensuing disease patterns, is enormous. It was this distinction, as much as any other, that made this study so important" (*ibid*.).

Another methodology employed by wholistic approaches is *biomimicry*. In short, biomimicry studies diets of non-human animals in order to draw inferences about human nutrition; in particular, these studies often focus on our "nearest animal relatives—gorillas and chimps" (Campbell 2014, p. 81). This method, like the ecological method, is subject to reductionist criticism because it is not conducive to the study of isolated N/D relationships. It can, however, reveal broad associations between diet and health:

> […] just noticing that chimps and gorillas have strong bones and muscles while eating [plant-based diets] undercuts the notion that humans need lots of animal protein to grow and maintain muscle mass. And of course we can point to the largest land animals in the world, elephants and hippos, whose 100 percent

> plant-based diets don't seem to render them weak or scrawny. [Campbell 2014, p. 81]

These observations are not 'airtight,' since there are many differences between, e.g., gorillas and humans. However, such criticism forwarded by the reductionist is particularly weak, if not disingenuous, given that the vast majority of 'basic' reductionist research in nutrition can accurately be characterized as investigations into the effects of nutrition on *rodent* health. Of course, there are good reasons for using rodent models, and there may be insight to be found in comparisons involving other non-human animals as well.

The third wholistic methodology described by Campbell is *evolutionary biology*, "in which we examine our physiology and determine what our bodies have evolved to ingest and process" (Campbell 2014, p. 81). For example, the evolutionary record of the morphology of human teeth can be compared to modern day plant- and meat-eaters (*ibid*.). I explicate a version this methodology in more detail below, as evolutionary biology is central to the study of nutritional ecology.

It bears repeating that wholistic methodologies are not meant to replace reductionist methodologies. And, unlike the case of biotechnological and agroecological approaches, the pursuit of reductionist nutritional science need not undermine or destroy the conditions necessary for Campbell's wholistic approach. What is required is the realization that wholistic nutrition can reveal real and significant associations between diet and health that are beyond the reach of reductionist approaches. Despite their highly critical disposition toward reductionist science, C&C are clear that wholistic nutrition has its own limitations and is not the only legitimate approach (C&C 2006, p. 106).

Nutritional Ecology

Now we turn to nutritional ecology, another *value-sensitive* alternative to mainstream nutritional research. Nutritional ecology is a nascent field that is not yet clearly defined (Raubenheimer and Boggs 2009, p. 1).[37] In the most general terms, it studies the intersection of nutrition, ecology, and evolutionary biology; in more technical terms, it is the "trophic branch of functional ecology" (*ibid*.). The roots of nutritional ecology trace to classical studies of insect nutrition in the 1950's, in which scientists investigated connections between plant-eating insect behavior and the nutrient composition of plants (the nutritional environment). These studies, however, lacked a systematic way of dealing with the complexity involved (Raubenheimer, et al. 2009, p. 7).

Beginning in the 1990's, professors Stephen Simpson and David Raubenheimer (henceforth, S&R) introduced what they call the 'geometric framework' which has proven a powerful method for integrating and analyzing nutritional, ecological, and biological data. Since its introduction, the geometric framework (GF) has been sufficiently fruitful that S&R co-authored *The Nature of Nutrition: A Unifying Framework from Animal Adaptation to Human Obesity* (2012), which surveys advances made over the last 20 years and points to a variety of future directions for research. In their introduction, they motivate the need for research in nutritional ecology by appealing to the pervasive complexity of nutrition:

> Nutrition touches, links, and shapes all aspects of the biological world. It builds the components of organisms, and fuels the dynamic interactions between these components; it determines whether or not wild animals thrive, how their populations grow, decline, and evolve, and how assemblages of interacting

[37] It says something that as of Jan. 2015 there is no entry for "nutritional ecology" in Wikipedia.

species (ecological communities) and ecosystems are structured. [S&R 2012, loc. 129]

The Methodology of Nutritional Ecology: The Geometric Framework

The geometric framework is designed to analyze complex interrelationships between organisms' behavior, fitness, and nutritional environment (S&R 2012, Ch. 2). GF represents 'nutrient space' on a geometric graph with at least two axes; in the simplest case, S&R represent protein on the x-axis and carbohydrate on the y axis. Foods are then represented as lines, dubbed 'food rails,' according to their nutritional composition. For example, food that has equal parts protein and carbohydrate (a P:C ratio of 1:1) is represented as a food rail with a slope of 1. An organism's food consumption can be understood as a point on the food rail at any given time. It starts at the origin, having not ingested any food, and then as it feasts it moves up the food rail. When there is just one food available, the food rail exhausts the possible locations for the organism in nutrient space.

The picture gets more complicated (and more realistic) when more foods with different nutritional compositions are represented on a single graph. Suppose an experiment is conducted in which two foods are made available with P:C ratios of 2:1 and 1:2. The food rails will be represented as lines with slopes of 2 and ½. In this case, by eating some combination of the two foods, the test subject is 'free' to navigate the nutrient space on and between the food rails. Now we can see how nutritional ecology is able to conduct experiments using GF: "[…] if we know the nutrient needs of an animal and the nutritional composition of the foods available to it, we can make a prediction about which foods it would eat and which it should avoid, and if it did otherwise we would be justified in wondering why" (Simpson and Raubenheimer 2012, loc. 351).

One key insight of nutritional ecology is that organisms have evolved to *regulate* their intake of nutrients so that even when consuming a diverse variety of foods the level and distribution (ratio) of nutrients remains stable. S&R call the 'optimal' nutrient level and distribution for an organism its 'intake target,' which varies across species (S&R 2012, loc. 329). In order to determine intake targets, organisms are presented with a variety of food options with varying flavors and nutritional compositions (e.g., including some options with very high P:C ratios and some very low). Researchers have shown that a wide range of organisms will 'defend' an intake target (S&R 2012, loc. 546); that is, given different varieties of foods that enable 'free range' of the nutrient space, organisms of particular species consistently select diets with particular quantities and proportion of protein to carbohydrate. For example, caterpillars (*Spodoptera littoralis*) were shown to defend an intake target P:C ratio of 1.2:1 (S&R 2012, loc. 689).

In establishing intake targets, the variety of foods helps ensure that the results do not merely reflect an organism's gustatory preference. In order to show that intake targets result from an *evolutionary mechanism* of nutrient regulation, GF models incorporate measures of Darwinian fitness. By placing organisms on various restricted diets it is found that different P:C ratios are better or worse for traits like fertility, growth rate, and life-span. Using this information it has been shown that not only are intake targets consistent for species across food varieties, they correspond to evolutionarily significant outcomes (S&R 2012, loc. 505). For example, fruit flies live the longest on a relatively low P:C diet (1:16) and lay the most eggs on a relatively high P:C diet (1:4); in an experiment in which flies were able to move freely in nutrient space, they defended an intake target ratio of 1:4 (S&R 2012, loc. 1335).

The second major insight of GF is that organisms exhibit particular "rules of compromise" when the nutritional environment renders it impossible to reach the intake target (S&R 2012, loc. 440). Again dealing with the simple case of protein and carbohydrate, imagine a case where the only food available has a P:C ratio of 1:2 and an organism's intake target ratio is 1:1. The organism will have to decide whether to overeat carbohydrate in order to ingest enough protein, or to undereat protein in order to avoid excess carbohydrate. One study showed that in conditions like this domesticated cats consistently will overeat carbohydrate in order to gain more protein, but there is a level of carbohydrate they will not exceed (S&R 2012, loc. 2850). While rules of compromise vary even among closely related species, some rule or other is generally detectable (S&R 2012, loc. 448).

Perhaps the most fascinating of Simpson and Raubenheimer's studies are those which apply GF to human nutrition; in particular, within the context of obesity (S&R 2012, Ch. 10; see also Simpson, Batley, and Raubenheimer 2003). Humans, like cats, will overeat carbohydrate and fat in order to gain enough protein. When studied within its broader social and economic contexts, this simple fact has generated novel and promising lines of research on human obesity. I explore some of the implications of human beings' rule of compromise below in the discussion of the benefits of wholistic nutrition and nutritional ecology.

The Values of Wholistic Nutrition and Nutritional Ecology

It is worth pointing out that unlike agroecology, neither wholistic nutrition nor nutritional ecology *explicitly* incorporates values into their methodologies. In this way they are like value-blind sciences; their links with values are often subtle. Through exploring these links we will see that the methodologies of wholistic nutrition and nutritional ecology are not very well-suited for projects aimed at manifesting the values of modernity, but they are a good fit for projects aimed at manifesting the values of *sustainability* and *egalitarianism*.

Like Campbell, S&R do not generally deploy the language of values. Nonetheless, they do not shy away from discussing the ways that nutrition pervades value-laden contexts:

> [Nutrition] drives the affairs of humans, from individuals to global geopolitics. Food security and the burden of famine and disease from undernutrition have been pervasive in history, and recently overnutrition has emerged as a major cause of preventable death and disease. Climate change, population growth, urbanization, environmental degradation, and species extinctions all are in one way or another linked to the need for nutrients. In short, nutrients are the interconnecting threads in the web of life. [Simpson and Raubenheimer 2012, loc. 129]

I have characterized the values associated with wholistic nutrition and nutritional ecology quite broadly. This is by necessity, given the multifarious contexts in which nutrition is salient. In the discussion that follows I give examples of some of the specific values interwoven in the umbrella value categories of *sustainability* and *egalitarianism*.[38]

Nutrition is at the forefront in studies of ecology; for example, trophic interactions (food webs) are foundational in population ecology. Shifts in the food

[38] Henceforth I will use these terms to refer to the *sets* of values they encompass.

supply anywhere in the trophic web reverberate throughout the entire ecosystem. As some populations increase, others decline, and the cycle continues. Stable ecosystems maintain a sophisticated balance in which the supply of nutrients is generally reliable despite constant fluctuations. There is a sense in which sustainable nutrition is the foundation of a well-functioning ecosystem. But this just scratches the surface of *sustainability*. Fardet and Rock explain that "[…] nutrition inevitably involves sustainability issues at the levels of the organism (a healthy, long life), economics (affordable foods), society (the availability of foods for all), and environment (respect for other people, animals, and nature as a whole)" (Fardet and Rock 2014b, p. 440).

From the perspective of reductionist nutrition it is easy to overlook the importance of sustainability. There is a feeling that when we are (say) vitamin deficient, we can just churn out more pills. Technology, it seems, allows us to escape nature's constraints. On this view, humans are not a part of nature; instead, we transcend and *control* nature. But once we see how nutrition is inextricably woven into complex relations between humans, non-human animals, and the environment, it becomes difficult to think of humans as existing outside of nature. There is a tension between dissociating human beings from the rest of the world and recognizing that sustainable nutrition – our survival – depends on the trophic balance (or balances) spanning the globe. In taking sustainability as a core value, *value-sensitive* approaches to nutrition see human beings, like all the other inhabitants of Earth, as organisms requiring nutrients, as interdependent constituents of nature:

> As Meyer-Abich wrote, "it is not possible to avoid living at the expense of others' lives" (2005, p. 741). In other words, we cannot be healthy alone; our health depends on the well-being of others. [This] picture of human nutrition naturally includes promoting animal well-being […], environmental

> preservation, and the mitigation of the expense to others (i.e., over- and undernutrition disparities between countries). [Fardet and Rock 2014b, p. 440]

While some descriptions of this view can start to sound downright metaphysical (in the 'spiritual' sense of the word – the hackneyed version of 'being one with nature'), it nonetheless has important implications for scientific investigations in nutrition. For example, Campbell believes this perspective is linked specifically with the methodology of biomimicry:

> […] biomimicry reframes the issue of nutrition as one in which *humans are seen as one species among many*. Observing animals that resemble us can provide insight into diet in a way that observing human eating habits, which have been affected by human technologies from agriculture to refrigeration to processing, can't. [Campbell 2014, p. 81, my emphasis]

Biomimicry presents a particularly interesting case because it has been criticized for being 'technocentric.' Marshall and Lozeva forward this criticism in part because biomimicry has generated technological innovations that do not do anything to further 'eco-friendly' goals, and may even work against them (Marshall and Lozeva 2009). For example, it has been used to develop "undetectable surveillance cameras based on the compound eyes of insects" and nanotechnology that emulates DNA (Marshall and Lozeva 2009, p. 2). Indeed, where biomimicry is a principle for product design, it is often linked with the values of modernity. In its context of use in wholistic nutrition, however, biomimicry is linked with 'ecocentric' rather than technocentric values. According to Marshall and Lozeva, a fundamental contrast between the two value outlooks is that the technocentrism is linked with anthropocentrism, while ecocentrism is not (Marshall and Lozeva 2009, p. 3). Campbell's description makes it clear that the use of biomimicry in wholistic nutrition rejects anthropocentrism.

It is worth exploring a brief tangent here. Reductionist research in nutrition often relies on the fact that humans exhibit virtually the same metabolic processes as lab rats at the cellular level. Biomimicry relies on the fact that humans have similar nutritional needs to non-human animals with which we are closely related. In the first instance we assume that relevant aspects of our bodies are governed by the same physical and chemical laws as rats'; in the second, that humans' physiological nutritional needs derive from ancestors we share with extant (non-human) animals. It is not the case that one view is value-laden and the other is not; both views make assumptions which are better or worse fits with various value outlooks. Reductionist research on rodents happens not to go against the grain of predominant cultural values; it is a drop of water in the ocean which easily goes unnoticed. On the other hand, since biomimicry (in nutrition) rejects anthropocentrism, it is a bit more like a drop of oil that resists and is resisted by the surrounding waters. This disparity results from purely contingent facts about the way social and cultural value systems are organized.

I raise this point because it is salient to the discussion of *egalitarian* values. These values emphasize forms of equality in different nutritional contexts. We have just seen how biomimicry supports a kind of equality between humans and non-human animals by rejecting anthropocentrism. Additional examples of egalitarian values are found in nutritional ecology, which is linked with *intergenerational* and *social equality* in virtue of revealing nutritional factors that contribute to injustices between generations and social classes. Both wholistic nutrition and nutritional ecology also touch on issues of *global justice* by recognizing the role of nutrition in issues such as starvation and climate change that disproportionately affect non-Western ('third world' or 'developing') countries.

In cultures where the values of modernity and related values like anthropocentrism, individualism, and Western exceptionalism are the norm (e.g., the United States), alternative approaches that are associated with egalitarian values may take on the appearance of promoting an 'unscientific' bias. This view is reinforced by the value-free scientific identity and exacerbated by the fact that we tend to notice values when they contrast with prevailing outlooks (like noticing the drop of oil in water). Once we accept that *all* science is value-laden we remove a significant barrier to investigating alternatives which, though they are linked with different values, have the potential to generate scientific knowledge and perhaps even improve people's lives.

In the remainder of this section I continue to establish links between wholistic nutrition and nutritional ecology and the values of sustainability and egalitarianism. At the same time I explore some of the potential benefits of these approaches which are occluded or unattainable via value-blind approaches.

The Benefits of Value-sensitive Approaches to Nutrition

Recall that under the reign of value-blind nutritional science people have gained longer life-spans while seeing a reduction in 'healthy life years.' The value-blind approach also commonly leads to the problems of *oversimplification* and *confusion*. Alternative approaches have the opportunity to improve on the value-blind approach by developing better (e.g., more realistic and less confusing) guides to nutrition and leading to an increase in healthy life years. Wholistic nutrition and nutritional ecology have the potential to improve on both fronts.

By analyzing whole diets rather than individual nutrients, wholistic nutrition provides strong evidence that a "whole foods, plant-based diet" (WFPB) is among the best choices for promoting human health (C&C 2006; Campbell 2014). In addition to the China Study itself, C&C cite various wholistic investigations that revealed the benefits of the WFPB diet, including that it may have the potential to prevent and even reverse heart disease (C&C 2006, p. 127), reduce obesity (C&C 2006, p. 139), and treat diabetes (leading to reduced insulin requirements) (C&C 2006, p. 152), just to name a few examples. The potential benefits of the WFPB diet are staggering – so much so that it seems too good to be true. Among other things, it seems too simple a solution. (We would all feel a little bit better if it had been discovered using some sort of high powered microscope or computer program!) But there is a difference between the simplicity of the WFPB diet and the problem of oversimplification commonly associated with reductionist approaches.

In general, advice about diets generated by wholistic nutrition avoids the problems of oversimplification and confusion because it is not easily taken out of context. Or, to put it another way, the context in which it is studied is also the context in which it is applied. In reductionist science, the goal is to find relationships between nutrients and diseases, not to find nutrients that are 'good for you.' Nonetheless, this is often the message received by the public. Wholistic nutrition, on the other hand, is aimed at finding healthy diets that actually are, in general, good for you. This is not to say that there are no exceptions, but the difference between reductionist and wholistic research is not the prevalence of exceptions. The problem with saying 'vitamin E is good for you' is not that there are exceptions; the problem is that whether or not it is good for you is highly context-dependent and the research that purportedly supports such a claim is only conducted in a few specific contexts (many of which almost never obtain in real life). In contrast, the health benefits of the WFPB diet are supported by studies conducted in the same context in which those benefits obtain.

Although in the reductionist literature one can find conflicting evidence for nearly any specific claim about the health benefits (or harms) of the individual nutrients and foods involved, overall trends have largely substantiated the WFBP diet as a good choice (indeed, it would be concerning if they did not). Fardet and Boirie (2014) recently conducted an extensive review of over 300 meta-analyses of reductionist research in nutrition spanning the last 60 years. They focused on the most popular topics, which studied obesity and type-2 diabetes, as well as "diet related chronic diseases [including] cardiovascular diseases […], cancers, digestive diseases, mental illnesses, sarcopenia, and some skeletal, kidney, and liver diseases" (Fardet and Boirie 2014, p. 741). The results showed that whole grains and plant-based foods were nearly always either 'protective' or 'neutral' with respect to these diseases (Fardet and Boirie 2014, pp. 748, 750, and 758). The results for animal-based foods were mixed; in different studies they were associated with 'deleterious,' 'neutral,' and 'protective' effects (Fardet and Boirie 2014, pp. 751-752 and 759). On the basis of these results, Fardet and Boirie conclude with a bit of common sense advice: people who eat a lot of animal-based foods should reduce their consumption, and people who eat few plant-based foods should increase their consumption (Fardet and Boirie 2014, p. 759).

Not only does the WFPB diet promote health, it supports the values of sustainability and egalitarianism. In a passage in *The China Study*, Campbell laments the fact that by discounting the results of wholistic nutritional research we may miss out on the potential benefits related to these values. In particular, he mentions the potential of the WFPB diet to promote a sustainable environment and decrease the costs of healthcare (C&C 2006, p. 288). Campbell maintains that by "seeking to understand the larger context" investigations conducted in wholistic nutrition have the potential to realize outcomes that manifest values like sustainability and social equality, values which have not been well-served by reductionist science.

Nutritional Ecology and Human Obesity

Perhaps the most intriguing results found by S&R (and others) working with the geometric framework (GF) concern human obesity. Studies involving human subjects have shown that human beings, like all other animals, seem to have nutritional intake targets and rules of compromise. These studies focus on three main macronutrients: protein, carbohydrate, and fat. For the purposes at hand carbohydrate and fat are grouped together (being 'non-protein' energy), so I will refer to them simply as "C+F." Using the same methods typically deployed with GF, researchers found that the people in the studies defended an intake target (P:C+F ratio) of about 1:6 with protein being about 15% of total energy intake (Simpson, Batley, and Raubenheimer 2003, p., 137; S&R 2012, loc. 3159). When supplied with very low P:C+F diets (relative to the intake target), subjects were willing to overeat C+F in order to ingest enough protein; however, when supplied with very high P:C+F diets, subjects underate C+F instead of overeating protein (S&R 2012, loc. 3212).

S&R refer to this apparent rule of compromise, one which strongly regulates for protein, as the "protein leverage hypothesis" (S&R 2012, loc. 3235). S&R note the tantalizing implications of the protein leverage hypothesis: perhaps obesity can be reduced by producing more high protein, low C+F foods. Preliminary investigations have already shown that "the cost of providing the extra protein needed to reduce intake [is] substantially less than the cost of obesity" (S&R 2012, loc. 3396). It is worth pointing out that this simple solution would not likely have been conceived via the reductionist approach:

> Regarding dietary causes of obesity, most emphasis in research over the past 40 years or more has been on changing patterns of fat and carbohydrate consumption. In contrast, the role of protein has largely been ignored because it typically comprises only 15% of dietary energy and protein intake has remained relatively constant within and across populations throughout the development of the obesity epidemic. We have shown that, paradoxically, these are precisely the two conditions that provide protein with the leverage both to drive the obesity epidemic, through protein's effects on food intake, and potentially (with caveats) to assuage the obesity epidemic. [S&R 2012, loc. 3495]

The picture becomes even more interesting when S&R expand their analyses to include some of the social and economic factors related to obesity. Basic surveys of the foods available at supermarkets show that most products have low P:C+F ratios; moreover, the cheaper the product, the less protein and more C+F it tends to have (S&R 2012, loc. 3388). S&R surmise that the low P:C+F ratio of typical Western foods likely contributes to the level of obesity in Western countries because "where fat- and/or carbohydrate-rich foods are more accessible, more affordable, in greater variety, or more palatable than alternatives […] people [are] effectively trapped on a suboptimal diet" (S&R 2012, loc. 3336). Similarly, the especially low P:C+F ratio of cheap foods helps to explain why people in lower socio-economic classes are at an even greater risk of obesity. This last point shows how nutritional ecology may be well-suited to help further goals linked with social justice.

Finally, S&R's studies on obesity revealed information relevant to issues of intergenerational equity. It turns out that certain conditions associated with obesity actually serve to *increase* an individual's protein requirement. In a nutritional environment in which foods are low in protein and high in C+F, this leads to a positive feedback loop (S&R 2012, loc. 3411). Suppose one's protein requirement starts at the average of 15% of total energy; then, to eat enough protein, given the foods available, one must overeat C+F thereby gaining weight and exacerbating the conditions that drive up the protein requirement. As the protein requirement increases, the level at which one overeats C+F increases, and the cycle continues. This helps to explain why weight-loss can be so difficult, leading to obesity as a lifelong condition.

This point relates to intergenerational equity in light of developments in epigenetics, which have revealed that some metabolic traits (e.g., traits that influence protein requirements) are passed down through the generations (from both parents, and even grandparents) (S&R 2012, loc. 1829). Thus, the cycle of obesity has a tendency to persist across lifetimes *and* across generations.[39] The upshot is that basic choices we make about the crops we grow, the ways we process them into foods (e.g., adding savory flavors to low protein foods (S&R 2012, loc. 3470)), and how we price them have disproportionate deleterious effects on people of different social and economic classes, and on future generations.

Given the advances in scientific knowledge and the potential benefits that accompany wholistic nutrition and nutritional ecology, it is clearly a loss if they are ultimately overlooked or marginalized. Once we see that even mainstream nutritional

[39] There are even implications for the development of obesity across cultures. For example, traditional ocean-islander cultures live on higher protein diets (up to 30%), which means that they are especially prone to obesity when shifting to a low P:C+F Western diet (S&R 2012, loc. 3431).

science is linked with values, the idea that these alternatives are value-laden and therefore unscientific will no longer seem plausible. The best way forward is to adopt a plurality of approaches – including reductionist research, wholistic nutrition, and nutritional ecology – in order to gain as much knowledge as possible and to encourage investigations that are potentially significant for a broad range of values.

Conclusion

I hope it is now plainly evident that modern value-blind approaches to science are not value-free. We have seen that research in biotechnological agriculture (as in the Green Revolution), transgenics, and reductionist (and other value-blind) approaches to nutrition have strong and consistent links with the values of modernity. I have been calling them "value-blind" sciences, but, of course, it is not science itself that is blind to its connections with values. If it becomes more widely known in scientific communities (and the societies in which they are embedded) that science – all science – is value-laden, then not even decontextualizing approaches will require this moniker. As I have maintained throughout, these approaches have produced astounding advances in our knowledge of the world, as well as products and technologies that have saved and improved countless lives. But we also must recognize that these approaches have their limitations.

The good news is that there are alternative approaches, including feminist science, agroecology, wholistic nutrition, and nutritional ecology. Once we shed the value-free image of science, it will become clear that the value-ladeness of these alternatives does not render them 'unscientific.' This realization is crucial. It allows us to overcome the (self-imposed) limitations that have prevented us from exploring domains and possibilities that have long seemed beyond our grasp, including possibilities that can contribute to human flourishing. Thus, leaving the value-free ideal behind is not just an academic pursuit; it is a cognitive and moral imperative.

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