Okasha's Unintended Argument for Toolbox Theorizing

(Unabridged)

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Abstract

Okasha claims at the outset of *Evolution and the Levels of Selection* that the Price equation lays bare the fundamentals underlying all selection phenomena. However, the thoroughness of his subsequent analysis of multi-level selection theories leads him to abandon his fundamentalist commitments. At critical points he invokes cost benefit analyses that sometimes favors the Price approach and sometimes the contextual approach, sometimes favors MLS1 and sometimes MLS2. And although he doesn't acknowledge it, even the Price approach breaks down into a family of alternative equations that parse the causes in different ways, none of which is uniquely correct and none of which achieves the ultimate isolation of effects due to what Okasha believes are the fundamental causes. I argue that his book provides good reason to re-conceive our understanding of evolutionary theorizing in terms of a toolbox view (developed here) and to stop subjecting the analyses of evolutionary concepts to a universalist standard.

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Introduction

Readers might wonder whether philosophical work on the levels of selection has run its course, and perhaps it had, but inquiry into the issue has taken on new significance in biologists' theorizing about evolution, and with Samir Okasha's *Evolution and the Levels of Selection* (hereafter *ELS*), it will take on new significance in the philosophy of science. Okasha rigorously analyzes new developments in evolutionary theorizing, and in doing so advances philosophical inquiry on a number of issues ranging from the conceptual basics of the theory of natural selection to limitations of reductionism. Just as importantly, he also demonstrates how inquiry into these philosophical issues can shed light on the new scientific developments, including the extension of theorizing to higher scale evolution (e.g. species selection) and to evolution through major transitions (e.g. from unicellular to multicellular organisms). Recent developments in evolutionary theorizing go beyond extending the explanatory scope of multi-level selection; they also include the development of formalisms for theorizing about selection, and these formalisms have been largely overlooked and left unanalyzed by philosophers. One of these formalisms is based on the Price equation, and Okasha uses this equation to provide a clear and rigorous analysis of multi-level selection. By bringing important scientific developments to the forefront of philosophical inquiry, *ELS* both advances philosophical discussion and brings this discussion to bear on some of the most intriguing and important theorizing in contemporary biology. I doubt that Okasha will dispute what I have said thus far, but he might contest what I take to be his third major accomplishment, an accomplishment that unintentionally emerges from a tension at the core of his book.

The tension arises from Okasha's efforts to achieve two goals. First, he sets out to construct an abstract conceptualization of multi-level selection that can provide a unifying, fundamental basis for exploring the multiplicity of philosophical issues raised in debates about the levels of selection. Second, he seeks extraordinary rigor, which eventually leads him to conduct cost/benefit analyses of alternative conceptualizations, which in turn undermines his first goal. This tension raises an epistemological question concerning the proper ideal for scientific theorizing: should scientists be guided by a commitment to find fundamental concepts and principles sufficient for providing a universal and unified account of nature, or should theorizing be pragmatic, with scientists seeking piecemeal explanations by drawing upon some concepts, principles or models to explain some aspects of nature and alternatives to explain other aspects? Although Okasha embarks on the fundamentalist mission, his rigorous scrutiny often morphs into what I will call 'toolbox theorizing'.[1] Hence, his third accomplishment, the unintended one, is to offer a tacit argument for the toolbox view.

[1] Similar terminology has been used by others, including Maxwell (manuscript), Cartwright *et al*, (1995), Cartwright (1999), Suárez and Cartwright (2008) and Wimsatt (2007) to cover ideas about theorizing similar to those advanced here.

1. MLS1 and MLS2

Okasha's analysis of biologists' theorizing about how evolution proceeds through major transitions leads him to conclude that one model-type (MLS1) accounts for early stages of the process and another model-type (MLS2) accounts for later stages. This raises a subtle issue. We might assume that these model-types represent different "natural kinds" of causal processes. On this view, MLS1 models some processes, MLS2 others. Given a particular instance of multi-level selection, either an MLS1 model, or an MLS2 model, but not both, provides the correct causal account. We might say of one token process, 'this is MLS1' and of another 'this is MLS2'. But given that evolution is continuous, how does an evolutionary process transition from being MLS1 to being MLS2? By leap, or by gradual transition? Perhaps intermediate stages are both MLS1 and MLS2. But what does this mean? Are there actually two separate processes occurring at once? Or is it one process, of which some aspects can be modeled by MLS1 and other aspects by MLS2?

Fundamentalism favors the locutions 'this instance of selection is an MLS1 process', 'this instance is an MLS2 process' and 'this instance includes two different processes, an MLS1 process and an MLS2 process'. But toolbox theorists would say 'the first example can be modeled as a MLS1 process, and not as an MLS2 process', 'the second example can be modeled as a MLS2 process, and not as an MLS1 process', and 'the third example can be modeled as an MLS1 process and as an MLS2 process, and depending on your explanatory interests, you should use one model, the other model, or both to account for the example.' In short, fundamentalists will declare 'this *is* an MLS1 process' whereas toolbox theorists will claim 'this *can be modeled as* an MLS1 process'.

Both fundamentalist and toolbox theorist are realists, but their metaphysical pretensions and methodologies differ. While fundamentalists seek the universally correct theoretical account for each natural kind of process, regardless of explanatory interests *about those natural kinds*, toolbox theorists seek true theoretical accounts that best address particular interests. Okasha often sounds like a fundamental theorist, distinguishing natural kinds of processes each of which can be comprehensively explained in terms of its fundamentals:

> MLS1 and MLS2 are distinct processes that can occur in nature; whether either occurs in a particular case is a matter of objective fact. But our explanatory interests may determine which process we wish to model, and thus which definition of collective fitness we choose. (p. 59)

Here, it appears that Okasha is saying that some examples of selection might involve two separate ("distinct") processes, and interests come in at the stage of deciding which process to model, not which aspect of the process to model. But in the next sentence, he uses the term 'aspect' instead of 'process':

> Any conventionalism here is of the innocuous sort that arises because all scientific investigations must focus on some aspects of nature at the expense of others. (p. 59)

This sentence evokes the toolbox view according to which theorizing about messy parts of the world involves developing a toolbox of theoretical concepts and models that can be used to explain different aspects of messy situations. (Readers might object that I am drawing too much from Okasha's language of 'aspects', but my interpretation is supported by the fact that he offers no argument for the idea that there are two objectively separate and distinct processes. Hence, even if Okasha unambiguously held this idea, he offers no argument in favor of it.)

Fundamentalist and toolbox theorists disagree about the point at which interests enter. Fundamentalists assume that the world is made up of natural kinds of processes, and that there is a one-to-one correspondence between a token process and the true model of that process: 'this particular process of selection simply *is* MLS1'. Toolbox theorists are open to the possibility of a messy world, one in which for a given token process, some concepts and models might be useful for constructing correct accounts of some aspects of this process, other concepts and models might be best for constructing correct accounts of other aspects. Okasha does not offer any argument in favor of the fundamentalist interpretation of MLS1/MLS2 and it is worth noting that his own description of the difference between MLS1 and MLS2 models sounds like toolbox theorizing:

> The key issue is whether the particles [e.g. organisms] or the collectives [e.g. groups of organisms] (or both) constitute the 'focal' level. Are we interested in the frequency of different particle-types in the overall population of particles, which so happens to be subdivided into collectives? If so, then the particles are the focal units; the collectives are in effect part of the environment. Alternatively, we may be interested in the collectives as evolving units in their own right, not just as part of the particles' environment. If so, we will wish to track the changing frequency of different particle-types *and* collective-types. Following Damuth and Heisler (1988), I refer to the first approach as multi-level selection 1 (MLS1), the second as multi-level selection 2 (MLS2). (p. 56)

2. Price versus Contextual Analyses

Fundamentalist might acknowledge that the toolbox view applies to theorizing at the surface, that scientists employ a toolbox of higher-level theoretical concepts. They could nevertheless insist that these concepts can ultimately be reduced to fundamental concepts that apply universally. On this view, it might be misleading to say, 'this process is MLS1' (rather than 'this process can be modeled as MLS1'), but it would be perfectly accurate to say 'this process *is* natural selection'. This fundamentalist move is consistent with the structure of *ELS*. Okasha begins his book by formulating a highly abstract conception of natural selection, which purportedly "lays bare the essential components of evolution by natural selection in a highly revealing way." (p. 19) His early analysis is guided by a quest for universal generality, and he repeatedly rejects conceptual proposals from the philosophical literature because they are not fully general. He argues that since the biological hierarchy (organelles, cells, organisms, species, etc.) itself evolved, the fundamental characterization of natural selection "cannot refer to highly evolved features, of either organisms or genetic systems, on pain of an inevitable loss of generality" (p. 17). This quest for universality leads him to Price's equation, of which he claims "unlike most formal descriptions of the evolutionary process, it rests on no contingent biological assumptions, so always holds true" (p. 19). At the beginning of his book, Okasha claims that the Price formalism "subsumes all more specific models as special cases" (p. 3). But there is a tension between this claim and his subsequent examination of contextual analysis, an alternative to the Price formalism:

> The tension between contextual analysis and the Price approach arises because they constitute non-equivalent ways of partitioning the total evolutionary change into components corresponding to each level of selection. (p. 93)
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> ...
>
> The Price and contextual partitions are both correct as *statistical* decompositions of the total change, for both of the above equations are true; but at most one of them can constitute a correct *causal* decomposition. In other words, presuming there is a 'fact of the matter' about how much of the total change is attributable to selection at each level, at most one of the equations captures that fact. For the two equations will always divide up the total change differently; and in certain cases they will disagree about whether there is *any* component of selection at one of the levels. So they embody conflicting conceptions of multi-level selection. (p. 94)

So far, so good for the fundamentalist. The fundamentals of natural selection, according to Okasha's first two chapters, are set out in the Price equation, and this equation provides the uniquely correct decomposition of causes for all cases of natural selection. Presumably, any alternative decomposition of causes must be false. But later in the book, when Okasha carefully examines cases of MLS1 that involve cross-level by-products, his meticulous analysis reveals that the situation is far messier than the fundamental theorist would like.

On the one hand, Okasha writes,

> "Contextual analysis seems superior on theoretical grounds. ... Moreover, in the case where a particle's fitness depends only on its own character, contextual analysis generates the intuitively correct answer ..." (p. 94)

On the other hand,

> However, the contextual approach has one implication that some theorists find deeply counter-intuitive. ... in the scenario known as 'soft selection', contextual analysis detects a component of selection at the collective level, which is intuitively wrong. (p. 95)

Okasha explains that this ambivalence arises because "there are two requirements that, pre-theoretically, we would like satisfied in order for there to be selection at the collective level" and the Price approach satisfies one and contextual analysis satisfies the other. He mentions that this might make "some sort of conventionalism" sound appealing, but he urges us to resist the temptation.

A brief digression is in order. Discussions of conventionalism often misdirect philosophical debate about the levels of selection by equivocating between different senses in which a decision might be "conventional". Reichenbach's (1938) discussion is helpful here. He distinguished among (a) decisions that effect the content and truth character of the resulting science; (b) decisions that effect the content but not truth character; (c) decisions that effect neither the content nor truth character. Philosophical discussions overlook possibility (b), thereby imposing a false dichotomy between (a) and (c), and hence a false dichotomy between "conventionalism" and realism—e.g. see Lloyd (2005), Sober (forthcoming) and Okasha (forthcoming). For further critique of this false dichotomizing, see Waters (2005).

After asking us to resist "conventionalism", Okasha, concludes:

> "In my view, the contextual approach is on balance preferable, despite the violation of the Lewontin conditions that it entails; for the Price approach cannot deal satisfactorily with cross-level by-products." (p. 99)[2]

Okasha is suggesting that in the MLS1 model of cross-level by-products, a theoretical alternative to the Price equation provides the uniquely correct decomposition of causes, if any theory does. This implies that the Price analysis provides an incorrect causal decomposition, and leads Okasha to revisit a conceptual question from the second chapter (when he was adhering to the universalist perspective of a fundamental theorist):

> "If the argument of this section—that the contextual approach to MLS1 is superior to the Price approach—is correct, this suggests that [the

[2] Okasha adds, "But the situation is not clear-cut. For as we shall see when we discuss 'genic selection' in Chapter 5, there are multi-level scenarios of the MLS1 variety which the contextual approach cannot satisfactorily handle, but the Price approach can." Yet further down on the same page he says that in one important respect, "the contextual approach provides a representation of multi-level selection that is inherently more general." (p. 99)

> strict nesting requirement for part–whole relations] is not well-motivated." (p. 99)

I am not quarrelling with the cost/benefit analysis of cross-level by-products that Okasha conducts in Chapter 3. I am pointing out that this analysis differs in kind from the fundamentalist analysis he conducted in his first two chapters. Moreover, the conclusion he draws about MLS1 models of cross-level by-products contradicts his earlier claim that the Price equation "subsumes all more specific models as special cases" (p. 3).

3. Price Equation 1.2 versus Price Equation 1.3

It turns out the multiplicity of conceptualizations reaches into the very heart of Okasha's analysis: the Price formalism. Since Okasha explicitly denies this, it is necessary to delve into the details. Evolutionary theorists have derived different formulations of the Price equation. Okasha scrutinizes two of them, the standard formulation and an alternative. The standard formulation is expressed as follows (I call this equation '1.2', even though it is the first formula in this paper):

$$\Delta \bar{z} = \mathrm{Cov}\,(\omega, z) + E_w(\Delta z) \qquad 1.2$$

$\Delta \bar{z}$ refers to change in the average value of character z from one generation to the next. Cov (ω, z) designates the covariance between fitness values (ω) and character values (z) in the parental population. This value is high if fitness and character values are highly correlated (e.g. if organisms with higher z values have more offspring than do organisms with lower z values). If transmission is perfect, that is if offspring inherit the exact z value of their parents, then the covariance of fitness and character values would alone determine the change in average z value from one generation to the next. But transmission is often biased, that is the z value of an offspring often differs from the z value of its parent (following Okasha, I assume asexual reproduction[3]). The second addend on the right-hand side (RHS) of equation 1.2, $E_w(\Delta z)$, concerns the effects of transmission bias, which can vary if transmission bias itself is correlated with z value (if transmission bias is greater among organisms that produce more offspring [higher w] than among organisms that produce fewer offspring, then the overall change in z [$\Delta \bar{z}$] will be less). So $E_w(\Delta z)$ is the fitness-weighted value of the transmission bias. Biologists often interpret equation 1.2 as a decomposition of total evolutionary change into the effects due to selection (the first addend on the RHS) and the effects due to transmission bias (the second addend).

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[3] Okasha makes additional simplifying assumptions which obscure complexities that would further undermine his fundamentalist efforts, but space limitations prevent me from substantiating this point.

But Okasha digs deeper and examines an alternative formulation of the Price equation, which he labels '1.3':

$$\Delta \bar{z} = \text{Cov}(\omega, z') + E(\Delta z) \qquad 1.3$$

Cov (ω, z´) refers to the covariance between fitness values of parents (ω) and character values in the offspring (z´). E(Δz) is the un-weighted expected value of transmission bias. Equation 1.3 offers an alternative decomposition of the effects due to selection [Cov (ω, z´) instead of Cov (ω, z)] and the effects due to transmission bias [E(Δz) instead of $E_w(\Delta z)$].

Okasha then asks:

> Does equation (1.2) or (1.3) provide the correct decomposition of the total change? This question is worth thinking about both for its intrinsic interest and because the same *type* of question will arise again in later chapters...Equations (1.2) and (1.3) both provide correct *statistical* decompositions of $\Delta \bar{z}$, for both equations hold true by definition, but it still makes sense to ask which if either provides the correct *causal* decomposition.

Notice that Okasha writes '*the* correct' [my emphasis]. From the perspective of fundamental theorizing, the goal is to identify *the* equation that correctly decomposes the ultimate causes. But, as Okasha readily admits, it is not always possible to decompose the effects of different causes:

> In general, causal decomposition is only possible where the causal factors make 'separable' contributions to the overall effect...This will not always be the case. To borrow an example of Sober's, an individual's height is affected by both their genes and their nutritional intake, but we cannot ask how many centimetres are due to genes and how many to nutrition; this question makes no sense (Sober 1988). By contrast, in classical mechanics, if an object is acted on by two or more physical forces, then the overall effect, that is, the net acceleration, *can* be decomposed into components corresponding to each force, using standard vector analysis. So causal decomposition is sometimes but not always possible. (p. 27).

He continues:

> It is common in biology to regard the total evolutionary change in a population as the net result of a number of different causal factors or 'forces', of which natural selection is one...Others include migration, drift, and transmission bias. This raises the question: is causal decomposition possible in this case? Can the total evolutionary change be divided into distinct components, each corresponding to a different causal factor? (pp. 27–8)

The question of whether causes can be decomposed is indeed an empirical one. But five critical points are in order. First, Okasha's discussion equivocates between *causes* and *effects of causes*. Second, the pertinent issue, as Okasha himself sets out (pp. 25–6), is whether the addends of a Price equation separates change "due to" natural selection from change "due to" transmission bias. Third, Sober's height example is irrelevant because selection involves actual difference making. It is not z (the value of a single individual), or $\bar{z}$ (the average value of z in a generation) being explained by natural selection; rather it is $\Delta \bar{z}$, the *difference* in average value of z from one generation to the next that is being explained. Although it makes no sense to ask how many centimeters of an individual's height are due to genes and how many are due to nutrition, it can make sense to ask whether a difference in height between two individuals is due to differences in their genes, differences in their nutrition, or both. Likewise, although it makes no sense to ask how many centimeters of the offspring's average height are due to genes and how many are due to nutrition, it can make sense to ask how much of a difference between the average height of parents and the average height of their offspring is due to differences in genes and how much is due to differences in nutrition. E.g. if parents and offspring are genetically identical, but offspring are taller than parents due to superior nutrition, then all the *actual difference* in height is due to *differences* in nutrition—see Waters (2007) for discussion of actual difference making. Fourth, Okasha does not consider the possibility that the effects of two causes might be partially, though not completely, decomposable. Finally, Okasha's discussion on pages 27–8 overlooks the difference between 'causal interactionism' and 'statistical interaction'. Standard analysis of variance (ANOVA) provides partial causal decompositions by incorporating an additional addend that designates the effects due to the statistical interaction of distinct causes.[4]

Contrary to what Okasha's discussion might suggest, what determines whether the effects of different causes can be completely decomposed is not whether the causes interact in a process, but whether differences in each of the respective causes bring about uniform differences in the value of the effect variable regardless of the actual values of other causes. The pertinent question about whether the effects of selection and transmission bias can be completely decomposed concerns whether differences in fitness values will have the same effect on evolutionary change regardless of the degree of transmission bias (and vice versa). And the answer is unequivocally no.[5] In equation 1.2, the difference

[4] This point is essential for understanding of the analysis of variance (ANOVA). A critical distinction, which Okasha's discussion on pages 27–8 overlooks, involves the difference between 'causal interactionism' and 'statistical interaction'. ANOVA provides partial causal decompositions by incorporating an addend that designates the effects due to the statistical interaction of distinct causes. See Tabery (2008) for an illuminating discussion of the concept of statistical interaction and Sesardic (2005) for a discussion of the difference between causal interactionism and statistical interaction.

[5] Okasha (forthcoming) disagrees. But even if it turned out that statistical interaction is equal to zero, this would be a contingent result of evolution. Hence, Okasha's claim that the Price

fitness makes is clearly dependent on the value of transmission bias (since the second addend is weighted by fitness). In equation 1.3, the difference fitness makes is also dependent on the value of transmission bias because the first addend involves a covariance between the fitness of parents and the trait value of their *offspring*, the latter of which will depend on the value of transmission bias. Both equations show that the same differences in fitness values will have different effects depending on the value of transmission bias. Hence, neither equation separates differences due to selection from differences due to transmission bias.

Okasha, however, reaches a different conclusion. He argues that equation 1.3 provides "the correct" causal decomposition and that equation 1.2 does not. How does he reach this conclusion? He begins by asking, 'which equation sequesters into one addend what would happen if selection were reduced to zero?' He answers that the effect of eliminating selection is limited to the first addend of equation 1.3, but is distributed across both addends in 1.2. One problem with his argument for this answer is that it crucially depends on metaphysical speculations about which of two modifications would produce counterfactual scenarios most similar to an actual selection situation: (a) the scenario generated by equalizing the fitnesses of all entities but leaving "everything else" unchanged (presumably the environment would need to be changed to equalize fitness); (b) the scenario generated by equalizing character values, but leaving everything else, including differential reproductive success unchanged (presumably differences in reproductive success would be due to chance).

The difference in outcome brought about by reducing the effects of natural selection the first way (a) is isolated in the first addend in equation 1.3 and distributed across both addends in equation 1.2. But the difference in outcome brought about by reducing the effects of natural selection to zero in the second way (b) is isolated in the first addend in equation 1.2 and distributed across both addends in equation 1.3. Okasha believes that the second scenario is "quite remote from the actual world" and concludes that therefore equation 1.3, not equation 1.2 provides "the correct causal decomposition of $\Delta\bar{z}$" (p. 30). I suspect Okasha's intuition about the remoteness of these counterfactual scenarios from "the actual world", rests not on a pipeline to metaphysical truth, but on tacit considerations about practicalities (it is easier to manipulate the actual world to produce situations of the first type (as evidenced by artificial selection) than scenarios of the second type. But there is a there is a separate problem with his argument.

Okasha assumes that the uniquely correct parsing is achieved by whichever equation provides a single addend answer to the question "what would happen if the effects of natural selection were entirely eliminated". But what if we asked instead "what would happen if natural selection was reduced somewhat or

formalism "rests on no contingent biological assumptions" (p. 19) is compromised in that it cannot completely decompose selection and transmission bias in a principled way.

increased somewhat? Would the differences still be isolated into a single addend?" Or, what if we asked, "what if transmission bias were increased or decreased, or eliminated altogether? Which equation would sequester the change in outcome due to changing transmission bias?" Consider the question, "what if transmission bias were decreased, would the difference in effect be isolated in a single addend (on the RHS) of one of the equations?" The answer, if we follow a line of reasoning parallel to Okasha's, is yes, but this time it is isolated in a single addend of equation 1.2, and not in a single addend of equation 1.3.

So which equation gives "the correct causal decomposition" of the effects of fitness and transmission bias in cases of selection? There is no answer. One equation does a better job of isolating the effects of eliminating selection (granting Okasha's metaphysical hunches), the other does a better job of isolating the effects due to differences in transmission bias. The process of evolution by selection involves the causal and statistical interaction of fitness and transmission bias. Okasha's quest for the uniquely correct formal framework for decomposing these causes cannot accomplish the task even in the simplest cases.

I suggest that we think of the different formulations of the Price equations as tools in a box that can be drawn upon to answer different questions. If biologists want to know what would happen if selection were eliminated by changing the environment, then equation 1.3 provides a cleaner parsing of causes (cleaner but not perfect). If they want to know what would happen if transmission bias were increased, then equation 1.2 provides a cleaner parsing. There is no uniquely correct and exhaustive decomposition of the effects of different causes on evolutionary change. There is no decomposition of fundamental causes that cuts nature at its joints.[6] Although neither 1.2 nor 1.3 provides the complete parsing that Okasha seeks, each provides informative *partial* causal decompositions. Depending on what question one asks, one partial decomposition might provide a cleaner parsing of causes than the other. It would be a mistake for theorists to anoint one equation as fundamentally correct and the other as mistaken. Both equations belong in the theoreticians' toolbox.

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[6] In his reply (Okasha forthcoming), Okasha argues that the equation generated by adding a third addend for statistical interaction does give a complete decomposition. Following Okasha, I will call this equation '1.4'. But 1.4 does not completely parse what he takes to be the fundamental causes, selection and transmission bias. What it parses are the *additive* effects of selection, the *additive* effects of transmission bias, and the *non-additive* effects of selection and transmission bias. So, his suggestion that this three way parsing cuts nature at its joints by completely isolating the effects of individual fundamental causes is not accurate. Moreover, it is unclear why one parsing of effects into additive and non-additive addends (1.4) is the uniquely "correct" causal decomposition whereas other parsings of effects into additive and non-additive addends (e.g. 1.2 or 1.3) must be incorrect. Once Okasha admits that there is no way to completely decompose effects into additive components without making empirical assumptions concerning what evolution has produced (ecological niches and transmission systems that do not give rise to statistical interaction between selection and transmission bias), he has sold the fundamentalist farm.

4. Conclusion

Although Okasha claims at the outset that the Price equation lays bare the fundamentals underlying all selection phenomena, the rigor of his subsequent analysis and thoroughness of his probing leads him to abandon his fundamentalist commitments. At critical points he invokes cost benefit analyses that sometimes favor the Price approach and sometimes the contextual approach, sometimes favor MLS1 and sometimes MLS2. And although he doesn't acknowledge it, even the Price approach breaks down into a family of alternative equations that parse the causes in different ways, none of which is uniquely correct and none of which achieves the ultimate isolation of effects due to what Okasha believes are the fundamental causes. While I doubt that Okasha will agree, his work provides good reason to re-conceive our understanding of evolutionary theorizing in terms of the toolbox view and to stop subjecting the analyses of scientific concepts to the universalist standard.

The fundamentalist and toolbox views can be summarized as follows:

> 1. Fundamentalist view: the aim of scientific theorizing is to identify the fundamental causal relationships that are universally responsible for a domain of processes. Achieving this aim entails articulating the fundamental theoretical concepts and causal principles that can provide a basis for constructing models that decompose the fundamental causes of each and every process in the uniquely correct way. Proponents of this view stress the idea that there is, of course, just one way the world actually is, and the aim of theorizing is to describe, in a principled manner, the one way it actually is.
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> 2. Toolbox view: the aim of scientific theorizing is to construct causal models that explain aspects of the processes in the domain and that provide a basis for manipulation those processes. Achieving this aim entails articulating a multiplicity of theoretical concepts and causal principles that can be drawn upon to construct models that might decompose the causes of some processes in a multiplicity of ways. In such cases, some concepts and models offer the best account of some aspects of the given process, others provide the best account of other aspects.

The toolbox view does not deny that there is just one way the world actually is; it merely denies the fundamentalist assumption that there must be a single way to formulate the basic concepts and principles that provides the uniquely correct and *comprehensive* account of the world.

Many philosophers are committed to the fundamentalist view of scientific theorizing. But why should we be committed to fundamentalism (or toolbox theorizing) *a priori*? Why not leave the question open to empirical considerations? Instead of proceeding from *a priori* metaphysics ('there *must* be a fundamental description'), why not proceed from empirically informed epistemology? Perhaps, given the messiness of the world the ideal theory turns out to be more like a toolbox than a fundamental theory. Different tools in the box might be employed to account for different aspects of the causal complexity. Perhaps what philosophers have *assumed* is a single concept, such as fitness, consists of a family of somewhat different concepts each useful for theorizing about somewhat different aspects, parts, or scales of entangled evolutionary processes.[7] Why *assume* that there must be some single way to decompose evolutionary causes that will suffice to answer all causal questions about evolution? Fundamentalists might answer this question with one of their own: 'why *not* assume that there must be a single way?' The answer to this question can be found in the results of Okasha's meticulous analysis.

References

Cartwright, Nancy D. (1999) The Dappled World: A Study of the Boundaries of Science. Cambridge: Cambridge University Press.

Cartwright, N., Shomar, T., & Suárez, M. (1995). The tool box of science. In W. Herfel, W. Krajewski, I. Niiniluoto, & R. Wojcicki (Eds.), Theories and models in scientific processes (pp. 137–149). Amsterdam: Rodopi.

Damuth J. and Heisler I. L. (1988) "Alternative Formations of Multi-level Selection", *Biology and Philosophy* 3: 407–430.

Kellert, Stephen, Helen Longino, and C. Kenneth Waters (2006) "The Pluralist Stance" in S. Kellert, H. Longino, and C. K. Waters *Scientific Pluralism, Volume XIX of the Minnesota Studies in the Philosophy of Science*, Minneapolis, MN: University of Minnesota Press, pp. 7–29.

Lloyd, E.A. (2005) "Why the Gene will not Return." *Philosophy of Science,* 72: 287–310.

Maxwell, Grover (manuscript) "Toolbox Theorizing" MCPS Maxwell Archive.

Okasha, Samir (forthcoming) "Reply to Sober and Waters", *Philosophy and Phenomenological Research*.

Okasha, Samir (2006) *Evolution and the Levels of Selection.* Oxford: Oxford University Press.

Reichenbach, Hans (1938) *Experience and Prediction*. Chicago: University of Chicago Press.

Sesardic, Neven (2005) *Making Sense of Heritability*, Cambridge: Cambridge University Press.

[7] This point is developed in a somewhat different way in Kellert, Longino, and Waters (2006).

Sober, Elliott (forthcoming) "Realism, Conventionalism, and Causal Decomposition in Units of Selection: Reflections on Samir Okasha's *Evolution and the Levels of Selection*" *Philosophy and Phenomenological Research*.

—— (1988) "Apportioning Causal Responsibility" *Journal of Philosophy* 85: 303–318

Suárez, Mauricio and Nancy Cartwright (2008) "Theories: Tools versus models", *Studies in History and Philosophy of Modern Physics 39*: 62-81.

Tabery, James (2008) "R. A. Fisher, Lancelot Hogben, and the Origin*(s)* of Genotype-Environment Interaction", *Journal of the History of Biology* 41(4): 717–761.

Wimsatt, William C. (2007) *Re-Engineering Philosophy for Limited Beings: Piecewise Approximations to Reality*. Cambridge: Harvard University Press.

Waters, C. Kenneth (forthcoming) "Okasha's Unintended Argument for Toolbox Theorizing", *Philosophy and Phenomenological Research.*

—— (2007) Causes that Make a Difference "Causes that Make a Difference", *The Journal of Philosophy*. Volume CIV, Number 11: 551–579.

—— (2005) "Why Genic and Multi-level Selection Theories are Here to Stay", *Philosophy of Science* 72(2): 311–33.