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Causal Explanations of Historical Trends

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Abstract

Philosophers interested in historical explanation have tended to focus on causal explanations of particular events, states of affairs, or observable traces. Yet researchers working in fields ranging from paleontology and evolutionary biology to climate science and economics seek to document and explain the occurrence of historical, population-level trends, where a trend is a persistent, directional change in some state variable. Examples of such trends include everything from evolutionary size increase (Cope's rule), to changes of gene frequencies in an evolving population, to global warming. This chapter explores the idea that explanations of historical trends are typically causal explanations. Woodward's interventionist theory treats causation as a relation between variables and so lends itself readily to the idea that trends can be causes and effects. A small extension of Woodward's theory can help illuminate cases in which scientists talk of one trend as being the cause of another. However, paleontologists often explain trends by claiming that they are passive, that is, that they involve a random walk away from a fixed boundary in the state space. This type of explanation of an historical trend does not invoke any causes in the interventionist sense, which suggests that some explanations of historical trends might not be causal explanations.

1. Introduction

Researchers in a variety of different fields—from paleontology to climate science—investigate historical trends. A trend is any persistent, directional change in some interesting variable or measure. The first challenge of such research is to document a trend, or to establish that it is real. The next step is then to try to explain it. In practice, scientists often treat historical trends as standing in causal relationships, and they often try to develop and test causal explanations of those trends. In what follows, I begin (in section 2) by describing one example of this kind of scientific research. The example, drawn from vertebrate paleontology, is one in which scientists seem to treat one morphological trend as the cause of another. Section 3 sets the stage for further discussion by offering a brief exploration of the metaphysics of historical trends. Section 4 develops an initially promising interventionist proposal for making sense of causal claims about historical trends, based largely on the work of Woodward (2003). However, in section 5, I turn to examine a problem case. Paleontologists distinguish between active and passive evolutionary trends (McShea 1994), and passive trends pose a special challenge for the interventionist approach.

In what follows, I focus mainly on examples from paleontology. Paleontology makes for an especially rich supply of case studies, in part because paleontologists have focused so explicitly in recent decades on questions about the relationship between patterns and trends, on the one hand, and underlying processes on the other (see Kemp 1999, Sepkoski 2012, and Turner 2011 for introductions to this work.) However, it is worth noting that researchers working in many different fields study historical trends, and the three basic questions they ask are usually the same: (1) Which trends are the "real" ones? (2) What are the causes of those trends? (3) What are their effects? Consider the following list of putative historical trends:

Macroevolutionary body size increase (Cope's rule)
Directional changes in gene (or trait) frequencies in a population
Grade inflation
Falling housing prices
Rising unemployment
Increasing economic inequality
Global warming
Rising sea levels
Increasing atmospheric CO2, in ppm
Human population growth
Declining fertility rates
Biodiversity loss
Increasing scientific knowledge
Etc.

This list makes it clear, I hope, that historical trends loom large in many different fields, and in many aspects of life. The study of historical trends is one distinctively historical variety of science, and one that gets left out of some recent characterizations of historical science (e.g., Cleland 2002, 2011). Researchers in both the natural and the social sciences often work on historical trends, and their work is sometimes highly relevant to policy. What's more, normative judgments about progress are typically anchored to empirical claims about directional historical trends. As Elliott Sober (1994) once put it: "progress = directional change + vaues." In some cases (e.g., with respect to global climate change), claims about the causes and effects of historical trends are politically controversial.

I mention these other cases only to underscore the importance of the scientific study of historical trends, and to suggest that there are some interesting methodological commonalities between paleontology and other seemingly unrelated areas of empirical research, from economics to climate science. This raises the stakes somewhat with respect to the guiding question of this paper—namely, how should we understand the meaning of causal claims about trends?

The spirit of this paper is largely exploratory. The plan is to begin with Woodward's interventionism and see whether it can help us to think through some examples from paleontology. The results of this exploration are mixed (with some success, but also some problems), but they are also preliminary. The question how to understand causal claims about trends is one that needs further work.

2. Morphological trends in the fossil record

Paleontologists often begin their work by seeking to document trends and patterns in the fossil record. For example, O'Keefe and Carrano (2005) identified two trends in the morphology of plesiosaurs, one group of marine reptiles that lived from about 220 until 65 million years ago. The scientists looked at 41 plesiosaur specimens representing a total of 28 distinct taxa. Working with a phylogeny for plesiosaurs that O'Keefe (2001) had developed a few years earlier, they focused on two interesting measures: body size, and the ratio of head to neck length (hereafter, the HN ratio). They documented two noteworthy trends in plesiosaur morphology. The first of these is a trend toward larger overall body size, consistent with the idea that paleontologists have come to know as "Cope's rule," after the nineteenth century American paleontologist E.D. Cope.

The reason for focusing on the HN ratio is that it may serve as a good proxy variable for trophic specialization. Scientists have long known that plesiosaurs evolved two different body types. The first of these (which were for a long time classified as the pliosauridae) had relatively long heads and short necks. The second (traditionally classified as the plesiosauridae) had long, snake-like necks and relatively short heads. The HN ratio presumably tells us something about the size and type of prey that the animals could have eaten, and it's plausible to think that the two body types represent adaptations to different kinds of food sources. When O'Keefe and Carrano looked for trends in HN ratio in the plesiosaurs, they found what they characterized as a trend toward "increasing trophic specialization." Over time, the HN ratios tended to get more extreme.

Rather than investigating the causes of size increase and trophic specialization in plesiosaurs, O'Keefe and Carrano focus more on the relationship between these trends and other quantifiable morphological trends. They looked at six other features that have to do with the plesiosaur locomotor system:

- Scapula length
- Coracoid length
- Ischium length
- Pubis length
- Humerus length
- Femur length

The scapula and coracoid bones are associated with the shoulder joint, whereas the pubis and ischium bones are associated with the hip joint. Studying the relationships among these variables can tell us something about how the plesiosaurs might have used their four flippers to propel themselves through the water. Any changes in the relationships among these variables would reflect changes in the biomechanics of plesiosaur swimming. For example, O'Keefe and Carrano note that over time, the girdle components (including both the shoulder girdle, consisting of the scapula and coracoid bones, and the pelvic girdle, consisting of the pubis and ischium) tended to get relatively longer, while the bones in the limbs tended to get relatively shorter. They interpret this change as an allometric consequence of the evolution of larger body size. Setting aside the biomechanical details (see their 2005, pp. 666ff,), the rough idea is that as the body size increases, the propulsion system must also change in order to generate sufficient force to move the larger animal through the water. So there is a morphological trend in the plesiosaurs—a change in the ratio of girdle length (i.e., the length of the pelvic and shoulder girdles) to propoidal length (i.e. the length of the femur and humerus, respectively)—and that trend is explained by reference to body size increase plus biomechanical constraints. In other words, O'Keefe and Carrano are *using one morphological trend to explain another*.

Figure 1.1. provides a rough illustration of the causal story that one can glean from O'Keefe and Carrano's (2005) paper.

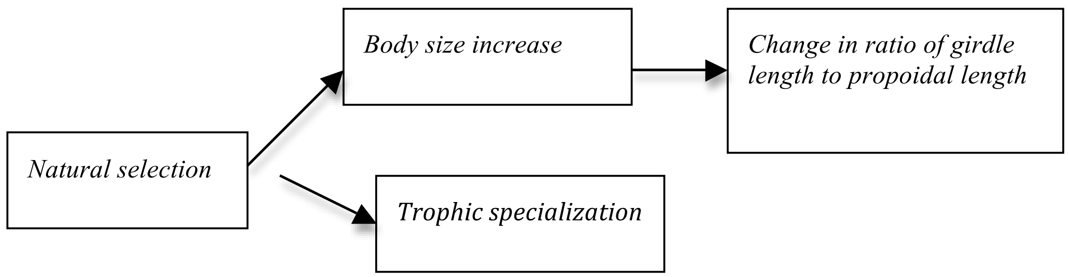

Figure 1. Causal claims implicit in the work of O'Keefe and Carrano (2005).

I want to be cautious here in attributing causal claims to O'Keefe and Carrano, because they themselves seem to shy away from talk of causation, preferring instead to say that trends are "correlated." Their work does at least suggest, however, the following two causal claims (represented in Figure 1):

> **C1.** Natural selection is the cause of certain trends in plesiosaur morphology, especially body size increase and trophic specialization.
>
> **C2.** Body size increase in the plesiosaurs caused other changes in the morphology of the locomotor system, in particular, changes in the ratio of girdle length to propoidal length.

Notice that C1 seems to imply that historical trends can stand in causal relationships with *historical processes*, such as natural selection, whereas C2 seems to imply that trends can stand in causal relationships with *other trends*.

Consider first claim C1. Although O'Keefe and Carrano do not explicitly claim that natural selection is what drove either body size increase or trophic specialization in the plesiosaurs, the do claim that body size increase was an "active" trend. In support of this claim, they invoke an empirical test first proposed by McShea (1994), and known as the stable minimum test. McShea argued that the stable minimum test can be used to determine whether an evolutionary trend is passive or driven. (See also section 4 of this paper for further discussion of passive trends.) To a first approximation, we can say that a trend is *driven* when there is a directional bias in the state space—for example, when body size increases are more probable than decreases. When a trend is *passive*, the clade does a "random walk" away from a fixed boundary in the state space. (For a lovely illustration of this concept, see Gould's 1996 description of the “drunkard’s walk.”) When a trend is passive, the size of the smallest members of the clade should remain constant. But if the size of the smallest members of the clade increases over time, the increasing minimum suggests that something is driving the evolution of the clade toward larger size. Traditionally, most scientists have just assumed that the “driver” would be natural selection (see, e.g. Hone and Benton 2005). Although O’Keefe and Carrano use slightly different terminology—they talk of “active” rather than “driven” trends—many, though not all, scientists use those terms interchangeably (Wang 1996 is one exception). Applying McShea’s stable minimum test to the plesiosaurs, O’Keefe and Carrano conclude that body size increase in the plesiosaurs was an active trend. The background assumption here is that the cause of an active trend would be natural selection. Natural selection is also a pretty obvious candidate for being the cause of trophic specialization in plesiosaurs. Although this idea would still need to be tested, the morphological trend in HN ratios plausibly results from the adaptation of different groups of plesiosaurs to different food sources, and to different ecological roles.

Although the mainstream view is that natural selection is a cause of evolutionary change, there is an interesting minority view according to which natural selection is itself just a special sort of historical trend—a biased directional trend in trait frequencies (or perhaps better, in gene frequencies) in a population (Matthen and Ariew 2002; Walsh, Lewens, and Ariew 2002). Without weighing in on the ongoing controversy concerning the status of natural selection, it is at least worth pointing out that if the statisticalist view of selection is correct, then it would follow that whenever scientists explain macroevolutionary trends in terms of natural selection, they are essentially explaining trends in terms of other trends.

What about claim C2? The conclusion of O’Keefe and Carrano’s paper is telling:

> Two broad trends are demonstrable in the evolution of the Plesiosauria—an active trend of body size increase and a trend toward divergent trophic specialization. Measures of the locomotor system show a complex set of correlations with these trends. Concerning body size, identical changes in the geometry of the locomotor system are evident in all plesiosaur subclades. This suggests that the physical constraints of thrust production placed demands on the locomotor system that *resulted* in allometric changes, specifically the relative shortening of propoidals and lengthening of girdle elements (2005, p. 672, emphasis added).

They seem to be making a causal claim here: Body size increase, together with certain facts about the biomechanics of swimming, provides a causal explanation of the trend in locomotor morphology. Tellingly, they refrain from arguing that trophic specialization caused any trends in locomotor morphology:

> The trend toward trophic specialization is also correlated with stereotyped geometries in the locomotor system. These patterns are statistically significant … but we lack the data to constrain speculation about why the observed correlations occur (2005, p. 672).

Their cautious restraint here seems to suggest that they do take themselves to have the data to support the causal claim expressed in C2.

3. The metaphysics of historical trends: Some preliminary distinctions

In order to get clear about what it means to say that trends can stand in causal relations, we first need get clear about what trends *are*. Here I offer some initial observations about the metaphysics of trends. The metaphysics of trends remains a relatively under-explored topic, and these observations barely scratch the surface. But they will be helpful in what follows.

First, recall that I defined a historical trend as a persistent directional change in some variable. Thus defined, a trend is one kind of pattern. All trends are patterns, but not all patterns are trends. A good example of a pattern that should not be considered a trend is the 32 million year periodicity of mass extinction events that paleontologists David Raup and Jack Sepkoski thought they had discovered (Raup and Sepkoski 1984). That was an alleged pattern in the fossil record, but the pattern was cyclical and did not involve any persistent directional change.

Second, it might be useful to distinguish at the outset between *population-level trends* and *trends in the properties of individuals*. For example, as a child grows up, her height increases in a directional fashion. Height, in this case, is a property of one individual. By contrast, an increase in the average height of American 5th graders over a given interval of time would be an example of a population-level trend. Here I will focus exclusively on population-level trends, such as body size increase and trophic specialization in plesiosaurs.

Third, the reality of a trend is relative to one's decision about which spatial and temporal scale to focus on. To give a simple example: over a given time interval, unemployment might fall in one particular region, even while it increases at the national scale. Although it's a bit awkward, we can say that "rising unemployment" is a real trend at the national scale but not at the local scale. Similarly, if we fix attention on a particular region, then rising unemployment might be a real trend over some relatively short time interval—say, six months. But if we zoom out and look at a longer 5-year interval, rising unemployment may no longer show up as a real trend. This fact about scaling effects is crucial for understanding debates within paleontology concerning macroevolutionary trends, such as Cope's rule of size increase. Cope's rule does show up as a real trend at some spatiotemporal scales, and in some clades, but not in others (McShea 1998, Turner 2011, p. 110).

Fourth, some trends are constitutive parts of others. For example, if a professor begins his career as a tough grader but eases up over time, the trend in average grades awarded by that person might be part of a larger grade inflationary trend on campus. When thinking about trends, it will be helpful to attend to the distinction between *causation* and *constitution*. The professor may himself be a contributing cause of grade inflation (let's save that issue for later), but it would not be quite correct to say that the upward trend in his grading is a cause of grade inflation. Rather, grade inflation on campus is constituted, in part, by the trend in this one faculty member's grading practices.

The approach I take, beginning in the next section, is to start with Woodward's (2003) articulation of an interventionist theory of causation and attempt to see whether that theory can shed some light on O'Keefe and Carrano's work on Plesiosaur evolution. One clear limitation of this approach is that it ignores all the other going philosophical theories of causation. One can envision a more grandiose philosophical project that would involve surveying those theories in order to see how well each one can handle causal claims about trends, but that's much more than I can take on here.

It may also be helpful to bypass some deeper questions about the metaphysics of trends. For example, are trends and patterns abstract objects? (See Dennett 1991 for fascinating discussion.) If so, what is at stake in debates about whether trends such as Cope's rule, or even global warming, are real? Can abstract objects stand in causal relations? Using Woodward's work to ground the subsequent discussion will happily make it possible to set some of these very difficult questions to one side.

4. An interventionist proposal

Woodward's (2003) interventionist theory of causation might seem to offer a solution to this puzzle about how trends can stand in causal relations. One reason for optimism is that

Woodward treats causation as a relation that obtains between different variables, not as a relation between concrete objects or events.

> It is most perspicuous to think of causal relationships as relating variables or, to speak more precisely, as describing how changes in the value of one or more variables will change the value of other variables. This is also the way that many scientists think about causal relationships (2003, p. 38).

Since a trend is merely a persisting directional change in the value of some variable, Woodward's approach seems very amenable to the idea that trends can be causes and effects. Woodward's thought, to a first approximation, is that "X causes Y" means that if you could perform an experiment in which you manipulate the value of X while holding fixed the values of all the other relevant variables, then the value of Y would change accordingly. And we do seem to be able to manipulate variables in a persistent, directional way. Think, for example of our manipulation of atmospheric CO2 concentrations.

Woodward himself does not say anything about historical trends. When he discusses the hypothetical manipulation of variables, he does not consider cases that would involve persistent directional changes in the values of those variables. Nevertheless, his interventionist approach seems able to accommodate such cases.

Another possible hitch is that Woodward seems to focus exclusively on individual-level properties. Thus, he writes that

> Values of variables are always possessed by or instantiated in particular individuals or units, as when a particular table has a mass of 10 kg" (2003, p. 39).

This seems to exclude cases involving directional changes in population-level properties. For example, the average body size of plesiosaurs is a variable whose values are not instantiated in any particular individual or unit. Even with this restriction, Woodward's approach might be able to explain how trends in individual properties (see section 2 above) could stand in causal relations. It's not entirely clear, however, that this restriction is essential to his view. There seems to be no principled reason why we could not intervene on variables associated with aggregate or population-level properties. What's needed is some additional principle to cover cases in which the values of variables change in a persistent, directional way.

Explaining how trends in population-level properties can stand in causal relations poses a bit more of a challenge. The distinction between constitution and causation (introduced in section 2) might seem to cause some trouble here. Consider Dennett's (1991) example of the mean geographical center of population of the U.S. The mean geographical center of population is an example of a population-level property. The U.S. Census Bureau defines the mean center of population in the following way:

> The mean center of population is the point at which an imaginary, flat, weightless, and rigid map of the United States would balance if weights of identical value were placed on it so that each weight represented the location of one person.[1]

Now suppose we perform an experiment in which one individual relocates from Connecticut to California, while somehow holding fixed the locations of everyone else. As a result, the mean center of population will shift a bit to the west. In this example, we are wiggling the value of an individual-level variable—i.e. the location of a single citizen—while holding other relevant variables fixed, and the value of the population-level variable wiggles along. Thus, Woodward's view would seem to imply that individuals can *cause* changes in the mean geographical center of population by moving across the country. At least, it would imply that if (contrary to Woodward's restriction mentioned above) we took that theory to apply to population-level variables.

One potential problem with this example of an intervention on the mean geographical center of population is that it seems to conflate *causation* with *constitution*. The mean center of population is just an aggregate measure of the addresses of all individual U.S. citizens. Those individual addresses are best described as constituting the mean center of population, rather than causing it. In general, it sounds odd to say that an average (or some other aggregate measure) is an effect of the properties of the individuals in a population. Perhaps one reason for the oddness is that we usually think of causes as temporally preceding their effects, whereas the constitution relation is not a temporal one. The change in the mean geographical center of population is simultaneous with the individual's relocation. At any rate, if we take this distinction between constitution and causation seriously, we seem driven toward the view that changes in the values of population-level variables are not, in general, caused by changes in the values of individual-

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[1] From the website of the U.S. Census Bureau (http://www.census.gov/population/www/censusdata/files/popctr.pdf), last accessed on 10 April, 2012.

level variables. Instead, they are constituted by changes at the individual level. This may help explain why Woodward chooses to restrict his focus to individual-level variables. As long as we focus exclusively on individual-level variables, the distinction between constitution and causation does not become an issue.

In the example from section 1, the main challenge was to try to understand how a trend in one population-level variable could cause a trend in another population-level variable. In spite of the above worry about the constitution/causation distinction, Woodward's interventionist account might seem able to make sense of causal relations between different population-level variables, in much the same way that it makes sense of causal relations between different individual-level variables. Perhaps what we need is a restriction saying that causal relations can only obtain between variables at the same level; population-level properties are generally constituted, not caused by, individual-level properties. This, however, leaves entirely open the possibility that population-level variables might stand in causal relations with one another.

Reisman and Forber (2005) take an interventionist line in response to the statisticalist views of Walsh, Ariew, Lewens, and Matthen. Reisman and Forber point out that in certain experimental settings, scientists can manipulate drift and selection and check to see how those manipulations make a difference to the subsequent evolutionary patterns. For example, experimentalists can and do control the "strength" of drift by manipulating the starting size of a population (of fruit flies, bacteria, or whatever). This line of argument may suggest that population size may cause population-level trends, in an interventionist sense of "cause." What this argument does not do, however, is help make sense of the idea that one trend can be a cause of another. The sort of causal claim exemplified by C2 remains in need of clarification.

The following interventionist proposal seems promising:

> *Trend-Trend Causation*. "A trend in the value of *X* causes a trend in the value of *Y* (where the values of *X* and *Y* are both population-level properties, or both individual-level properties)" *means that* "If you could manipulate the value of *X* in a persistent, directional way, while holding fixed all the other variables that might make a difference to *Y*, then the value of *Y* would also move in a correspondingly persistent, directional way."

This proposal slightly modifies Woodward's original theory in order to accommodate both individual property and population-level historical trends. It certainly seems consistent with the spirit of the original theory.

The above proposal makes good sense of the initial case that I described in section 1. In that opening example, O'Keefe and Carrano seemed to be saying that the trend in plesiosaur body size was causing certain other morphological trends relevant to plesiosaur locomotion. As body size increases (in a persistent, directional fashion) while certain biomechanical constraints on swimming remain fixed, then something else has to give. In this case, there was a resulting trend in the ratio between the lengths of the bones in the shoulder and pelvic girdles, on the one hand, and the lengths of the propoidal bones on the other. The interventionist proposal above clarifies what we mean by this talk of the trend in body size causing the trend in the morphology of the locomotor system. We are, in essence, talking about a hypothetical experimental intervention. If we could somehow manipulate the body size of a plesiosaur while holding fixed all the biomechanical constraints on swimming, then in order for the animal to remain viable at all, certain other aspects of its locomotor system would have to change: The bones in the girdle would get relatively longer, while the propoidal bones get relatively shorter. It is tempting to say, with a bit of personification, that natural selection did in fact carry out this experiment during the Mesozoic.

Thus, the interventionist proposal seems to make good sense of the idea that population-level trends can stand in causal relations with other population-level trends, and thus illuminates the type of scientific practice described in section 1. We now seem to have a clearer idea of what's going on when scientists causally explain one historical trend in terms of another. Indeed, the fact that interventionism can make sense of cases that its defenders have not yet considered would seem to count strongly in favor of an interventionist approach to causation. In the next section, however, I argue that there are some serious problems with the appealing interventionist proposal.

5. Passive trends

The analysis of trend-trend causation developed in section 4 represents a plausible extension of Woodward's interventionist theory, and one that sheds light on a least some of the causal claims that figure in the historical sciences. For example, the claim that increasing atmospheric CO2 concentration is a cause of global warming is an example of a claim about trend-trend causation. However, the proposal developed above only goes so far. I'll now consider a rather different case, also drawn from paleontology, that is difficult to understand in interventionist terms.

Suppose that we are tracking the mean body size of some clade. And suppose that increases and decreases in body size are equally probable. With each time interval, it is as if a fair coin is flipped to determine whether size increases or decreases. Next, suppose that there is a fixed boundary in the state space, or a fixed minimum size for the clade. That fixed boundary could result from natural selection working against small-bodied organisms, but it could also be a result of biomechanical constraints. If the clade starts out at or near this fixed lower boundary, then the mere process of diversification will lead to an increase in the mean body size, even if there is no directional bias in favor of larger size, and even if natural selection "doesn't care" about body size. The clade will do a random walk away from the fixed boundary. This idea was first proposed as an explanation of Cope's rule by Stanley (1973), and has been invoked by other scientists since then (e.g. Gould 1988, 1997).

Passive trends do involve persistent directional change in the mean, but that change is (by definition) not caused or driven by a persistent directional change in some other variable. If a passive trend were caused by some other trend, in somewhat the same way that the morphological changes in plesiosaur limbs, hips, and shoulder joints were caused by body size increase, then it would not be passive. So the proposed extension of interventionism that I explored in section 4 (trend-trend causation) will not help here. Is there some other way of thinking about the causes of passive trends in interventionist terms? Or would it be best to say that passive trends have no (interventionist) causes at all?

We can approach these issues in a systematic way by considering all the possible variables that one could intervene on in ways that might make a difference to the character of a passive trend. For illustrative purposes, it will help to stick with the example of body size evolution. In principle, one could manipulate (i) the starting body size of the clade, (ii) the fixed boundary in the state space, or the minimum body size for the clase, or (iii) the strength of the directional bias in the state space. I'll consider each of these three possibilities in turn.

5.1 Manipulating the starting value of the target variable

One initially plausible suggestion is that the starting mean body size of the clade is a cause of the passive trend. If we could explain why mammals started out so small, we would thereby have (partly) explained the subsequent size increase. Moreover, the starting body size of the clade is certainly a variable on which we can perform hypothetical interventions. It's easy to imagine a counterfactual scenario in which the first mammals were the size of mastodons. Nevertheless, what we're looking for is some *other* variable that would stand in a causal relationship to mean body size. When we imagine interventions on the starting mean body size of the clade, all we are doing is imagining how later values of that variable might depend on the starting value of the same variable.

It is an interesting question what an interventionist should say about cases where the later values of a variable depend on the earlier values. For example, if you heat up a pot of water and let it sit at room temperature for five minutes, the temperature of the pot at the end of that interval will depend on the starting temperature. Should we say that the starting temperature is a cause (in the interventionist sense) of the later temperature? Interventions on the starting temperature will certainly make a difference to the temperature at the end of the five minute interval. One problem here is that the starting temperature and the ending temperature are not different variables; they are just different values of the same variable. For that reason, the starting temperature would not count as a cause on Woodward's view.

Recall the definition of 'trend' as any persistent, directional change in some variable of interest. The initial value of that variable will always be something that one can (in principle, at least) manipulate. But what we want to explain, in the case of a trend, is the persistent, directional change.

5.2 Manipulating the fixed boundary in the state space

One can also imagine hypothetical interventions on the fixed boundary in the state space. Imagine, for instance, that the minimum body size for mammals increases over time, so that at some later time, the smallest possible mammal is the size of a large dog. This could happen if minimum body size were determined by some environmental factors (say, the strength of the earth's gravitational field, or the density of the atmosphere) that change over time. Most scientists do in fact think that the oxygen content of the atmosphere imposes a size maximum on certain kinds of organisms, especially insects, and that this maximum has changed in the past. The shifting lower boundary could well drive a change in the mean body size of the clade. In this case, however, the trend would no longer fit the canonical definition of "passive." It would be an example of what I have elsewhere called a "shifting boundary" trend (Turner 2009). In the literature, passive trends are typically associated with fixed boundaries in the state space.

According to the interventionist picture, two variables stand in a causal relationship to one another when changes in the value of one are sensitive to changes in the value of another (other things being held fixed). The problem is that in the case of a passive trend, mean body size increases while the size minimum remains fixed in the state space. We could drive up the mean body size by moving the minimum size upwards—that would be a shifting boundary trend—but what about a case where the mean body size trends upward with no change at all in the fixed boundary? In such a case, the fixed boundary would not seem to count as a cause of the upward trend. The fact that shifting the boundary could be an (interventionist) cause of a trend in body size does not mean that the fixed boundary is a cause where the trend is passive. In the case of a passive trend, the boundary in the state space is one of the "background" variables that's being held fixed.

5.3 Manipulating the directional bias in the state space

One important difference between passive and driven trends is that the latter are generated by a directional bias in the state space. For example, if increases in body size were more probable than decreases, the resulting trend toward larger body size would be driven. The strength of the directional bias is also a variable that one could hypothetically manipulate. (See, e.g., Turner 2009 for some discussion of "shifting bias" trends.) Intuitively, where a trend is driven, we might want to say that the directional bias in the state space is the cause. It seems natural, in other words, to say that body size increases because there is a directional bias toward larger size. This is akin to saying (in the context of coin tossing) that we've obtained a long sequence of heads because the coin is weighted toward heads, that the bias toward heads is the cause of the pattern. If in the case of driven trends, we're inclined to say that the directional bias causes the trend, then why not say that the *absence* of any bias is what causes a passive trend? We can cause a passive trend by setting things up so that the probabilities of size increase and decrease are equal.

One problem with the above suggestion is that it's impossible to generate a passive trend merely by manipulating the strength of the directional bias in the state space. A passive trend also requires the fixed boundary in the state space, and it requires that the system start out at or near that fixed boundary. Suppose, for example, that we start out with a clade (a set of evolving lineages) whose mean size is well above the minimum boundary. If we suppose that size increases and decreases are equally probable, there's no reason to expect any trend at all toward larger size. Of course, such a trend is always possible. But saying that a trend toward larger size is caused by the absence of a directional bias would be akin to saying (in a coin tossing context) that a long sequence of heads is caused by the fact that the coin is fair.

Recall that interventionism treats causation as a relation between variables. In the case of passive trends, we have seen that there are three variables that one could, in principle, intervene on: (i) the starting value of the variable of interest (e.g. the starting mean size of the evolving clade), (ii) the fixed boundary in the state space, and (iii) the strength of the directional bias in the state space. Passive trends require that all three of these factors be set up in just the right way. Grantham (1999) aptly refers to these factors as "structuring causes," and there may well be some loose sense in which this set-up is the "cause" of a passive trend. The interventionist approach has some difficulty with this case, however. Taking each of these three variables in isolation, it's difficult to make any sense of the idea that any one of them is an interventionist cause of the passive trend. Obviously, by intervening on any one of these variables—say, by shifting the boundary, introducing a bias, or moving the starting value away from the boundary—we can make a difference to the resulting pattern or trend. However, the crucial thing to see is that in a passive diffusion model, all of these variables are *held fixed*: the size minimum doesn't change; the initial mean size of the clade does not change as the system evolves; and the directional bias remains set to zero. Yet mean body size trends upward. Because these variables all remain fixed while mean body size increases, we cannot point to changes in these variables as causes of the change in mean body size.

6. Conclusion

I began with a straightforward observation about the practices of the historical natural sciences. Historical scientists often investigate the causes and effects of trends. Indeed, they often develop and test causal explanations of historical trends. One task for philosophers of science is to see whether our best philosophical theories—in this case, theories of causation—can make intelligible this bit of scientific practice. Here I have argued that with a small modification, Woodward's interventionist theory can indeed make sense of many causal claims about trends. This interventionist approach does a good job explaining what it might mean to say that one trend causes another; thus, it does a good job illuminating the paleontological case study with which I began. It does, however, run into problems in the case of passive trends. There the trends in question seem not to have any (interventionist) causes. Insofar as scientists do explain a trend by claiming that it's passive, they are not offering a causal explanation of it, at least not in the interventionist sense explored here.

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