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The past vs. the tiny: historical science and the abductive arguments for realism

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Abstract

Scientific realism is fundamentally a view about unobservable things, events, processes, and so on, but things can be unobservable either because they are tiny or because they are past. The familiar abductive arguments for scientific realism lend more justification to scientific realism about the tiny than to realism about the past. This paper examines both the "basic" abductive arguments for realism advanced by philosophers such as Ian Hacking and Michael Devitt, as well as Richard Boyd's version of the inference to the best explanation of the success of science, and shows that these arguments provide less support to historical than to experimental realism. This is because unobservably tiny things can function both as unifiers of the phenomena and as tools for the production of new phenomena, whereas things in the past can only serve as unifiers of the phenomena. The upshot is that realists must not suppose that by presenting arguments for experimental realism they have thereby defended realism in general.

Keywords: Realism; Abduction; Historical science; Hacking; Devitt; Boyd

Scientific realism is fundamentally a view about unobservable entities, events, properties, and processes. There are different reasons why things are unobservable. Some things, such as positrons and electrons, are unobservable because of their small size relative to us. Other things, such as dinosaurs and the trial of Socrates, are unobservable because they existed or occurred in the past. Since there are different kinds of unobservables, there are different species of scientific realism. An *historical realist* is someone who takes the scientific realist view of things that are

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unobservable because they are past, while an *experimental realist* is someone who takes the realist view of things that are unobservable because they are so tiny. In this paper, I will argue that the abductive case for historical realism is weaker than the case for experimental realism. This fact has gone unnoticed for a long time because most of the participants in the debate between scientific realists and their critics (including a variety of selectively skeptical empiricists, constructivists, verificationists, and others) have paid little attention to historical science.

Before explaining why this omission is important, the claim that there *are* unobservables in historical science needs to be defended. Kitcher (1993) has noted that past entities, events, and processes are unobservable. Whether they are unobservable in principle or only in practice may depend upon the (logical? physical? technological?) possibility of time travel, but such considerations are irrelevant to current scientific contexts. Yet someone might nevertheless insist that dinosaurs are observable, on the grounds that there are certain subjunctive conditional statements that are true of dinosaurs but not true of other things, such as electrons. For example, it is true that if there were a dinosaur out on the green, then I would be able to see it, smell it, and so forth, but this is not at all true of electrons. If the claim that x is observable just means that some such subjunctive conditional statements are true of x, then there is nothing wrong with saying that dinosaurs are observable. However, this does not change the fact that we cannot observe them, because they no longer exist.[1]

Unobservable entities, processes, and so forth can play either of two roles in science. First, they can serve as *unifiers of the phenomena*. Unobservables play the unifying role insofar as scientists formulate hypotheses about them with the aim of reducing the number of facts that must be taken as brute (in the sense of Friedman, 1974) and/or providing the best explanatory unification of the phenomena (in the sense of Kitcher, 1989). Kitcher's ideal of explanatory unification has a Leibnizian flavor: roughly, he thinks that the best explanatory unification of the phenomena is one that maximizes the number of conclusions about the phenomena while minimizing the number of premises as well as the number of inferential patterns.[2] On the other hand, Ian Hacking (1983) has also pointed out that unobservable entities such as electrons and positrons can serve as *tools for the production of new phenomena*. Hacking's idea is that once scientists learn some of the causal properties of unobservables, they can build experimental apparatus that will enable them to do things with those unobservables, with the aim of producing and controlling new observable effects. Clearly, these two roles—the unifying role and the producing role—are not exclusive, and many unobservables, including electrons, do double duty. The crucial premise of my argument, however, is that things that are unobservable because they no longer exist, and events that are unobservable because they occurred a long time ago, cannot possibly serve as tools for the production of

[1] I thank Michael Lynch for helping me to see how to put this point most effectively.

[2] Leibniz held that God's perfection consists in producing the richest and most diverse effects by the simplest means. See Leibniz (1989, p. 38).

new phenomena. This may seem like a trivial observation, but it has important consequences for the familiar abductive arguments for scientific realism.[3]

In this paper, I will focus exclusively on the positive arguments for scientific realism that have been refined over the last few decades by such philosophers as J. J. C. Smart (1963), Hilary Putnam (1978), Richard Boyd (1984, 1985, 1990), Ian Hacking (1983), Michael Devitt (1991), and Jarett Leplin (1997). The thesis is only that the positive *a posteriori* arguments for historical realism are weaker than those for experimental realism. This does not, of course, settle the question of whether we should be realists about the past; for that, we would also need to see how the classical arguments against realism (especially the underdetermination argument and the pessimistic meta-induction) fare in the context of historical science. Nor do I offer anything like a general appraisal of the abductive arguments for realism. My only concern is to show that one species of realism is better supported by those arguments than another.

1. Two kinds of realism

Realism is in fact a family of views about unobservables, including an *epistemological view* (we have scientific knowledge of unobservables, for our theories about them are true or approximately true); a *metaphysical view* (unobservables are mind- and theory-independent); a *view about truth* (statements about unobservables are made true or false by the way the world is); a *view about reference* (many of the terms that we use to think and talk about unobservables genuinely refer); an *axiological view* (scientists aim to discover the facts about unobservables, and not merely to achieve accurate prediction and control); and a *view about how to characterize scientific progress* (later, better-confirmed theories about unobservables are more likely to be true, or are better approximations of the truth, than the theories they have superseded). Realists disagree amongst themselves about which of these views is the most important, and different varieties of anti-realism can be arrived at by negating the different realist theses. In addition, scientific realism is closely allied with philosophical naturalism. Most realists take the metaphilosophical view that the arguments for realism ought to be empirical arguments—no different in principle from the kinds of arguments exchanged by scientists—and realists are often committed to broadly naturalistic theories of meaning, reference, and knowledge. Finally, most realists believe in the reliability of inference to the best explanation, which is to say that they accept the underlying principle that the best potential explanation of the phenomena (according to some set of standards, such as simplicity, comprehensiveness, explanatory power, and so forth), is probably true.

[3] This distinction between the unifying role and the producing role is similar to one drawn by Nola (2002). Nola's "realism by hypothesis" is realism about unobservables that play the unifying role, and his "realism by manipulation" is realism about unobservables that play the producing role. In formulating the argument of this paper I have been much influenced by Nola's distinction between these two kinds of realism. What is missing from his discussion, however, is any consideration of historical science.

This paper focuses exclusively on the metaphysical and epistemological dimensions of scientific realism, while leaving aside questions about truth, reference, explanation, and progress, that other philosophers will no doubt find important. It will help to begin with some definitions:

Historical Realism (HR):

HRM. *Realist Metaphysics*. Most of the unobservable events, entities, and so forth, that scientists currently believe in, and that are unobservable because they existed or occurred in the past, existed or occurred independently of our thoughts, theories, vocabularies, conceptual schemes, and so forth.

HRE. *Realist Epistemology*. We currently have a good deal of scientific knowledge of things, events, and so forth that are unobservable because they existed or occurred in the past. (Or, many of our beliefs about past entities, events, and so forth are true or approximately true.)

Experimental Realism (ER):

ERM. *Realist Metaphysics*. Most of the unobservable events, entities, and so forth, that scientists currently believe in, and that are unobservable because of their small size relative to us, exist or occur independently of our thoughts, theories, vocabularies, conceptual schemes, and so forth.

ERE. *Realist Epistemology*. We currently have a good deal of scientific knowledge of things, events, and so forth that are unobservable because of their small size relative to us. (Or, many of our beliefs about unobservably tiny entities, events, and so forth are true or approximately true.)

What I aim to show is that the arguments for HRE and HRM are weaker than the arguments for ERE and ERM.

There are several very general worries that one might have concerning the *a posteriori* arguments for realist epistemology and metaphysics. First, one might reasonably wonder whether empirical arguments that proceed from observed facts about science (e.g. the fact that scientific theories and/or methods are so successful) could possibly be relevant to the metaphysical question whether some things are mind-dependent or independent. Claims to the effect that something is mind-dependent or independent usually take the form of counterfactuals, such as:

(A) ''If human physicists had never existed, there never would have been any electrons''.

Or:

(B) ''Even if human physicists had never existed, there still would have been electrons''.

It is worth asking whether counterfactual statements like these have any consequences at all concerning the success of scientific methods and theories. Realists,

being also naturalists, often want to treat their claims about mind-independence as if they were empirical hypotheses about science. Is the disagreement between someone who believes (A) and someone who believes (B) really an empirical disagreement, or is it a purely conceptual one? Granted, most naturalists deny that there is any sharp distinction to be drawn between empirical and conceptual disagreements, but it is not enough merely to assert this. Realists need to show how counterfactual statements such as (B) help explain facts about science, or else they need to show that (A) and (B) are observationally testable. This is important because both Devitt and Boyd, whose views will be discussed in Sections 4 & 5, take the abductive arguments for realism to support claims such as (B).[4] Both Boyd and Devitt, being naturalists, are committed to the view that claims such as (B) are empirical hypotheses about science. For purposes of this paper, I will leave aside worries about the coherence of such a view.

The most familiar and most widely discussed challenge to the abductive arguments for realism has been Laudan's (1981) claim that the arguments are undermined by historical examples of instrumentally reliable theories whose central theoretical terms did not refer. This line of criticism has been developed further by philosophers such as Carrier (1991), who gives two examples of theories—the phlogiston theory of combustion and the caloric theory of heat—that yielded accurate novel predictions even though their central theoretical terms did not refer. Again, for purposes of this paper, I will assume that realists have adequate replies to the pessimistic meta-induction.

These two lines of objection to the abductive arguments for realism would apply with equal force to the arguments for historical and experimental realism. By assuming that these and other general objections to the abductive arguments are answerable, it is possible to isolate the question whether the difference between the past and the tiny makes any difference to the realists' abductive arguments.

One might think that realism about the past has a higher degree of initial plausibility than does realism about the tiny. If that were the case, then the arguments for historical realism would not need to be as good as those for experimental realism. However, someone who takes this line will need to point to a relevant difference between the two kinds of realism that explains why the one is initially more plausible than the other. Moreover, Michael Dummett's (2002) exploration of the verificationist view of statements about the past shows that we need to take seriously the possibility that historical realist metaphysics might be false. Dinosaurs, the Big Bang, and other things and events of the past have not escaped the attention of social constructivists, either.[5] This suggests that realism about the past needs to be defended just as vigorously as realism about the tiny.

[4] See, for instance, Devitt's definition of 'scientific realism' (1991, p. 24), as well as Boyd's (1990, p. 215).

[5] See, for example, the discussion of social constructivist views of dinosaurs in Parsons (2001, Ch. 4).

2. Two roles for unobservables

Things that are unobservable because they existed or occurred in the past can only serve as unifiers of the phenomena, and never as tools for producing new phenomena. Here is the general argument:

1. The unobservables of historical science are all unobservable because they no longer exist, or (if they are events) because they occurred in the past.
2. Things that no longer exist, and events that occurred in the past, cannot be manipulated or altered by experimenters in the present.
3. Therefore, the unobservables of historical science can never be used as tools for the production of new phenomena.
4. Therefore, if the unobservables of historical science have any scientific role to play, it can only be as unifiers of the phenomena.

A case study will help to drive this conclusion home.

The paleontologists Kevin Padian and Paul Olsen once devised an experiment to test rival hypotheses about the posture of therapod dinosaurs, a group of animals that has been extinct for 65 million years (Padian & Olsen, 1989). They wanted to determine whether therapods walked with a forward-leaning semi-erect posture, or with a fully erect posture. The experiment they devised was based on their earlier research on ancient crocodilians (Padian & Olsen, 1984). They had coaxed a living crocodile to walk through a patch of mud, using the two different crocodile gaits—the high walk and the low-slung sprawling walk. They had then compared these tracks to the preserved tracks made by ancient crocodilians. Finally, they had inferred, on the basis of the observable similarities between the two sets of tracks, that the stances and gaits of the ancient crocodilians resembled those of the living animals. Padian & Olsen argue that inferences like this will go through, so long as we are comparing ancient and living organisms belonging to 'a single phylogenetically restricted group' (1989, p. 231). The underlying methodological assumption is that 'functional similarity between animals is correlated with degree of phylogenetic relationship' (ibid., p. 232). Since birds are probably the closest living relatives of the therapod dinosaurs, Padian & Olsen ran a similar experiment with a 25 kg South American rhea from the Oakland Zoo. They had the bird walk across a bed of potter's clay, and they observed some similarities between the rhea's tracks and those of ancient therapods: both animals place their feet near to the midline of the trackway; in both cases, the middle toe is pointed slightly inward; neither animal's toes leave drag marks, and so on. These similarities, they argue, lend some support to the hypothesis that therapods had an erect posture, like living ratites.

Is this an example of historical science? Experimental science? Both? It is obviously historical in the sense that Padian & Olsen ultimately want to test hypotheses about the posture of Mesozoic therapods, animals that have been extinct for at least 65 million years. The question is how they go about testing these hypotheses.

In a recent paper that contrasts the methods of experimental science with those of historical science, Carole Cleland offers the following helpful description of experimental science:

> The hypotheses investigated in classical experimental science postulate regularities among event-types. A test condition C is inferred from the hypothesis and a prediction is made about what should happen if C is realized (and the hypothesis is true). This forms the basis for a series of controlled experiments. (Cleland, 2002, p. 476)

Suppose that the hypotheses being investigated are biomechanical generalizations about the relationships between posture and certain footprint features. Then those hypotheses are tested by having an animal with an erect posture walk across different substrates at different speeds. Not only is the work experimental in this sense, but it also has the virtue of repeatability. Anyone who could get their hands on a rhea or an ostrich could run the same set of trials in the back yard. Notice, however, that if Padian & Olsen's work is described in this way—that is, as the testing of various biomechanical hypotheses—the historical part is left out of the picture.

It turns out that Padian & Olsen's work accords just as well with Cleland's characterization of 'prototypical historical science':

> Historical scientists focus their attention on formulating mutually exclusive hypotheses and hunting for evidentiary traces to discriminate among them. The goal is to discover a "smoking gun". A smoking gun is a trace(s) [sic] that unambiguously discriminates one hypothesis from among a set of currently available hypotheses as providing "the best explanation" of the traces thus far observed (2002, pp. 480–481).

This is exactly what Padian & Olsen have done. They have formulated mutually exclusive hypotheses about Mesozoic therapods (erect *vs*. semi-erect posture), and their argument boils down to the claim that therapod footprints are the smoking gun that indicates that the animals had an erect posture. However, in order to show *that* the footprints constitute a smoking gun, they need to invoke biomechanical generalizations about the relationship between footprint features and posture, and as we have seen, it is those generalizations that needed to be tested experimentally. We might therefore describe this as a case of hybrid research, in which experimental science is pressed into the service of historical inference. Nevertheless, there is another sense in which their research is not experimental at all, a sense in which experimentation in this paleontological context is utterly different from experimentation in chemistry and physics.

In order to see why, consider Ian Hacking's story of his conversion to a form of scientific realism about entities, which he defines as the view that 'a good many theoretical entities really do exist' (1983, p. 27). Hacking says that he was converted upon learning about an experiment in which some researchers used a device to spray positrons and electrons at a Niobium drop, thus altering the drop's electric charge. According to Hacking, the best reason for thinking that electrons and

positrons really do exist is that we can do things with them, and can even use them in the construction of tools for the detection of other unobservables, such as quarks:

> Moreover, it is not even that you use electrons to experiment on something else that makes it impossible to doubt electrons. Understanding some causal properties of electrons, you guess how to build a very ingenious complex device that enables you to line up the electrons the way you want, in order to see what will happen to something else … Electrons are no longer ways of organizing our thoughts or saving the phenomena that have been observed. They are ways of creating phenomena in some other domain of nature. Electrons are tools. (1983, p. 263)

At this point, I am less interested in Hacking's defense of a kind of scientific realism (more on that in a moment) than in his description of the role that electrons and other unobservable entities have come to play in physics. That is, they are manipulated and used as tools for the creation of new phenomena. Padian & Olsen are just as concerned with unobservable entities as anyone working in experimental physics. The difference is that paleontology's unobservable entities cannot possibly play the role that electrons play in the design, construction, and use of experimental apparatus. While it is true that the design of Padian & Olsen's experiment was theory-dependent, in the sense that it was informed by background theories of taphonomy and evolution, they could not *do* anything with the dinosaurs, or use them in any way to create new experimental phenomena. The best they could do was to use a living rhea. In this case, the unobservable therapod trackmakers are not 'ways of creating phenomena in some other domain of nature', but rather 'ways of organizing our thoughts or saving the phenomena that have been observed' (Hacking, ibid.). Or to put the same point in terms of the distinction that I drew earlier: the unobservable therapods are serving as unifiers of the phenomena—in particular, of the fossilized tracks—but they could not possibly be used as tools for producing new phenomena. Of course, the reason for this is the same as the reason why no one can observe a living dinosaur: they no longer exist.

Having thus distinguished between the two roles that unobservables can play in scientific research, I will turn now to the abductive arguments for realism.

3. Two basic arguments for realism: Devitt and Hacking

I begin with a version of what Hacking calls 'the experimental argument for realism' (1983, p. 265), and Nola calls 'the argument … from manipulability' (2002, p. 9). It goes like this:

1. Scientists can interact with unobservable x's (e.g. electrons and positrons) and thereby alter observable conditions in predictable and systematic ways.
2. The fact that scientists are thus able to control the observable by means of the unobservable x's would be inexplicable if those x's were not real.

3. Therefore, the unobservable x's are probably real.

Leaving aside the question of how powerfully this argument supports ERM and ERE, it clearly does not support HRM or HRE at all. Our experimental interventions may give us some reason to think that electrons are real—and no reason for thinking that N-rays, gemmules, and angels are. However, no such interventions will justify the belief that mastodons, as opposed to one-eyed giants, were real. Scientists also believe that therapod dinosaurs were real while *Brontozoa*—the humongous, flightless, Mesozoic poultry that were posited by the Reverend Edward Hitchcock in 1858 in order to explain the largest of the footprints he found in the Connecticut River Valley—were not (Hitchcock, 1974, pp. 178–179). But the argument from manipulability contributes nothing to the justification of that belief. Since the experimental argument for realism is unavailable in this context, the cumulative case for realism with respect to historical science is going to be somewhat weaker then the case for realism with respect to experimental science.

A more promising argument for historical realism is the stripped-down inference to the best explanation that Michael Devitt has called the 'basic' abductive argument for realism, in order to distinguish it from the so-called inference to the best explanation of the success of science. Here is Devitt's argument:

> The basic argument for the unobservable entities is simple. By supposing they exist, we can give good explanations of the behavior and characteristics of observed entities, behavior and characteristics which would otherwise remain completely inexplicable. Furthermore, such a supposition leads to predictions about observables which are well-confirmed; the supposition is "observationally successful". Abduction thus takes us from hypotheses about the observed world to hypotheses about the unobservable one. (1991, pp. 108–109)

Elsewhere Devitt stresses that whether or not it explains the empirical success of science, realism itself is observationally successful (1991, p. 114). I have just two observations to make about this argument. First, it is available to those who want to defend realism with respect to historical science, so the case for HRE and HRM is at least as good as Devitt's basic argument. Second, these two arguments for realism—Devitt's abductive and Hacking's experimental argument—correspond neatly to the two roles that unobservables can play in science. Hacking's argument is available whenever the unobservables are serving as tools for the production of new phenomena, and Devitt's is available whenever the unobservables are serving to unify the phenomena. That is why Devitt's argument is available in the context of historical science. The cumulative case for scientific realism is therefore strongest when these to arguments converge on the same conclusion, which will happen whenever the unobservables are playing both roles simultaneously.

One possible response to all this would be to deny that Hacking's argument lends any extra support to realism in the first place. Then the case for both experimental and historical realism would rest entirely with Devitt's basic argument. Someone who takes this view would need to explain what, if anything, is wrong with Hacking's argument, and why that argument lends no extra credence to

experimental realism. However, even this would not show that the case for historical realism is just as good as the case for experimental realism, because it could turn out that Devitt's basic argument, taken alone, lends more support to the one view than to the other. Although I will not argue the point here, it is at least worth considering the possibility that tiny things, events, and processes are better explanatory unifiers than past things, events, and processes. If that were true, then Devitt's basic argument would provide more justification for realism about the tiny than for realism about the past.

4. The classical abductive argument for realism: Boyd

Perhaps the most enthusiastically endorsed, most roundly criticized, and most widely discussed argument for scientific realism is the inference to the best explanation of the empirical success of science, which most people trace back to Hilary Putnam's (1978) famous "no miracles" argument and J. J. C. Smart's (1963) "cosmic coincidence" argument. This argument has undergone numerous refinements over the years, and it has been subjected to harsh criticism by Laudan (1981) and others, only to be reformulated and reaffirmed—most recently, for example, by Leplin (1997).

One of the most highly regarded versions of the no miracles argument is that offered by Richard Boyd, who has repeatedly stressed the dialectical interaction between scientific theory and methods: the use of heavily theory-dependent methods leads to the development of better theories, which in turn leads to methodological refinements, and so forth. Boyd says that a theory is instrumentally reliable if it yields true predictions about phenomena, and that methods are instrumentally reliable if scientists who employ them end up accepting instrumentally reliable theories. His abductive argument for realism then proceeds as follows:

> According to the realist, the only scientifically plausible explanation for the reliability of a scientific methodology that is so theory-dependent is a thoroughgoingly realistic explanation: scientific methodology, dictated by currently accepted theories, is reliable at producing further knowledge precisely because, and to the extent that, currently accepted theories are relevantly approximately true. (Boyd, 1990, p. 223)

In the passage just quoted, Boyd is primarily addressing selective skeptics, such as Van Fraassen, who reject realist epistemology. Boyd's argument is that if we did not already have a good deal of theoretical knowledge of unobservables, then the instrumental reliability of theory-dependent methods would be inexplicable.

Boyd's dialectical version of the no miracles argument is, I think, most plausible in the case of experimental science. The more we know about things that are unobservably tiny, the more ingenious will be our experimental apparatus and designs. The better our experimental apparatus and designs, the better able we will be to test our theories about the unobservably tiny things, while being careful to rule out false positives and false negatives. Since the experimental methods are so heavily

theory-dependent, their success at producing instrumentally reliable theories would be a miracle if the original theories were not approximately true. It is no coincidence that in one of Boyd's classical statements of his view, the whole argument is couched in terms of experimental methods, experimental design, and the assessment of experimental results (1985, pp. 4–6). For example, the experiment described by Hacking, in which positrons and electrons were sprayed at a niobium ball, was heavily dependent upon background theories about electrons, positrons, and much else. The success of those experimental methods at producing an instrumentally reliable theory of quarks and fractional electric charges would be a mystery if the original background theories were not true or approximately true. For Boyd, this dialectical interaction between theories and experimental methods is what clinches the case for realism. Devitt, who accepts this argument though he does not take it to be the most powerful one in the realist's arsenal, captures the idea nicely when he says that 'not only are scientists learning more and more about the world', but that they are also 'learning more and more about how to find out about the world' (1991, p. 163). How does this dialectical argument fare in the context of historical science, where the unobservables can only serve as unifiers of the phenomena, and never as tools for the production of new phenomena? I argue that it does not fare quite so well. Perhaps this is no surprise, since it is not clear that Boyd ever intended his argument to be used outside the context of experimental science.

In order to see why the argument does not support historical realism as powerfully as it supports experimental realism, we need to look a bit more closely at its structure. Boyd's dialectic is as shown in Fig. 1. The arrows in this diagram represent dependence relations. The argument is that the success of our theory-dependent methods (B) at producing instrumentally reliable theories (C) would be inexplicable if our theories about unobservables (A) were not approximately true, or if ERE were false. The question is whether there are any cases of historical science involving a dialectical process that is analogous to the one invoked by the argument for experimental realism. If the dialectic in Fig. 2 were present, then it might be possible to generate a convincing Boyd-style argument for historical realism. Consider Padian & Olsen's experimental work, described earlier. Their method involved the testing of biomechanical generalizations by having flightless birds make tracks in different substrates at different gaits and speeds, and then using those generalizations to draw conclusions about ancient therapod dinosaurs. These methods (B) are, to be sure, heavily dependent upon theories about the past. They depend on (A) theories of taphonomy, evolutionary theory, biomechanical theories, and so forth. Padian & Olsen's claim that their inferences about ancient therapods are warranted because birds and therapods are closely related phylogenetically involves a number of assumptions about past evolutionary processes. What's more, these theory-dependent methods enable them to identify a smoking gun for the hypothesis (C) that therapods had an upright posture. The trouble is that this last hypothesis does not have a very high degree of instrumental reliability. When conjoined with the biomechanical generalizations, it does imply some accurate predictions about observable therapod tracks, but the set of

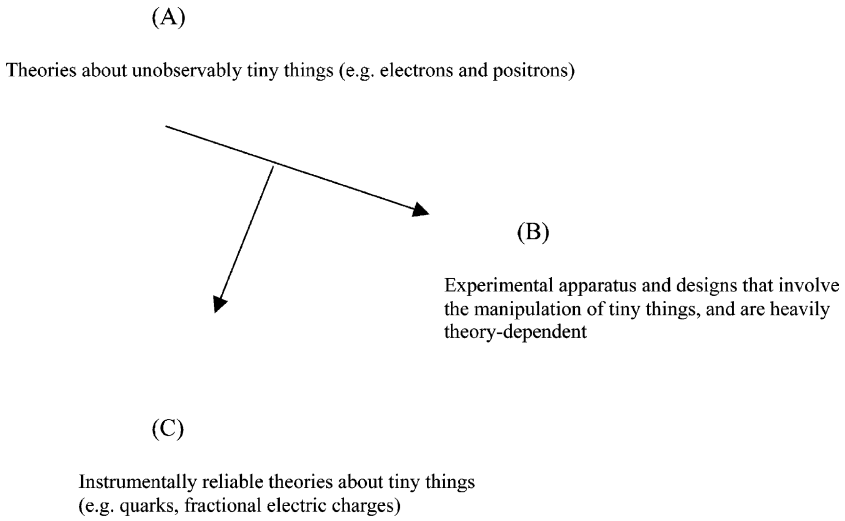

Fig. 1.

phenomena predicted by this hypothesis about ancient therapods is coextensive with the set of phenomena that are unified by the hypothesis. Since the tracks are the only relevant traces we have or will ever have, the hypothesis about therapod posture does not lead to any *novel* predictions in either the temporal sense or the more important epistemic sense—that is, predictions of phenomena that are different from the phenomena that the hypothesis was originally introduced to explain

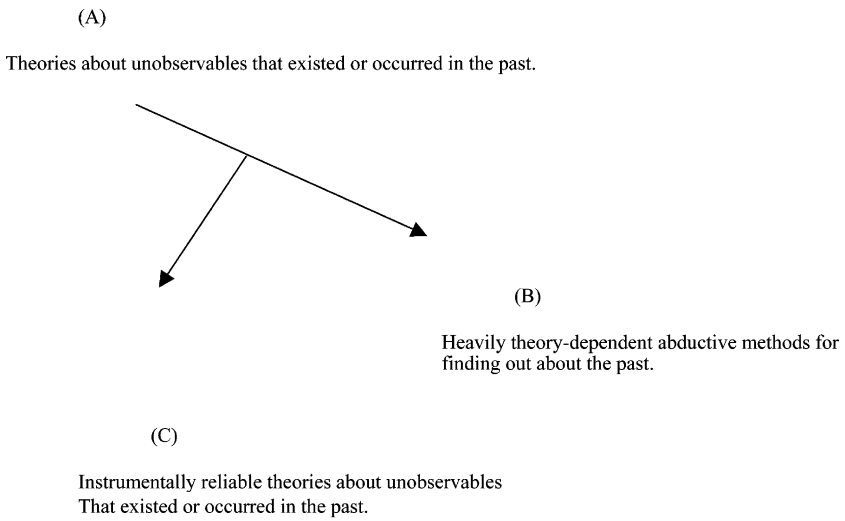

Fig. 2.

(see, e.g., Leplin (1997)). The whole argument would be much stronger if the resulting theory (C) were more instrumentally reliable—that is, if it implied novel predictions, or even just predictions of phenomena that can be reproduced at will under experimental conditions.

Another important thing to see is that in this case, in contrast to the case described by Hacking, the theory/method dialectic is not going to continue. The hypothesis that therapods had erect posture has not helped scientists continue to improve their methods for finding out about the past. One would be hard pressed to identify any current paleontological methods that depend on the theory that therapods had erect posture. While the Boyd-style dialectic is not completely absent from this episode of historical science, neither is it present in all of its richness.

Here then is one initial, though admittedly not very powerful reason for thinking that Boyd's inference to the best explanation of the success of theory-dependent scientific methods will lend less support to historical than to experimental realism. It could turn out that in cases where the unobservables in question existed or occurred in the past, the Boyd-style dialectic between theory and method will only be partial. In order to make this argument stick, it would be necessary to consider a much wider range of examples of theories about past unobservables. This turns out not to be necessary, however, because there is a much more powerful reason for thinking that Boyd's inference to the best explanation lends more support to experimental than to historical realism.

I come now to the main argument of the paper. Laudan (1981, pp. 45–46) has suggested that Boyd's argumentation is viciously circular. This objection is restated nicely by James Ladyman, who writes that 'since it is IBE involving unobservables that is in question in the realism debate, it is circular to appeal to the explanatory power of scientific realism at the meta-level to account for the overall success of science because realism is itself a hypothesis involving unobservables' (2002, p. 218). However, if we are considering the dialectical argument for experimental realism, Boyd would seem to have an adequate reply to this objection: he could point out that IBE involving unobservables is *not* the main thing in question. The theory-dependent methods (B) that figure so prominently in the argument for experimental realism are *not* in the first instance abductive methods; they are, rather, experimental methods, involving the design of experimental apparatus and the use of unobservables to produce new observable effects in the laboratory setting. By setting up the argument as an inference to the best explanation of the success of theory-dependent experimental methods—methods which are not abductive, because they involve using unobservables to produce phenomena, rather than positing unobservables as unifiers of the phenomena—the realist can avoid the charge of vicious circularity. However, the abductive argument from the success of theory-dependent methods will indeed be viciously circular if it is offered as an argument for historical realism. The reason for this is simply that all of our knowledge of past unobservables is abductive to begin with.

If experimental methods were abductive, then the Boyd-style argument for experimental realism would be just as vulnerable to the charge of vicious circularity as the argument for historical realism. How reasonable is it, then, to claim that

experimental methods are not abductive? It would be misleading to suggest that experimenters never reason abductively—for example, that they never infer to the best explanation of experimental results. However, Boyd's dialectical argument for experimental realism is best interpreted as an inference to the best explanation of the success of what experimenters *do*. The experimental methods whose success the realist hopes to explain are methods for building apparatus, manipulating test conditions, and (as Hacking would put it) intervening in nature. It is possible to defend experimental realism in a way that avoids the charge of vicious circularity, so long as one emphasizes the practical nature of the experimental methods.

In order to avoid misunderstanding, it is worth pointing out that realists may be vulnerable to two different charges of circularity: a general objection and a more specific one. The general objection, advanced by philosophers such as Van Fraassen (1980), is that realists are not entitled to the use of abductive arguments at all. For example, Van Fraassen has pointed out that if the initial pool of potential explanations of some phenomenon is a *bad lot*, which is to say that all of the potential explanations in the pool are false, then it would be a mistake to conclude that the best explanation in the pool is true or even likelier to be true than not. Unless the realist can address this and other reasons for being skeptical about abduction in general, the abductive arguments for realism will beg the question against critics like Van Fraassen. For present purposes, we can leave this general objection to one side, because it has equal force against the abductive arguments for historical and experimental realism. The more specific complaint is that it is viciously circular to attempt to defend realism by arguing that realism affords the best explanation of the success of abductive methods. This more specific objection is the one that Boyd can avoid by arguing that the relevant experimental methods—the methods whose success the realist seeks to explain—are not themselves abductive.

Here, then, is the central argument of this paper:

1. Things that are unobservable because they existed or occurred in the past can only play the unifying role, and can never be used as tools for the production of new phenomena.

2. The methods used to learn about past unobservables will therefore always be abductive; that is, they will always involve inferences to the best explanatory unification.

3. Therefore, what is at stake in the debate about historical realism (HRE) is the reliability of abductive inferences to conclusions about past unobservables.

4. The classical inference to the best explanation of the success of historical science is itself an abductive inference to a conclusion about past unobservables.

5. Therefore, the classical abductive argument for historical science is viciously circular.

The dialectical defense of experimental realism is not circular in this sense.

Some philosophers might admit that the Boyd-style argument for historical realism is circular, while denying that it is viciously so. For example, it could be that the argument is rule-circular but not premise-circular, whereas only the latter kind of circularity is vicious. Or it could be that abduction is a non-transitive form of inferential justification (see, e.g., Post (1996)). However, if the only difference between two arguments A and A^*, is that A is circular while A^* is not, then presumably we would say that A^* is the better argument, even if the circularity exhibited by A is not vicious. Therefore, even if the circularity of the Boyd-style argument for historical realism is not a sufficient reason for rejecting that argument altogether, it is still a reason for thinking that it is weaker than the corresponding argument for experimental realism.

I have already defended premise 1 of the above argument; let me now say something brief by way of a defense of premise 2. The idea that patterns of historical inference are by and large abductive is nothing new (see, e.g., Cleland (2002), as well as Gould (2002, pp. 103 ff.)). If scientists suppose that there are/were unobservable x's because that is the best way to unify the observable phenomena, then those scientists are reasoning abductively. The hypothesis that there are unobservable x's is being advanced as the best potential explanation of the phenomena, and what makes it the best is its power to unify. Where hypothesized unobservables play the unifying role, our reasoning about them can only be abductive. The subconclusion 3 follows clearly from 1 and 2, while 4 is a premise that all parties accept already.

5. Conclusion

Both scientific realists and their critics have too often taken scientific realism to be a view about things that are unobservable either because of their great distance from us or, more often, because of their small size relative to us. There is some danger here of confusing the genus (realism) with the species (experimental realism). If the arguments I have made here are sound, then realists have a better motive for doing this than their critics, because the case for experimental realism is stronger than the case for historical realism. I have given two main arguments for thinking that this is so.

First, when we consider the two basic abductive arguments for realism—Michael Devitt's stripped-down argument and Ian Hacking's experimental argument—we find that each one corresponds to one of the possible roles that unobservables can play in science—namely, the unifying role and the producing role. The cumulative case for realism will be strongest when these two arguments converge, which is to say that it will be strongest when the unobservables in question are playing both of their potential roles. This can happen in the case of tiny things, but it can never happen in the case of past things. The reason for this, as I argued in Section 3, is that past things can never be used as tools for the production of new phenomena, whereas tiny things can play the unifying role as well as the producing role.

Second, when we consider the classical abductive argument for realism, it turns out that the argument depends on there being a certain kind of dialectic between scientific theory and scientific method. This dialectic, as Boyd (1985) argues, is present in many if not most cases involving unobservably tiny things, and it is present at least partially in many episodes of historical science as well. However, there is some reason to doubt whether the dialectic is ever fully present in cases involving past unobservables. I have not tried to settle this issue here, but it is something that warrants further exploration.

There is a much deeper problem with any and all abductive arguments for historical realism. Since what is at stake in the disagreement between those who accept and those who reject HRE and HRM is precisely the trustworthiness of abductive inferences to conclusions about past unobservables, and since the realists' abductive arguments for HRE and HRM are themselves abductive inferences to conclusions about past unobservables, all of those arguments are vulnerable to the charge of vicious circularity. The arguments for experimental realism are better off, since the methods whose success is to be explained are experimental methods, rather than abductive patterns of reasoning.

The fact that the abductive arguments give less support to historical realism than to experimental realism does not necessarily mean that historical realism is unjustified or that experimental realism is justified. Arguments for some conclusion C may be sufficient to justify it, even though they are weaker than similar arguments for a different conclusion, C^*. On the other hand, arguments for the conclusion C may be insufficient to justify it, and yet still be better than similar arguments for C^*. The take-home message is that realists must not be permitted to suppose that by presenting an argument for experimental realism they have thereby defended realism in general, and in every case they must be clear about whether their realism is a view about the past or about the tiny.

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