



Historical geology: Methodology and metaphysics

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ABSTRACT

This chapter engages critically with Carol Cleland’s recent work in the philosophy of historical science. Much of the practice of historical geology fits her description of the methodology of “prototypical historical science” quite well. However, there are also important kinds of historical scientific research that do not involve what she calls the search for the smoking gun. Moreover, Cleland’s claim that prediction is not a major factor in historical natural science depends on taking an overly restrictive view of what counts as a prediction. Finally, Cleland’s approach, which emphasizes methodology, is just one possible way of thinking about the difference between historical and nonhistorical science. Rather than focusing on the “how” of historical science, one can also focus on the “what” of historical science—on the nature of the processes and events that historical geologists study.

INTRODUCTION

In his contribution to the original volume on *The Fabric of Geology* (Albritton, 1963), George Gaylord Simpson argued that geology is largely a historical science. At that time, the logical empiricists had set the agenda for the philosophy of science. The deductive-nomological model of scientific explanation was on everyone’s mind, and there was lively discussion about the nature of historical explanation. At issue was the degree to which historical research in geology and other fields fit the logical empiricist picture of natural science.

Hempel and Oppenheim’s (1948) deductive-nomological (D-N) model of scientific explanation provided the impetus for much of the discussion of historical natural science in the 1950s and 1960s. Hempel (1942, 1965 [his chapter 9]) argued that historical explanations typically involve sequences of sketches of D-N explanations—a view that seemed to relegate historical science to second-class status. But thinkers such as W.B. Gallie (1959) argued that there is a distinctive mode of historical expla-

nation, which at the time was often called “genetic explanation.” Gallie and others argued that genetic explanation was perfectly respectable even though it doesn’t fit the D-N mold. (See Dray, 1954, and Goudge, 1958, for other versions of this view.) Simpson, too, went along with Gallie in arguing that there is a distinctive mode of historical explanation:

The question “How come?” is peculiar to historical science. . . . Answers to this question are *the* historical explanations. (Simpson, 1963, p. 35)

Kitts (1963) also sided with Gallie and Simpson in holding that a distinctively historical mode of explanation prevails in geology. On the other hand, Watson (1969) pushed back against this view, arguing,

Simpson is wrong in his conclusion that explanations in historical geology are not made according to natural laws, that is, they are not examples of the covering-law model of explanation. (Watson, 1969, p. 488)

This earlier debate about the nature of historical explanation remains very much in the background of current discussions of philosophical issues in historical science, and some of the issues

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remain live ones. For example, one issue that was in play during this earlier debate was whether historical explanations support predictions.

Much has changed since 1963, however, both in philosophy and in geology. In the philosophy of science, the scientific realism debate raged during the 1980s and 1990s, and continues in some quarters today. That debate pitted scientific realists against a variety of empiricists, pragmatists, instrumentalists, and social constructivists. Few of the major players in the realism debate paid any attention to historical science (Turner, 2007). For the most part, the earlier debate about the nature of historical explanation fell off of philosophers' radar screens.

Over the past few years, there has been yet another shift within the philosophy of science toward the study of scientific practice. Rather than starting with a general picture of how science works (or is supposed to work) and then asking how well particular fields fit that picture—a top-down approach—many philosophers these days prefer to start at the bottom with a careful characterization of some limited domain of scientific practice. The work of Robert Frodeman, a trained geologist as well as a philosopher, provides an early example of this shift. Frodeman (1995) argued that geology is a hermeneutic or interpretive science. Carol Cleland's influential account of "prototypical historical science" is another excellent example of this new way of doing things (Cleland, 2001, 2002, 2011). Her work offers a natural point of departure for philosophers who wish to explore some of the issues and themes that occupied Simpson and the other contributors to the original *Fabric of Geology* volume.

In this chapter, accordingly, I engage critically with Cleland's work. I'll identify some limitations—some varieties of historical research that do not fit her description of "prototypical historical science." I'll also challenge her claim that historical science typically does not involve prediction. Most importantly, I will draw a distinction between *methodologically historical science* and *metaphysically historical science*. The take-home message is that there are different ways or respects in which scientific work can be historical. Cleland's account of prototypical historical science focuses on methodology—roughly, on *how* historical research is done. But one can also focus on *what* it is that historical researchers study, on the nature of historical processes, or on the nature of the unobservables that historical scientists posit. I'll argue that both of these are potentially valuable ways of thinking about the historical side geology. Thus, my aim here is not to reject Cleland's account of prototypical historical science, but rather to argue that it's not the whole story.

METHODOLOGICALLY HISTORICAL SCIENCE

Cleland (2001, 2002, 2011) draws a contrast between "prototypical historical science" and "classical experimental science." She treats these as ideal types, and acknowledges that particular episodes of scientific practice may be mixed, in the sense that they incorporate some experimental and some historical work. According to her picture, prototypical historical and classical ex-

perimental science differ in two fundamental (and related) ways: First, they have different kinds of target hypotheses, and second, they have different modes of testing—that is, of confirming or disconfirming—those target hypotheses.

According to Cleland, the target hypotheses of classical experimental science have to do with regularities among event types. By contrast, the target hypotheses of prototypical historical science involve claims about token events and processes in the past. An example of the former would be the claim that an iron bar expands when heated. An example of the latter would be the claim that an asteroid struck Earth ca. 65 Ma. This distinction between two kinds of target hypotheses has a long history; it's really just the distinction between particular and general statements, a distinction which has always been central to logic. In the late nineteenth and early twentieth centuries, neo-Kantian philosophers, such as Wilhelm Windelband and Heinrich Rickert, defended a distinction between *nomothetic* and *idiographic* science. Nomothetic science (as the term 'nomos' suggests) aims to discover the laws of nature, whereas idiographic science seeks to document and explain particular sequences of events. This is not to say that historical scientists don't care about generalizations at all. In fact, in reply to Cleland's work, Ben Jeffares (2008) has recently emphasized the role that generalizations play in facilitating historical inferences. Cleland's point, however, is that there is a difference between *using* generalizations and *testing* them; scientists might use experimental methods to test a generalization, and then use that generalization in the course of doing historical work. She thinks that historical science is basically idiographic, in the sense that the target hypotheses are generally claims about particular events.

Cleland's main insight is that different types of target hypotheses seem to call for different styles of confirmation and disconfirmation. Experimentalists test generalizations by holding fixed as many variables as possible and manipulating the value of the one variable of interest. They may conduct a series of trials in order to rule out false positives and false negatives. Historical scientific research, by contrast, has more in common with detective work. Scientists begin by formulating different historical hypotheses that would, if true, explain the occurrence of some set of historical traces. Then they seek a "smoking gun"—a trace or set of traces that is explained by one of those rival hypotheses but not the other.

Much of the work that historical geologists do seems to fit Cleland's description of the search for the smoking gun. I'll discuss some of the limitations of her view in the next section. However, the following example will help to illustrate some of the respects in which her account gets the scientific practice right.

In the town in Connecticut where I live, there is a remarkable basalt formation, known locally as the Higganum dike. It's a low ridge or levee-like structure ~25–30 m wide that runs for many miles in a southwest to northeast direction. About 200 Ma, as the supercontinent Pangaea was breaking up, what is now the northeastern U.S. underwent a process of rifting and faulting. Raymo and Raymo (2001, p. 99) suggest that the Hartford and Newark basins, at that time, would have looked a lot like the rift valleys

of eastern Africa today. At that time, the Higganum basalt was formed by an intrusion of hot magma into the surrounding rock.

Although the Higganum basalt has not been studied too intensively, geologists from the University of Connecticut have tried to test hypotheses about how it was formed. Did the basalt formation result from a vertical flow of magma, upwards through fissures created by rifting? Or did it result from a lateral flow of magma, say from a prehistoric volcano? There is some evidence—e.g. from observations of volcanoes in Iceland—that the latter process can produce formations that look a lot like the one in Higganum. Philpotts and Asher (1994) set out to find a smoking gun that could reveal something about the original direction of magma flow. They collected rock samples from different parts of the Higganum basalt and looked at a number of features, such as the rotation of phenocrysts and the presence of ramp structures near the contact between the basalt flow and the surrounding rock. These mostly structural traces in the rock showed that the magma had moved outward from a central fingerlike plume coming up from the Earth's mantle, but there was also clear evidence of backflow. And this was just one segment of the larger basalt dike. Philpotts and Asher argue that each segment might have been formed in much the same way, and that the larger dike was caused by a series of regularly spaced volcanic plumes, or as they put it, “a linear array of intrusive fingers.” What they found was a smoking gun that confirmed a modified version of the lateral flow hypothesis. Cleland's account of the methodology of historical science gets things right in this case. It helps clarify the nature of confirmation in this and other relevantly similar cases of historical work in geology.

HISTORICAL SCIENCE THAT ISN'T “PROTOTYPICAL”

One limitation of Cleland's characterization of prototypical historical science is that there is quite a lot of broadly historical natural science that does not fit her description very well (Turner, 2009). Because she never claims that all historical science involves a search for the smoking gun, the examples I'll discuss in this section should not be construed as counterexamples against her view. But the examples do show that historical research in geology and other fields involves a variety of different methods.

Paleontologists often describe their work as an attempt to draw inferences from patterns in the fossil record to conclusions about the underlying evolutionary processes that generated those patterns. Since the 1970s and 1980s, some of the most innovative work in evolutionary paleontology has involved looking at huge sets of data, often including thousands of fossil specimens, in order to discern interesting patterns. The use of databases has greatly facilitated this kind of research, an approach that Michael Ruse (1999, p. 214) has aptly termed “crunching the fossils.” In this sort of work, there just aren't any smoking guns—unless, of course, you stretch the notion of a smoking gun beyond recognition, so that a pattern in a huge set of data might count as one. To give one example, paleontologists have noted that the fauna that

shows up in the fossil record immediately after mass extinction events tends to have smaller bodies than the fauna that precedes those extinction events. This pattern in the historical record is known as the “Lilliput effect” (Harries and Knorr, 2009). Now, there is no one fossil specimen—no smoking gun—that establishes the reality of the Lilliput effect. None could, because the Lilliput effect is just a pattern that one sees in a vast number of fossil specimens. Nor could any one fossil discovery serve as a smoking gun for one of the various hypotheses about what sorts of evolutionary processes could give rise to the Lilliput effect.

A second limitation of Cleland's approach is that it makes no reference to the role that modeling plays in historical science. Michael Weisberg (2007) helpfully frames the issues by drawing a contrast between modeling, which he defines as the “indirect theoretical investigation of a real world phenomenon using a model” (p. 209), and *abstract direct representation*. Weisberg conceives of these as two different and complementary strategies of scientific theorizing. Scientists who practice abstract direct representation formulate theories and hypotheses that represent some real-world system directly. Then they try to test those theories and hypotheses. Modelers proceed less directly by constructing model systems that are thought to represent the real-world systems in some important respects, and then studying the properties of those model systems. Cleland's account of prototypical historical science is clearly an account of abstract direct representation; the practice of modeling does not figure into her account at all.

Historical scientists frequently rely on modeling, and examples are prolific. For example, an important turning point in the development of paleontology occurred in the early 1970s, when a group of scientists meeting at the Marine Biological Laboratory (MBL) in Woods Hole, Massachusetts, developed the first computer simulation of macroevolutionary processes (Raup et al., 1973). This model, which came to be known as the MBL model, was one of the first developments of what some historians and philosophers take to have been a “revolutionary” episode in paleontology in the 1970s and 1980s, an episode chronicled by Sepkoski (2012). For a second example, numerical modeling has also played a significant role in geological science. In the 1960s, Mikhail Budyko, a Soviet geologist, developed the first model of a runaway ice-albedo feedback effect. Budyko showed that as ice sheets expand, they reflect an ever larger amount of the sun's energy back into the atmosphere, which can lead to further cooling, which, in turn, can lead to further expansion of the ice sheets. This helped to inspire what came to be known as the “snowball Earth hypothesis” (Hoffman et al., 1998). As computing power has increased and numerical modeling has become more sophisticated, scientists have continued to use modeling to investigate the snowball Earth scenario. For instance, Hyde et al. (2000) used a climate simulation to show that a snowball Earth scenario would be compatible with a belt of open water near the equator—a place where life could have survived. Others (e.g., Oreskes et al., 1994) have commented on the role that modeling plays in the Earth sciences. Even Simpson (1963, p. 39) noted that models play a role in historical science.

In addition to computer simulation and numerical modeling, there are also rich traditions in paleontology and archaeology of building present-day models of ancient and prehistoric items. For example, scientists have built models to study the acoustical properties of the cranial crests of duckbilled dinosaurs (Weishampel, 1997). Paleontologists sometimes also use living organisms as models for extinct ones. In one study, scientists had living birds run through mud and then studied the resulting trackways in order to learn something about the relationships between stride length and running speed (Padian and Olsen, 1989). Experimental archaeologists build models of ancient tools and then try using them in order learn something about life in the past (Coles, 2010). In recent years, a team of scientists at Michigan State University has been using experimental models to investigate Gould's (1989) famous claim that evolutionary history is contingent. Richard Lenski and his colleagues have realized that you can "replay the tape" of evolution in the lab using multiple populations of evolving *E. coli* bacteria (Lenski and Travisano, 1994; Travisano et al., 1995).

Cleland might say that the instances of modeling described above are hybrid forms of scientific practice that incorporate some historical and some experimental science. Many of the examples just described involve experimentation, whether the actual manipulation of real-world model systems or "virtual" experimentation using simulations. But the ultimate goal in all of these cases is to reconstruct the past. The problem is that Cleland treats both prototypical historical science and classical experimental science as examples of abstract direct representation. Modeling doesn't come into play anywhere in her discussion of scientific methodology.

Cleland herself might well be amenable to the claim that her account of prototypical historical science gives us a picture of one especially common way of doing historical science, but that there are other interesting ways of studying the past. At no point does she claim that searching for the smoking gun is the only way to study the past. It's worth noting, however, that her larger agenda is to show that prototypical historical science is just as good as classical experimental science, from an epistemological perspective. As long as that larger agenda focuses on prototypical historical science, it will leave out some important varieties of historical research.

CLELAND'S NON-PREDICTIVISM ABOUT HISTORICAL SCIENCE

In some of her more recent work, Cleland (2011) has emphasized one other methodological difference between historical and experimental science. The latter, but not the former, involves prediction. She writes:

Predictions are traditionally construed as being in principle *logically derivable* from target hypotheses plus pertinent background information (which may be general as well as circumstantial). Successful predictions of this sort, however, rarely play a central role in scientific decisions to accept hypotheses about bygone token events. (2011, p. 8; emphasis added)

Cleland is in good company here, as many other scientists and philosophers have claimed that prediction has little or no role to play in the testing of historical hypotheses (compare, e.g., Gallie, 1959; Frodeman, 1995, p. 966; Gould, 2002, p. 102). I'll call this view *non-predictivism* about historical science. For her part, Cleland associates prediction with the style of empirical testing that one finds in classical experimental science.

One of Cleland's favorite examples of a historical hypothesis is the claim by Luis and Walter Alvarez (in Alvarez et al., 1980) that a meteorite collided with Earth ca. 65 Ma, contributing to the extinction of the dinosaurs (see Chapter 1, this volume). The discovery of unexpectedly high levels of iridium in the K-T (Cretaceous-Paleogene) boundary clay served as a smoking gun. Only two of the going hypotheses about the causes of the end-Cretaceous extinctions—volcanism and extraterrestrial impact—could explain the occurrence of the iridium. Thus, the iridium was a bit of evidence that counted in favor of these two hypotheses and against the others. Most significantly for present purposes, Cleland argues that no one could have predicted the high levels of iridium in the K-T boundary clay from the hypothesis that a meteorite collided with Earth 65 Ma. The problem is that although some extraterrestrial objects do contain iridium, plenty of them do not. If we think of prediction as involving deductive inference—note Cleland's use of the term "logically derivable" in the passage quoted above—then she is right to say that in this case, we have confirmation without prediction.

Cleland spells the argument out in more detail:

The Alverezes didn't predict excess iridium in the K-T boundary and then set out to find it. They stumbled upon it while exploring a different question: How long did it take for the boundary layer to be deposited? Even today scientists couldn't predict an iridium anomaly from the conjecture that a meteorite struck Earth tens of millions of years ago. Our current understanding of Earth and planetary science informs us that there are just too many highly plausible, extenuating circumstances capable of defeating an inference to an iridium anomaly from a gigantic meteorite impact, e.g. an iridium-poor meteorite, *dispersal of an initial iridium anomaly by geological processes*, and unrepresentative samples of the K-T boundary. (2011, p. 8; emphasis added)

One could say that the asteroid impact was a causally necessary, but not sufficient condition for the iridium anomaly. Cleland is basically making the same point here that W.B. Gallie made in the context of an earlier debate about historical explanation. According to Gallie (1959), historical (or as he called them, "genetic" explanations) typically cite temporally prior conditions that are causally necessary but not sufficient for later outcomes. That, he argued, is why historical explanations do not usually support predictions.

In earlier work (Turner, 2007, Chapter 5 therein), I argued that historical scientists can in many cases derive predictions from their target hypotheses, but that the predictions are often difficult to test because (among other reasons) the predicted traces are all too often degraded or destroyed by the intervening geological processes. This, in essence, is what Darwin had in mind when he predicted the occurrence of transitional forms in the fossil record

but then invoked the incompleteness of the record to explain why such forms had not been found. When Cleland refers in the passage quoted above to the “dispersal of an initial iridium anomaly by geological processes,” she is working with the same idea. But note the difference between saying (a) that one cannot predict an iridium anomaly from the hypothesis of a meteorite impact, and (b) that one *can* predict an iridium anomaly from the impact hypothesis, but the prediction isn’t risky because the expected geological signal may have been degraded or destroyed. The first point harkens back to Gallie; the second to Darwin. In the passage above, Cleland shifts back and forth between these two views. Note also that the two views are inconsistent. Either scientists can predict the iridium anomaly from the impact hypothesis, or they can’t.

Nothing prevents us from thinking of prediction in probabilistic terms. Consider the law of likelihood, which says that the evidence (E) supports one hypothesis (H_1) over another (H_2), just in case $\text{Prob}(E|H_1) > \text{Prob}(E|H_2)$. Some philosophers of science think that confirmation is a matter of comparing the likelihoods of the rival hypotheses, where the likelihood of some hypothesis (H) is the conditional probability of the evidence, given H . This likelihoodist account of confirmation gives us one way of seeing how the discovery of a smoking gun might confirm a historical hypothesis. Suppose that H_1 is the hypothesis that an asteroid collision caused the end-Cretaceous extinctions, while H_2 says that the culprit was some kind of global pandemic. The probability of an iridium anomaly given an asteroid impact is clearly greater than the probability of an iridium anomaly given a global pandemic. Now we could, if we wanted to, use the law of likelihood to specify some looser sense of “prediction.” We could say that H_1 predicts E , relative to H_2 , just in case $\text{Prob}(E|H_1) > \text{Prob}(E|H_2)$. Intuitively, it seems like there is a (looser, non-deductivist) sense in which the Alvarez impact hypothesis predicts an iridium anomaly while the global pandemic hypothesis does not.

Perhaps the best thing to say here is that given some suitably narrow sense of “prediction,” Cleland is right that prediction doesn’t play much of a role in prototypical historical science, although it does figure prominently in classical experimental science. However, in some broader, more relaxed sense of “prediction,” prediction plays a major role in virtually all empirical testing in science.

Cleland does seem to allow that prototypical historical scientists often make “prognostications” about what we should expect to observe under such-and-such conditions. She argues, however, that such prognostications are too vague to contribute much to the empirical testing of the target hypotheses of historical science. To illustrate this point, she cites some of Peter Ward’s work (discussed in Ward, 1983) on the Alvarez impact hypothesis. Ward was interested in testing the claim that the meteorite impact caused the end-Cretaceous mass extinctions. He was working on ammonites in particular, and he “predicted” that one should see ammonites disappear from the fossil record rather abruptly at the K-T boundary. While working on the Spanish coast, on the Bay of Biscay, he found that ammonites disappeared from the rock

record ~10 m beneath the K-T boundary, what he took to mean tens of thousands of years before (see Chapter 1, this volume). In later work done somewhat to the north, over the French border, he found that the ammonites persisted right up to the K-T boundary and disappeared suddenly. Cleland concedes that Ward was testing a prediction, in some weak or loose sense, but she adds the following:

Ward’s “prediction” cannot be interpreted as amounting to the claim that ammonites will be found along the northern coast of Spain, even though this is where he began his investigations, because the ammonites that made it successful were discovered in France. At best, it may be interpreted as a vague prognostication to the effect that (if the Alvarez hypothesis is true) it is *likely* that there are rocks *somewhere* on Earth with ammonite fossils immediately below but not above the K-T boundary sediments. (Cleland, 2011, p. 10; emphasis in the original)

In one way, this case resembles Darwin’s prediction of transitional forms in the fossil record. Darwin never predicted the existence of any particular transitional forms; he just pointed out that if his common ancestry thesis were correct, then we ought to find some transitional forms somewhere in the fossil record (and he fretted about the fact that none had yet been found). But Darwin’s prediction, vague though it may have been, came out right, and many scientists hold that the discovery of transitional forms, from *Archaeopteryx* to *Tiktaalik*, provides massive confirmation of Darwin’s theory (see, e.g., Asher, 2012). When vague prognostications come out right, they carry evidential weight. So why not go ahead and call them predictions? Ward’s failure to find ammonite fossils just below the K-T boundary in northern Spain can always be chalked up to the incompleteness of the fossil record. Presumably, though, Cleland would agree that when Ward’s fieldwork on the French coast paid off, that carried some positive evidential weight.

Some philosophers of science (e.g., Leplin, 1997) hold that predictions carry special evidential weight when they exhibit novelty. There is some disagreement about how exactly to analyze novelty, but the rough idea is that the hypothesis should not be tailored to accommodate the predicted result. Philosophers also disagree about whether novelty in this sense adds anything to the degree of confirmation that one gets from showing that a hypothesis has true observational consequences. But many do have the intuition that novel predictive success is the strongest kind of scientific evidence that one can produce. Tellingly, vague prognostications (in Cleland’s sense) can exhibit novelty. Take the claim that “it is *likely* that there are rocks *somewhere* on Earth with ammonite fossils immediately below but not above the K-T boundary sediments” (Cleland, 2011, p. 10). The Alvarez impact hypothesis was constructed independently of the knowledge of any sites where the ammonite fossil record peters out right at the K-T boundary. The hypothesis was not (and could not have been) tailored to accommodate the data that Ward would later gather on the French seacoast. If vague prognostications can exhibit novelty, then it’s plausible to see them as playing some role in the confirmation of historical hypotheses.

To summarize the results of this section: Cleland's non-predictivism about historical science depends on taking a narrow view of what counts as a prediction. We could, however, say that we make a prediction whenever we say something about what evidence we should find in the future if some hypothesis is true. For example, although Philpotts and Asher do not describe their work as testing predictions, one could (speaking loosely) say that the lateral versus vertical magmatic flow hypotheses make different predictions about what sorts of features one should see in the Higganum basalt. Cleland resists using the term "prediction" in this looser sense, preferring instead to talk of "prognostications." However, when prognostications come out right, they do lend support to historical hypotheses. Cleland's terminological scruples about the word "prediction" are unnecessarily restrictive. The good news is that her non-predictivism is not really essential to her account of prototypical historical science.

METAPHYSICALLY HISTORICAL SCIENCE

So far, I've argued that there are important kinds of historical scientific practice that do not fit Cleland's account of the methodology of historical science as the search for the smoking gun. These include, first, the inference from pattern to process, and second, the practices of modeling in geology, paleontology, and archaeology. This doesn't mean that Cleland's account of prototypical historical science is mistaken; a lot of historical science fits her picture quite well. But it does mean that what she has offered is just a picture of one way of studying the past. Going further, I've argued (in the previous section) that Cleland's non-predictivism about historical science only makes sense if we work with an unreasonably narrow conception of prediction. But nothing about her account of prototypical historical science entails non-predictivism. I now want to suggest that focusing on methodology isn't the only interesting or fruitful way of thinking about how historical science differs from other nonhistorical kinds of research.

Scientists in many different fields posit *unobservable* entities, events and processes in order to predict and explain observable phenomena. This basic fact about science raises many questions that philosophers have explored under the heading of the scientific realism debate. In one way or another, most of those questions have to do with whether observability imposes some kind of limit on what we can know, or what we can meaningfully talk about. In earlier work (Turner, 2007), I argued that different things can be unobservable for different reasons. Some things—the entities, processes, and properties that fundamental physicists like to talk about—are unobservable in part because of their small size relative to us. In some cases, there may be other, deeper physical reasons why those things cannot be observed. For instance, some of the properties that physicists attribute to microphysical things are just not the sorts of properties that human sense organs could ever detect. We might say that these sorts of things are spatially or physically unobservable. Many other things are temporally unobservable. We can't observe them

because they occurred long ago, or because they don't exist anymore. We cannot observe the volcanic events that formed the Higganum basalt dike 200 Ma. This difference between historical and other sorts of scientific work has less to do with methodology and more to do with metaphysics. The difference, in other words, is less a matter of how the science is done, and has more to do with the objects of scientific study.

The distinction between the unobservably past and the unobservably tiny would not be very interesting if there were no other relevant difference between these two categories of unobservables. But there is one major difference: physicists can and do manipulate entities, processes, and events at the microphysical level. For example, the purpose of particle accelerators such as the Large Hadron Collider¹ is to carry out controlled interventions in the microphysical world. "Unobservable" does not imply "unmanipulable," because scientists can design experimental apparatus in the light of our best theories about what the unobservable microphysical world is like. The situation with respect to historical science is very different, however, since no one can intervene in the past. This *asymmetry of manipulability* has important methodological and normative consequences (see Turner, 2007, for the extended argument), but the asymmetry itself is really a metaphysical one. According to this view, the variety of methods that one sees in historical science—from Cleland's search for the smoking gun, to paleontologists' attempts to infer process from pattern, to the use of numerical models to study geological processes—all have one thing in common: all are ways of studying the past without being able to manipulate it.

Consider once again the example of Philpotts and Asher's work on the Higganum basalt dike. As we saw earlier, their work is methodologically historical insofar as the scientists are engaging in what Cleland describes as the search for the smoking gun. The work is also historical in what I am here calling a metaphysical sense. The scientists are positing unobservable past geological events in order to make sense of the presently observable traces. They obviously cannot intervene in those events that took place 200 Ma.

Not only is history unmanipulable; it also exhibits a certain degree of contingency. Simpson (1963) observed that geologists often study contingent historical processes.

The actual state of the universe or any part of it at a given time, its configuration, is not immanent and is constantly changing. It is *contingent* in Bernal's (1951) term, or *configurational* as I prefer to say (Simpson, 1960). History may be defined as configurational change through time. (Simpson, 1963, p. 24–25, emphasis in the original)

Simpson even went so far as to define historical science as the "determination of configurational sequences, their explanation, and the testing of such sequences and explanations" (1963, p. 25). The current arrangement of the continents is a good example of what Simpson means by a configuration. The movement

¹This, the world's largest and highest-energy particle accelerator, is located near Geneva, Switzerland.

of the continents over geological time is a kind of configurational change. The later configurations depend upon—or are contingent upon—the earlier ones. If the configuration at some earlier time had been even slightly different, then the downstream configurations would also be different. Stephen Jay Gould (1989) would later go on to popularize this idea that history is contingent, or sensitive to variations in earlier conditions and configurations. Subsequently, the notion of historical contingency has received a lot of attention from philosophers of biology (Beatty, 1995, 2006; Turner, 2010, 2011 [chapter 8]; Desjardins, 2011). Although Gould was primarily interested in evolutionary history, geological processes also seem to exhibit a degree of historical contingency. For example, the Higganum basalt formation that Philpotts and Asher studied is a good example of a configuration whose existence is highly sensitive to initial conditions. Had there been no rifting or faulting in the region 200 Ma, the Higganum basalt dike would not exist at all. Not all geological processes are contingent. Contingency comes in degrees (Ben-Menahem 1997), and the degree of contingency in geological processes is an empirical question.

Contingency and unmanipulability are both features of some of the processes that geologists study. They have more to do with the “what” of historical geology than with the “how,” although they surely also have methodological implications. The main claim I want to make is the rather modest one that there are different senses in which historical geology might be historical. There’s the methodological sense that’s captured by Cleland’s account of prototypical historical science. But that’s not the only sense in which some parts of geology might qualify as historical science. The processes that historical geologists are interested in have these additional features of unmanipulability and (in many cases) contingency.

CONCLUSION

Anyone who wants to think about what it means for certain kinds of geological research to be historical would do well to take Cleland’s work as a starting point. While acknowledging the value of her account of prototypical historical science, I’ve also tried to show here that her view has three limitations. First, there are important varieties of historical research that do not fit her characterization of “prototypical historical science.” Second, her claim that prediction does not play much of a role in historical science depends on taking an excessively narrow view of what counts as a prediction. Third, there are other ways of thinking about what’s distinctive about historical science that focus more on the “what” rather than the “how” of historical science, more on the metaphysics rather than the methodology.

The arguments of this chapter are pluralistic in spirit: There are a variety of different methods of studying the past. Searching for a smoking gun to discriminate among hypotheses about past events is one important method, but it’s not the only one. And there are a variety of different ways, both methodological and metaphysical, in which natural science can be historical.

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REFERENCES CITED

- Albritton, C.C., Jr., ed., 1963, *The Fabric of Geology*: Reading, Massachusetts, Addison-Wesley, 372 p.
- Alvarez, L.W., Alvarez, W., Asaro, F., and Michel, H.V., 1980, Extraterrestrial cause for the Cretaceous-Tertiary extinction: *Science*, v. 208, p. 1095–1108, doi:10.1126/science.208.4448.1095.
- Asher, R.J., 2012, *Evolution and Belief: Confessions of a Religious Paleontologist*: Cambridge, UK, Cambridge University Press, 300 p.
- Beatty, J., 1995, The evolutionary contingency thesis, in Wolters, G., and Lennox, J.G., eds., *Concepts, Theories, and Rationality in the Biological Sciences*: Pittsburgh, Pennsylvania, University of Pittsburgh Press, p. 45–81.
- Beatty, J., 2006, Replaying life’s tape: *The Journal of Philosophy*, v. 103, no. 7, p. 336–362, doi:10.5840/jphil2006103716.
- Ben-Menahem, Y., 1997, Historical contingency: *Ratio*, v. 10, p. 99–107.
- Bernal, J.D., 1951, *The Physical Basis of Life*: London, Routledge and Kegan Paul, 80 p.
- Cleland, C., 2001, Historical science, experimental science, and the scientific method: *Geology*, v. 29, p. 987–990, doi:10.1130/0091-7613(2001)029<0987:HSESAT>2.0.CO;2.
- Cleland, C., 2002, Methodological and epistemic differences between historical and experimental science: *Philosophy of Science*, v. 69, p. 474–496, doi:10.1086/342455.
- Cleland, C., 2011, Prediction and explanation in historical natural science: *The British Journal for the Philosophy of Science*, v. 62, no. 3, p. 551–582, doi:10.1093/bjps/axq024.
- Cleland, C.E., 2013, this volume, Common cause explanation and the search for a smoking gun, in Baker, V.R., ed., *Rethinking the Fabric of Geology*: Geological Society of America Special Paper 502, doi:10.1130/2013.2502(01).
- Coles, J.M., 2010, *Experimental Archaeology*: Caldwell, New Jersey, Blackburn Press, 286 p.
- Desjardins, E., 2011, Historicity and experimental evolution: *Biology and Philosophy*, v. 26, p. 339–364, doi:10.1007/s10539-011-9256-4.
- Dray, W., 1954, Explanatory narrative in history: *The Philosophical Quarterly*, v. 4, no. 14, p. 15–27, doi:10.2307/2217274.
- Frodeman, R., 1995, Geological reasoning: Geology as an interpretive and historical science: *Geological Society of America Bulletin*, v. 107, no. 8, p. 960–968, doi:10.1130/0016-7606(1995)107<0960:GRGAAI>2.3.CO;2.
- Gallie, W.B., 1959, Explanations in history and the genetic sciences, in Gardiner, P., ed., *Theories of History*: Glencoe, Illinois, The Free Press, p. 386–402.
- Goudge, T.A., 1958, Causal explanations in history: *The British Journal for the Philosophy of Science*, v. 9, no. 35, p. 194–202, doi:10.1093/bjps/IX.35.194.
- Gould, S.J., 1989, *Wonderful Life: The Burgess Shale and the Nature of History*: New York, W.W. Norton, 347 p.
- Gould, S.J., 2002, *The Structure of Evolutionary Theory*: Cambridge, Massachusetts, Harvard University Press, 1433 p.
- Harries, P.J., and Knorr, P.O., 2009, What does the ‘Lilliput Effect’ mean?: *Palaeogeography, Palaeoclimatology, Palaeoecology*, v. 284, p. 4–10, doi:10.1016/j.palaeo.2009.08.021.
- Hempel, C.G., 1942, The function of general laws in history: *The Journal of Philosophy*, v. 39, p. 35–48, doi:10.2307/2017635.
- Hempel, C.G., 1965, *Aspects of Scientific Explanation and Other Essays in the Philosophy of Science*: New York, Free Press, 505 p.
- Hempel, C.G., and Oppenheim, P., 1948, Studies in the logic of explanation: *Philosophy of Science*, v. 15, p. 135–175, doi:10.1086/286983.
- Hoffman, P.F., Kaufman, A.J., Halverson, G.P., and Schrag, D.P., 1998, A Neoproterozoic snowball Earth: *Science* v. 281, no. 5381, p. 1342–1346.
- Hyde, W.T., Crowley, T.J., Baum, S.K., and Peltier, W.R., 2000, Neoproterozoic ‘snowball Earth’ simulations with a coupled climate/ice-sheet model: *Nature*, v. 405, p. 425–429, doi:10.1038/35013005.
- Jeffares, B., 2008, Testing times: Regularities in the historical sciences: *Studies in History and Philosophy of Biological and Biomedical Sciences*, v. 39, p. 469–475.

- Kitts, D.B., 1963, Historical explanation in geology: *The Journal of Geology*, v. 71, p. 297–313, doi:10.1086/626903.
- Lenski, R.E., and Travisano, M., 1994, Dynamics of adaptation and diversification: A 10,000 generation experiment with bacterial populations: *Proceedings of the National Academy of Sciences of the United States of America*, v. 91, p. 6808–6814, doi:10.1073/pnas.91.15.6808.
- Leplin, J., 1997, *A Novel Defense of Scientific Realism*: Oxford, UK, Oxford University Press, 204 p.
- Oreskes, N., Shrader-Frechette, K., and Belitz, K., 1994, Verification, validation, and confirmation of numerical models in the earth sciences: *Science*, v. 263, no. 5147, p. 641–646, doi:10.1126/science.263.5147.641.
- Padian, K., and Olsen, P., 1989, Ratite footprints and the stance and gait of Mesozoic theropods, in Gillette, D.D., and Lockley, M., eds., *Dinosaur Tracks and Traces*: Cambridge, UK, Cambridge University Press, p. 231–242.
- Philpotts, A.R., and Asher, P.M., 1994, Magmatic flow-direction indicators in a giant diabase feeder dike, Connecticut: *Geology*, v. 22, p. 363–366, doi:10.1130/0091-7613(1994)022<0363:MFDIIA>2.3.CO;2.
- Raup, D.M., Gould, S.J., Schopf, T.J.M., and Simberloff, D., 1973, Stochastic models of phylogeny and the evolution of diversity: *The Journal of Geology*, v. 81, p. 525–542.
- Raymo, C., and Raymo, M.E., 2001, *Written in Stone: A Geological History of the Northeastern United States*: Hensonville, New York, Black Dome Press, 163 p.
- Ruse, M., 1999, *Mystery of Mysteries: Is Evolution a Social Construction?*: Cambridge, Massachusetts, Harvard University Press, 296 p.
- Sepkoski, D., 2012, *Rereading the Fossil Record: The Growth of Paleobiology as an Evolutionary Discipline*: Chicago, Illinois, University of Chicago Press, 440 p.
- Simpson, G.G., 1960, The history of life, in Tax, S., ed., *Evolution after Darwin*, vol. 1: Chicago, Illinois, University of Chicago Press, p. 117–180.
- Simpson, G.G., 1963, Historical science, in Albritton, C.C., Jr., ed., *The Fabric of Geology*: Reading, Massachusetts, Addison-Wesley, p. 24–48.
- Travisano, M., Mangold, J.A., Bennett, A.F., and Lenski, R.E., 1995, Experimental tests of the roles of adaptation, chance, and history in evolution: *Science*, v. 267, no. 5194, p. 87–90, doi:10.1126/science.7809610.
- Turner, D., 2007, *Making Prehistory: Historical Science and the Scientific Realism Debate*: Cambridge, UK, Cambridge University Press, 223 p.
- Turner, D., 2009, Beyond detective work: Empirical testing in Paleobiology, in Ruse, M., and Sepkoski, D., eds., *The Paleobiological Revolution: Essays on the Growth of Modern Paleontology*: Chicago, Illinois, University of Chicago Press, p. 201–214.
- Turner, D., 2010, Gould's replay revisited: *Biology and Philosophy*, v. 26, p. 65–79, doi:10.1007/s10539-010-9228-0.
- Turner, D., 2011, *Paleontology: A Philosophical Introduction*: Cambridge, UK, Cambridge University Press, 227 p.
- Ward, P.D., 1983, The extinction of the ammonites: *Scientific American*, v. 249, p. 136–147, doi:10.1038/scientificamerican1083-136.
- Watson, R.A., 1969, Explanation and prediction in geology: *The Journal of Geology*, v. 77, p. 488–494, doi:10.1086/628374.
- Weisberg, M., 2007, Who is a modeler?: *The British Journal for the Philosophy of Science*, v. 58, p. 207–233, doi:10.1093/bjps/axm011.
- Weishampel, D.B., 1997, Dinosaurian cacophony: Inferring function in extinct organisms: *Bioscience*, v. 47, p. 150–159, doi:10.2307/1313034.

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