

# Prediction and Explanation in Historical Natural Science

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## ABSTRACT

In earlier work (Cleland [2001], [2002]), I sketched an account of the structure and justification of ‘prototypical’ historical natural science that distinguishes it from ‘classical’ experimental science. This article expands upon this work, focusing upon the close connection between explanation and justification in the historical natural sciences. I argue that confirmation and disconfirmation in these fields depends primarily upon the explanatory (versus predictive or retrodictive) success or failure of hypotheses vis-à-vis empirical evidence. The account of historical explanation that I develop is a version of common cause explanation. Common cause explanation has long been vindicated by appealing to the principle of the common cause. Many philosophers of science (e.g., Sober and Tucker) find this principle problematic, however, because they believe that it is either purely methodological or strictly metaphysical. I defend a third possibility: the principle of the common cause derives its justification from a physically pervasive time asymmetry of causation (a.k.a. the asymmetry of overdetermination). I argue that explicating the principle of the common cause in terms of the asymmetry of overdetermination illuminates some otherwise puzzling features of the practices of historical natural scientists.

- 1 *Introduction*
  - 2 *The Methodology of Historical Natural Science*
  - 3 *Justification in Historical Natural Science*
    - 3.1 *Prediction*
    - 3.2 *Adequate historical explanations are not potential predictions*
    - 3.3 *The centrality of common cause explanation*
  - 4 *Common Cause Explanation and the Asymmetry of Overdetermination*
    - 4.1 *The priority of common cause over separate causes explanations*
    - 4.2 *The threat of information degrading processes*
-

## 1 Introduction

An inspection of any recent issue of *Nature* or *Science* quickly reveals that historical research is common in natural science, occurring in fields as diverse as paleontology, geology, biology, planetary science, astronomy, and astrophysics. Some celebrated examples are: the hypothesis that the continents were once joined together into a super continent (Pangaea), which explains surprising patterns of ‘frozen’ magnetism found in certain ancient igneous rocks; the Alvarez meteorite-impact hypothesis, which explains the startlingly high concentrations of iridium and shocked quartz found in the mysterious K–T (Cretaceous–Tertiary) boundary marking the end of the fossil record of the dinosaurs; and the ‘big-bang’ theory of the origin of the universe, which explains the mysterious isotropic, 3 K, background radiation first detected in the mid-1960s. Given the increasingly high profile successes of the historical natural sciences, it is surprising that philosophers of science have, with the exception of evolutionary issues in biology, devoted little attention to it. This is particularly puzzling when one considers that some of the most widespread practices of the historical natural sciences do not seem to closely resemble those of stereotypical experimental science, the latter of which is commonly held up as the paradigm of ‘good’ science.

In earlier papers (Cleland [2001], [2002]), I identified fundamental differences in the methodology of ‘prototypical historical science’ and ‘classical experimental science’. The target hypotheses of prototypical historical natural science differ from those of classical experimental science in being about long-past, token events, as opposed to regularities among types of events. Hypotheses concerning long-past, token events are typically evaluated in terms of their capacities to *explain* puzzling associations among traces discovered through fieldwork. In contrast, the acceptance and rejection of hypotheses in classical experimental science depends upon the success or failure of *predictions* tested in controlled laboratory settings.<sup>1</sup> I argued that these differences in practice could be epistemically justified in terms of a pervasive time asymmetry of causation. ‘The asymmetry of overdetermination’ (as it was dubbed by David Lewis [1979]) underpins the objectivity and rationality of the methodology of prototypical historical natural science, explaining why the latter is not, as sometimes maintained, inferior to classical experimental science.

<sup>1</sup> As I discussed (Cleland [2002]), philosophical investigations (e.g., Hacking, Franklin) into the methodology of experimental science have established that much of the work that goes on in experimental science does not have this character. Nevertheless, the classical conception of experimental science is the gold standard to which historical science is often held hostage.

This essay explores the intimate connection between explanation and justification in prototypical historical natural science. I begin, in Section 2, by reviewing my analysis of the practice of prototypical historical natural science, fleshing out salient details with a case study and correcting misunderstandings that have appeared in the literature. (The reader is urged to consult my earlier papers [2001], [2002] for further detail.) In Section 3, I address Turner's ([2004], [2007]) and Jeffares's ([2008]) charge that successful and failed predictions play a much more central role in the evaluation of prototypical historical hypotheses than I have acknowledged. I show that the actual practices of historical scientists do not support this claim. Historical hypotheses about particular past events are rarely rejected in the face of failed predictions and they are often accepted in the absence of successful predictions. Indeed, as we shall see, the predictions that are actually made in prototypical historical research are typically too vague for their success or failure to play central roles in the evaluation of the hypotheses with which they are associated. Someone who is still under the influence of the covering law model of explanation might retort that good explanations have the same logical structure as prediction; a truly adequate explanation is a potential prediction. I consider and reject various attempts to accommodate historical explanation within the basic framework of the covering law model. As I discuss, the evidential warrant for hypotheses in prototypical historical science is founded upon common cause explanation, which is not prediction-like in character. Even narrative explanations, which are common in much of the historical natural sciences, depend upon the identification of common causes for their empirical justification.

Common cause explanation has long been justified in terms of the principle of the common cause. Some philosophers of science (e.g., Sober [1988] and Tucker [2004]), however, find this principle highly dubious because they believe that it is either purely methodological or strictly metaphysical. I defend a third possibility in Section 4: the justification for the principle of the common cause depends upon the truth of the thesis of the asymmetry of overdetermination, which is empirically well grounded in physics, as opposed to logic or a priori metaphysics. The thesis of the asymmetry of overdetermination supplies the needed nonlogical justification for the principle of the common cause. As I show, explicating the principle of the common cause in terms of the asymmetry of overdetermination illuminates some otherwise puzzling features of the practices of scientists engaged in prototypical historical research, such as why (contra Sober and Tucker) they exhibit a general preference all other things being equal (in the absence of empirical or theoretical information suggesting otherwise) for common cause explanations over separate causes explanations.

## 2 The Methodology of Historical Natural Science

In 'Methodological and Epistemic Differences Between Historical Science and Experimental Science' (Cleland [2002]), I argued that most historical research in natural science exhibits a distinctive pattern of evidential reasoning characterized by two interrelated stages: (i) the proliferation of multiple competing hypotheses to explain a puzzling body of traces encountered in fieldwork, and (ii) a search for a 'smoking gun' to discriminate among them. A smoking gun discriminates among rival hypotheses about long-past, token events by showing that one or more provides a better explanation for the total body of evidence available than the others. As I emphasized, this pattern of evidential reasoning is not always found in the historical natural sciences, and it is sometimes found in (nonclassical) experimental research. Which pattern of evidential reasoning is exhibited depends upon a scientist's epistemic situation. It is because scientists concerned with investigating long-past, token occurrences typically find themselves in a different epistemic situation than classical experimentalists (whose focus is on tenseless regularities) that the above pattern of evidential reasoning predominates in their work.

The stages that I identified in prototypical historical natural science are not, as Kleinhans *et al.* ([2005]) assert, in conflict. The body of evidence on the basis of which a collection of rival hypotheses is formulated does not include the smoking gun that subsequently discriminates among them. A smoking gun represents a piece of additional evidence that wasn't available at the time the hypotheses concerned were formulated; undiscovered traces do not constitute actual evidence. The discovery of a smoking gun changes the evidential situation, revealing that one or more of the hypotheses under consideration provide a better explanation for the total body of evidence *now* available than the others.

The findings of historical science are just as tentative and subject to revision as those of experimental science. The original collection of competing hypotheses may be culled and augmented repeatedly in light of new evidence and/or advances in theoretical understanding. Ideally, this process converges upon a single hypothesis. But there are no guarantees. And even supposing that a scientific consensus is reached on a single hypothesis, there are no guarantees that future empirical or theoretical work won't bring to light scientifically viable new possibilities. If this happens, the previously well-accepted hypothesis will acquire a rival, and the process of searching for a smoking gun begins anew. In this context, it is important to keep in mind that the correct hypothesis may not be among those being entertained, and indeed may never be entertained by humans; historical scientists are just as limited by their imaginations as experimentalists. Moreover, even supposing that the correct explanation is among those under consideration, there are no guarantees that a

smoking gun for it will be found even supposing that one exists. Breakthroughs in historical science frequently wait upon the development of sophisticated technologies for detecting and analyzing miniscule or highly degraded traces (Cleland [2002]). In the absence of the requisite technology, historical scientists have little recourse but to resign themselves to a collection of equally viable, rival hypotheses.

The history of the debate over the end-Cretaceous mass extinction, which famously extinguished the dinosaurs, along with what is now estimated to be 75–85% of all species then on Earth, provides a particularly good illustration of the dynamic interrelation in historical natural science between proliferating alternative hypotheses and searching for a smoking gun to discriminate among them. Prior to 1980, many different explanations for the end-Cretaceous extinctions were taken seriously by paleontologists, including pandemic, evolutionary senescence, climate change, nearby supernova, volcanism, and meteorite impact (Powell [1998], p. 165). None of the evidence available at the time provided strong support for any one of these hypotheses over the others, and most paleontologists suspected that we would never know which is correct. It thus came as a surprise when the father and son team of Luis and Walter Alvarez ([1980]) discovered something momentous in the K–T boundary.

Found all over the world, the K–T boundary marks the end of the Cretaceous and the beginning of the Tertiary. It consists of a distinctive thin layer of clay sandwiched between two layers of limestone, suggesting a sudden collapse of biological activity. Geologists long suspected that it held the secret to the end-Cretaceous mass extinction, but no one knew how to unlock it. Walter Alvarez, a geologist, was interested in how long it took for K–T boundary sediments to be deposited; was the extinction event rapid or slow? His father Luis, a physicist, suggested using the element iridium as a clock since it is supplied at a known constant rate by meteoritic dust. Detecting the expected low levels of iridium required a particle accelerator, which Luis had access to at UC Berkeley. The results were staggering. Clays from the K–T boundary contained iridium levels 30 times higher than the limestones on either side. Luis's calculations showed that the amount of iridium was too great to be explained in terms of known geological processes. Subsequent tests confirmed the presence of an iridium anomaly in K–T boundary clays from around the world. At some sites in North America levels were 1,000 times higher than background.

Luis and Walter knew they were in possession of a smoking gun for the mysterious end-Cretaceous mass extinction. Earth's crust is depleted in iridium because iridium (like iron) is a heavy element and most of it sank into the mantle and core during planet formation. Although not all meteorites are rich in iridium, asteroids and comets left over from the formation of the solar

system typically have higher concentrations. So meteorite-impact was a very promising candidate for explaining the anomalous levels of iridium. But as volcanologists (e.g., Officer and Drake [1985]) pointed out, volcanism brings mantle material to the surface. Moreover, there is evidence of fissure eruptions spread over an area of at least 1 million square kilometers in the Deccan traps region of India approximately 65 mya (million years ago). Accordingly, volcanism provides an alternative possibility for explaining the iridium anomaly. None of the other competing hypotheses for the end-Cretaceous mass extinction could explain the excess iridium. The Alvarezes' discovery of anomalous levels of iridium in the K–T boundary thus functioned as a smoking gun for discriminating meteorite impact and volcanism from their pre-1980 rivals.

Further research supported meteorite impact over volcanism. Fieldwork undercut the claim that volcanism could produce a global iridium anomaly similar to that found in the K–T boundary (e.g., Schmitz and Asaro [1996]). Even more importantly, analysis of K–T boundary sediments produced a smoking gun for meteorite impact over volcanism. Large quantities of mineral grain, predominately quartz, exhibiting a highly unusual pattern (cross-hatched, parallel sets) of fractures was found in K–T boundary sediments from around the world (Bohor *et al.* [1984]). It takes enormous pressures to fracture minerals in this way. At the time, there were only two places on Earth where they were known to occur, the sites of nuclear explosions and meteor craters. Subsequent fieldwork failed to substantiate the claim that violent volcanic eruptions produce shocked minerals of this sort (Kerr [1987]; Alexopoulos *et al.* [1988]). The combination of excess iridium and shocked quartz in the K–T boundary was thus enough to convince most members of the scientific community that a huge (10–15 km wide) meteorite struck Earth 65 mya. Since this time, more evidence of meteorite impact (microspherules and fullerenes containing extraterrestrial noble gases) has been discovered in the K–T boundary. But it is generally agreed by planetary and earth scientists that the combination of an iridium anomaly and shocked minerals cinched the case early on.<sup>2</sup>

<sup>2</sup> In this context, it is worth noting that although the discovery of the Chicxulub crater, which is roughly 200 km across and straddles the northern coast of Mexico's Yucatan Peninsula, is sometimes cited as pivotal, it was not. Indeed, a few geologists are still not convinced that this is the right crater. The problem is that it is difficult to connect a local impact crater with a global extinction. In contrast, the presence of excess iridium and shocked quartz in K–T boundary sediments from around the world points to a meteorite impact with global, and hence potentially catastrophic, effects. Had the Chicxulub crater been discovered in the absence of the iridium and shocked quartz, it is unlikely that it would have been construed as compelling evidence for a meteorite-impact explanation for the end-Cretaceous extinctions. On the other hand, once the iridium and shocked quartz were discovered, it wouldn't have surprised scientists if no one had been able to locate a crater of the right size and age. Seventy percent of Earth's surface is covered by ocean, making an ocean impact more probable than a land impact, and an oceanic crater would almost certainly have been obliterated by the active geology of the seafloor,

The iridium and shocked minerals weren't enough, however, to convince paleontologists that the second prong of the Alvarez hypothesis is true—that the mass extinctions were caused by the meteorite impact. The extinctions had to be worldwide and geologically instantaneous. The available fossil evidence was very imprecise, unable to distinguish extinction events occurring within a period of a few years from those occurring at different times throughout intervals of 10,000 to perhaps 500,000 years. Moreover, some of the fossil evidence seemed to suggest that the extinctions were well underway by the time the impact occurred (Clemens *et al.* [1981]), leading some paleontologist to infer that something else (climate change, evolutionary senescence, or extensive volcanism were some popular conjectures) was at fault, and the impact, at best, delivered the *coup de grâce*. Additional fieldwork was required to establish a scientifically more compelling causal connection between the impact event and the extinction event.

Paleontologists fanned out across the globe, studying the fossil records of different kinds of organisms on either side of the K–T boundary. Peter Ward ([1990]) established that the fossil record of the ammonites goes right up to the K–T boundary and then suddenly disappears. Studies also documented substantial changes in the morphology of the calcareous shells of tiny planktonic foraminifera on either side of the K–T boundary. Paleobotanists made some of the most significant fossil discoveries. Using high-resolution techniques, they discovered abundant fossilized angiosperm (flowering plant) pollen right up to the lower level of the boundary, at which point it disappears and is replaced on the other side with abundant fossilized fern spores (Johnson and Hickey [1990]). As botanists know from experience with modern catastrophes (e.g., the explosion of Mount St Helens) ferns are opportunistic plants that quickly colonize devastated areas. These detailed fossil studies from around the world indicated that the extinction was massive (involving many different kinds of organisms), rapid, and catastrophic. Most paleontologists were won over to the second prong of the Alvarez hypothesis, illustrating that a smoking gun may consist of a large and diverse body of new evidence.

The remarkable cross-disciplinary, scientific consensus that was finally achieved on the Alvarez hypothesis stands as one of the crowning achievements of historical natural science. As a consequence it provides a particularly compelling case study for illustrating my account of the methodology of

which moves in conveyor like fashion away from mid-ocean ridges, where it forms, to the margins of continents, where it sinks back into the mantle at subduction zones. Indeed, many geologists who were convinced by the iridium and shocked quartz that a devastating meteorite impact occurred around 65 mya were pleasantly surprised when a crater of the right size and age was identified straddling a landmass.

prototypical historical natural science. In subsequent discussions of the epistemic justification of these methods, I will frequently return to it.

### 3 Justification in Historical Natural Science

The focus of this section is on the function of explanation and prediction in the evaluation of scientific conjectures about long-past, token events. I argue that explanation, as opposed to prediction (or for that matter retrodiction), plays the central role in the acceptance and rejection of hypotheses in prototypical historical science. Someone who is still under the influence of the covering law model of scientific explanation (and this includes a surprising number of philosophers!) might retort that all truly adequate explanations constitute potential predictions. I reject this claim. As I discuss, most historical explanations appeal to causal relations, as opposed to logical relations of deducibility. With a few notable exceptions, causal accounts of explanation are open to (without endorsing) the possibility of explanatory relations between causes and effects that do not come under either deterministic or probabilistic/statistical laws of nature. It is my contention that the evidential warrant for conjectures about long-past, token events is grounded in common cause explanation. Even narrative explanations, which are widespread in the historical sciences, depend upon common cause explanations for their empirical plausibility. This helps to explain the close connection between explanation and justification in the reasoning of scientists engaged in prototypical historical research.

#### 3.1 Prediction

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. The iridium anomaly, which played such a pivotal role in the acceptance of the first prong of the Alvarez hypothesis, provides a good illustration. 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 huge 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 (exposed outcrops of which



are rare).<sup>3</sup> This helps to explain why the hypothesis that the great Permian extinction of 250 mya was caused by a meteorite impact is still taken seriously despite the failure of scientists to identify an iridium anomaly associated with it (Erwin [2006], Chapter 2).

Nor can the acceptance of hypotheses about the remote past be interpreted as a matter of retrodiction. Contemporary scientists do not have the requisite background knowledge to logically infer a meteorite impact from the discovery of an iridium anomaly anymore than they have the knowledge to logically infer an iridium anomaly from a meteorite impact. Geological processes are just as capable of concentrating material that was originally dispersed as they are of dispersing material that was once concentrated. A good example is provided by placer deposits, which are formed when flowing water picks up weathered minerals from widely separated locations and mechanically transports them to a central location, where they fall out as the flow of water slows (sorted according to weight) and form localized enrichments of minerals. Half of all gold ever discovered comes from placer deposits. Moreover, many other geological processes (e.g., hydrothermal processes, magmatic processes, and precipitation) working separately and together can produce enrichments of minerals. This is not to deny that scientists sometimes reason from traces to long-past causes. As we shall see in the next section, however, the reasoning involved is that of inference to the best explanation, which does not have the same logical structure as retrodiction or prediction.

Many historical scientists and some philosophers, however, seem to have a weaker notion of prediction in mind. Peter Ward, for instance, characterized his ammonite studies as testing a 'prediction' [his term] of the (second prong of the) Alvarez hypothesis for the end-Cretaceous extinctions. As with the iridium anomaly, even today no one could logically infer the extinction of the ammonites from the Alvarez hypothesis on the basis of the current state of our scientific knowledge. Some animals (including the ammonites' close relatives the nautilus) made it through the end-Cretaceous extinction event, and it isn't at all clear what made the difference between those that survived and those that didn't. Some paleontologists believe it was just a matter of luck. Similarly, one cannot retrodict the Alvarez hypothesis from the extinction of the ammonites. Extinctions are caused by a wide variety of different circumstances; indeed, most biologists believe that we are currently in the midst of a major (human caused) extinction event. The question is what do 'predictions'

<sup>3</sup> It is worth noting that the explanatory power of historical hypotheses vis-à-vis the evidence that supports them cannot be interpreted as a matter of accommodation. The evidence (smoking gun) that cinches the case for an historical hypothesis over its rivals is frequently discovered after the hypothesis was formulated. The Alvarez hypothesis provides a good illustration. The hypothesis did not originate with the Alverezes, despite the fact that it now bears their name. It was propelled from the backburner to the front burner of geological science with the discovery of positive evidence (an iridium anomaly) that such an event actually happened.

such as Ward's really amount to and what role do they actually play in the practices of historical natural scientists?

The history of Ward's ammonite studies is revealing. He began on the Spanish side of the Bay of Biscay, whose sea cliffs contain abundant ammonites and some of the best exposed, well preserved outcrops of the geological section containing the K–T boundary in the world. The closest ammonite he could find to the lower level of the boundary was 10 m beneath it, leading him to suspect that they had become extinct thousands of years earlier (Ward [1983]). Serendipitous (as it turned out!) encounters with armed Spanish soldiers and disgruntled Basques eventually motivated him to change location, and he moved a short distance up the coast to France, where to his surprise he found abundant ammonites extending right up to the boundary. Apparently, the ammonites in what is now northern Spain suffered an ecological crisis during the late Cretaceous but continued to thrive just a few miles up the coast, in what is now southern France. Ward ([1990]) concluded that the fossil record of the ammonites supported the Alvarez hypothesis after all.

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 K–T boundary sediments. Given the rarity of exposed, well-preserved rock records of the pertinent geological age containing both ammonites and the K–T boundary, and the threat of ecological crises such as the one that extinguished the ammonites in Spain, it is impossible to assign even a rough numerical probability to this forecast. This helps to explain why Ward's failure to find ammonites close to the K–T boundary in Spain was not decisive in refuting the Alvarez hypothesis, and why he was open to finding ammonites elsewhere despite his failure to encounter them in Spain.

The debate over the snowball Earth hypothesis provides another good illustration of a *prima facie* precise prediction that in hindsight was clearly vague. Somewhat ironically, Derek Turner ([2004], [2007], Chapter 7) cites it as a rare but telling case in which an historical hypothesis was rejected on the basis of a failed novel prediction. According to the snowball Earth hypothesis, the entire planet, from the equator to the poles, was covered in ice for several million years on several different occasions during the neoproterozoic (ca. 850–555 mya). Some physical geologists suspect that an event this extreme would produce a planet-wide 'hydrological shutdown,' which provides the basis for a novel prognostication: geological sections of the pertinent age should reveal periods during which no sediments were formed (because no weathering occurred). Leather and colleagues ([2002]) set out to test this

'prediction' in northern Oman, which is one of the few places on Earth where one can find neoproterozoic deposits of the right age. But they didn't find evidence of a hydrological shutdown. They discovered bands of glacial debris interspersed with and broken up by layers of sediment deposited over a fairly short time period.

The paleogeological community did not, however, respond to Leather and his colleagues' discovery in the manner proposed by Turner by rejecting the snowball Earth hypothesis. In the first place, the concept of a snowball Earth was never as definite as Turner seems to think. From the beginning, there was disagreement about whether the planet was almost or completely covered in ice (were there any areas of open ocean?), how hard the freeze was (slushy or frozen solid at the equator?), how long individual episodes lasted, etc. Second, the claim that a snowball Earth would produce a planet-wide hydrological shutdown, in which no sedimentary (including glacial) deposits are formed for a long period of time, was based upon climate models incorporating a large number of somewhat speculative background assumptions about atmospheric, oceanic, and continental conditions and processes, e.g., atmospheric CO<sub>2</sub> levels, extent of sublimation processes over sea-ice, thickness of sea-ice cover, thickness of continental ice sheets, varieties of nonhydrological processes of chemical weathering, length of total freezes, paleoaltitude and tectonic evolution of the continents, frequency of interglacial/nonglacial periods, etc. As a consequence, the claim that no sediments would be deposited during a snowball Earth episode was open to question. Third, there was the problem of interpreting what is found at a unique geological site; subsequent geological processes may intermingle material deposited at different times, producing misleading rock records and radiometric ages. Given these background considerations, it should come as no surprise that the debate over the snowball Earth hypothesis continues to this day, with some researchers (e.g., Fielding *et al.* [2006]) contending that although the glaciations were nearly planet-wide, they were of short duration, alternating with longer periods of warmer, interglacial conditions, and that sublimation of sea-ice drove a significantly diminished (but not fully shut down) water cycle.

As Turner concedes, cases in which historical hypotheses are rejected on the basis of failed predictions are the exception rather than the rule. He pins the problem on the difficulty of 'testing' novel predictions in historical science. In so doing, he implicitly endorses the widely accepted view that the practices of stereotypical experimental science provide the prototype for all of science. It is thus hardly surprising that he concludes that the historical sciences are epistemically disadvantaged vis-à-vis the experimental sciences (Turner [2004], [2007]).

But as I have argued, the actual practices of historical natural scientists provide little support for this widespread view. Historical scientists frequently

fail to reject hypotheses in the face of predictive failure. This is of course also true of experimental scientists who are always faced (*à la* the Duhem-Quine thesis) with the possibility that a false auxiliary hypothesis (versus the target hypothesis) is responsible for a failed prediction. But experimental scientists can always hope to improve the situation by retesting the target hypothesis and controlling for suspicious auxiliary assumptions. Historical scientists are in a very different epistemic situation because they cannot perform controlled experiments on their target hypotheses, and (as the debate over the snowball Earth hypothesis underscores) they are faced with an enormous number of worrisome auxiliary assumptions given the length and complexity of the time spans involved. It is thus hardly surprising that failed predictions do not count much against the truth of the prototypical historical hypotheses investigated by natural scientists. Instead, scientists reject hypotheses about particular past events on the grounds that another hypothesis does a much better job of explaining the total body of evidence available.

The fate of the contagion hypothesis for the extinction of the dinosaurs provides a salient illustration. It cannot be viewed as refuted by the discovery of an iridium anomaly in the K–T boundary because, as the scientists involved would readily admit, the presence of iridium in the context of their background understanding of Earth history does not provide evidence that the dinosaurs did not go extinct as a result of an epidemic shortly before or after the impact. What the presence of iridium does is provide positive support in the form of independent evidence for either volcanism on a massive scale or the impact of a huge meteorite, either of which has the capacity (under the right circumstances) to produce a mass extinction. It is thus not an accident that scientists did not speak of the contagion hypothesis as being ‘refuted’ by the discovery of iridium in the K–T boundary. Instead they simply stopped talking about the contagion hypothesis and moved on to the pressing question of whether volcanism or a meteorite impact provides the best explanation for the iridium anomaly.

Vague prognostications that succeed, on the other hand, sometimes carry great weight in prototypical historical natural science. But it is not in virtue of representing a successful prediction that they do so. If (analogously to the Alvarezes’ serendipitous discovery of an iridium anomaly in K–T boundary sediments) Ward had accidentally stumbled upon ammonite fossils immediately below the K–T boundary in France, instead of having deliberately gone looking for them there, his findings wouldn’t be any less significant. Regardless of the circumstances in which they are acquired, traces function as a smoking gun if they can be used to establish that one hypothesis provides a better explanation for the total body of evidence available than its rivals. A scientific consensus on the meteorite-impact hypothesis for the K–T extinctions was achieved because it *explains* an otherwise puzzling body of traces

(e.g., shocked quartz, glassy spherules, etc., and fossil records of ammonites, foraminifera, plant pollen, fern spores, etc.) better than any of its competitors. Some of this evidence was discovered while pursuing vague predictions and some of it was discovered serendipitously. The Alvarez hypothesis explains this extensive and diverse body of evidence better than any of its currently available, scientifically plausible competitors. It is for this reason that the Alvarez hypothesis currently dominates scientific thought about the end-Cretaceous mass extinction.

Viewed in light of the above, the vague ‘predictions’ of historical natural scientists appear to play a very different role in their research than the role played by prediction in classical experimental science. Instead of specifying conditions for testing and evaluating target hypotheses, the prognostications of historical natural scientists serve as tentative guides—educated guesses, based informally upon both theoretical and empirical background knowledge—about where additional evidence (ideally, a smoking gun!) *might* be found for a hypothesis and perhaps even what form it *might* take. Ward’s vague prediction suggested where to look for evidence for the second prong of the Alvarez hypothesis as well as what form it might take. He eventually got lucky while pursuing it. As Leather and colleagues’ research on the snowball Earth hypothesis underscores, not everyone is so fortunate. Even the Alvarezes’ discovery of an iridium anomaly may be interpreted as guided by an extremely vague (tacit) prediction. Walter Alvarez took samples from the K–T boundary because, like many geologists, he believed that crucial evidence for what caused the end-Cretaceous extinctions might be found there even though no one at the time had any idea what form it might take. Unfortunately, it is beyond the scope of this article to explore this proposal in further detail.

### 3.2 Adequate historical explanations are not potential predictions

At one time, the emphasis on explanation over prediction in scientific decisions to accept and reject historical hypotheses wouldn’t have been viewed as significant. For on the traditional covering law model of scientific explanation (Hempel [1965]; Hempel and Oppenheim [1948]), explanation and prediction have the same logical structure. The prototype for the covering law model, the D-N (deductive-nomological) model, analyzes explanations as deductively valid arguments whose premises are statements of general law and (sometimes but not always) initial conditions, and whose conclusions are statements of the phenomenon (event, fact, or regularity) to be explained. Every adequate explanation thus constitutes a potential prediction (Hempel [1965], p. 367). In order to accommodate statistical or probabilistic laws, Hempel augmented

the covering law model with the D-S (deductive statistical) and I-S (inductive statistical) models of explanation; he assumed that there are as yet unknown logical principles of inductive inference analogous to those of deductive inference. All three models analyze explanations as arguments in which the explanatory burden rests upon laws of nature.

Historical explanation was a problem for the covering law model from its inception. The covering law model places the explanatory burden on laws of nature. Laws (whether deterministic or statistical/probabilistic) that are strong enough to logically license deductive or inductive inferences between token events must be universal (within the pertinent domain of discourse) and exceptionless. Explanations in the historical sciences rarely invoke even rough generalizations of this sort. The long causal chain stretching between a prehistoric event and its contemporary traces is just too complex, involving the intersection of many independent causal processes, to be captured in a plausible generalization of the kind required by the covering law model; scientifically compelling statistical or probabilistic laws require reliable information about frequencies, which is rarely available, particularly in cases involving uncommon events such as mass extinctions. Hempel was fully aware of these difficulties. His solution was to demote historical explanations to mere 'explanatory sketches' (Hempel [1965], pp. 235–40), thus reinforcing the widespread view that the historical natural sciences are inferior to the experimental sciences. Hempel attributed the undeniably compelling nature of some historical explanations to the tacit assumption of partially specified laws and background conditions. On Hempel's view successful historical explanations are incomplete arguments with gappy premises functioning as promissory notes.

The covering law model no longer dominates philosophical thought about scientific explanation. Yet many philosophers and scientists implicitly accept it. Kleinhans and colleagues' ([2005]) discussion of explanation in the geological sciences provides a good illustration. They insist that the approximate generalizations of contemporary geology (both historical and nonhistorical) are in principle reducible to the stricter generalizations of chemistry and physics. In their words, 'earth science generalizations, such as the cited example regarding earthquakes, describe contingent distributions and processes which can be reduced "locally" because they can be *exhaustively* translated in physical and/or chemical terms' (p. 295, emphasis added). There is little empirical support, however, for the claim that strict traditional laws, disguised by a welter of contingencies, underlie the restricted, exception-ridden generalizations of the historical sciences. Geologists are notoriously bad at predicting earthquakes even for extensively studied, local regions of well-mapped fault systems such as the San Andreas. Moreover, there are reasons to believe that even the laws of physics and chemistry may not be as universal and exceptionless as commonly thought (Cartwright [1983]).

A philosophically more popular strategy for transforming the rough generalizations of the special sciences into universal, exceptionless truths is to tack on *ceteris paribus* clauses. The basic idea is to blame their sketchiness on the complexity of their subject matter—in the case of historical science, on a surfeit of unknown or poorly understood contingencies spanning the time frame between a hypothesized ancient event and the evidence that it supposedly explains. As Sandra Mitchell ([2000], [2002]) points out, however, to be scientifically compelling *ceteris paribus* laws require knowledge of some contingencies, i.e., specific conditions upon which the applicability of the generalization depends. This poses a particularly serious problem for the historical sciences. Scientists just don't know enough about all the things that might happen in the temporally extended causal chain linking a postulated long-past cause to its present day traces to determine what should be included in an approximate generalization and what should be consigned to a *ceteris paribus* clause. Merely insisting, as an article of faith, that the rough generalizations of the historical sciences can be fleshed out in this manner doesn't help much because scientists can't actually use the conjectured *ceteris paribus* laws to generate prediction-like explanations with the requisite precision.

In light of the inadequacies of reductive and *ceteris paribus* accounts of the rough generalizations of the special sciences, Mitchell proposes reconceptualizing law-of-nature to include degrees of contingency or, in her words, 'stability over changes in context' (Mitchell [2002], p. 334). The laws of physics exhibit the greatest (but not perfect<sup>4</sup>) stability and the laws of the special sciences the least. In this way she hopes to preserve the function traditionally ascribed to natural laws in prediction and explanation.

Ben Jeffares ([2008]) endorses Mitchell's weaker concept of law-of-nature and argues that the investigation of such laws is just as central to historical science as the search for a smoking gun. In his words, 'the historical sciences also seek regularities in the world and *have to* in order to secure their claims about the past' (p. 470, emphasis added). According to Jeffares, historical scientists require generalizations 'directly' linking prior causes to their present day effects in order to make predictions, the success or failure of which he contends is just as crucial to the evaluation of historical hypotheses as it is to the evaluation of experimental hypotheses.

There is little doubt that historical scientists deploy generalizations from the experimental sciences in analyzing and interpreting traces discovered in the field. A salient example is the use of radiometric dating methods, which are

<sup>4</sup> Mitchell's account is designed to accommodate Nancy Cartwright's ([1983]) provocative claim that the most basic of the known laws of physics fail to conform to the strict, traditional notion of law of nature; on her view, the laws of fundamental physics may not be as universal and exceptionless as traditionally portrayed.

grounded in the highly stable, statistical laws of quantum theory. It is clear, however, that generalizations of this sort play a secondary role in historical research. They are not the targets of historical research but rather useful tools borrowed from other disciplines for special purposes. It is also true that historical scientists sometimes investigate much less stable, special purpose regularities in laboratory settings. Jeffares cites archaeologists ‘experimenting’ with differences in marks produced by dogs gnawing bones and humans using primitive tools to butcher animals as an example. As Jeffares concedes, however, this regularity is being pursued as a means to an end, as opposed to an end in itself (p. 470). Archaeologists are seeking a tool (analogous to radiometric dating methods) for analyzing evidential traces discovered in the field.

An even more serious problem is that the generalization being pursued by Jeffares’ archaeologists isn’t truly historical. It doesn’t *directly* subsume token causes and effects separated by indefinitely long distances in time. With the passage of time marks on bones become less distinct, making it increasingly difficult to discriminate those produced by animals from those produced by tools. This fact is somewhat obscured by the (geologically speaking) short time span of our species but may be readily appreciated by entertaining the plight of paleontologists tens of millions of years from now trying to use the same generalization to differentiate among marks on fossilized bones. As discussed earlier, regularities holding between cause and effect event types whose tokens are separated by protracted intervals of time tend to be very fragile. Each link in the causal chain represents a causal liability (an opportunity for interference), and the longer the time span, the greater the number of contingencies that the generalization must accommodate. As a consequence it is not only difficult to identify generalizations capable of directly linking long-past causes to their present day traces, most generalizations that are identified are extremely unstable (in Mitchell’s sense). One cannot infer predictions capable of playing pivotal roles in the actual evaluation of hypotheses from generalizations saddled with such high degrees of contingency. Jeffares’ mistake is in thinking that he can retain the explanatory power of prediction (*à la* the covering law model) with a much weaker notion of natural law.

The purpose of the preceding discussion has been to establish that historical explanations in natural science cannot be interpreted as potential predictions. The dominant contemporary philosophical theories of historical explanation place the explanatory burden on causal features of the world, as opposed to natural laws. Indeed, in keeping with certain contemporary metaphysical accounts of causation, some causal theories of explanation leave open the possibility of explanatory causal relations that do not come under natural laws of any sort (e.g., Salmon [1993] and Anscombe



[1971]). In any case, however, it is clear that scientists sometimes acquire a good understanding of how an event was caused without being able to predict its occurrence. A good example is the inability of seismologists to successfully predict earthquakes even though they are able to explain those that do occur in remarkable detail.

### 3.3 The centrality of common cause explanation

The most common modes of causal explanation in the historical sciences are narrative explanation and common cause explanation. Narrative explanation dominates thought about explanation in human history, where intangible human desires and purposes play key explanatory roles. It is also common in evolutionary biology and historical geology. The basic idea behind narrative explanation is to construct a story—a coherent, intuitively continuous, causal sequence of events centering on a precipitating event and culminating in the phenomena (traces) in need of explanation. In some cases, the purpose is only to establish the plausibility that certain sorts of causal processes could have given rise to the phenomena concerned; at best it represents a potential explanation. In other cases, however, the narrative is interpreted as showing how the phenomena *actually* came about. Because much is unknown about the events in the sequence narrative explanations have a significant fictional component, involving omissions and additions. This poses a potential problem insofar as it conflicts with the traditional emphasis in natural science on evidential warrant. The problem is exacerbated by the central role of explanation in the confirmation and disconfirmation of historical hypotheses. If the primary reason for accepting a historical hypothesis is its explanatory power and it draws its explanatory power primarily from the coherence and continuity of a quasi-fictional story, then historical natural science really does seem inferior to experimental science; in the absence of empirical warrant a narrative explanation amounts to little more than a ‘just-so’ story.

Common cause explanation promises a solution to the problem of evidential warrant faced by narrative explanations in natural science. The basic idea behind common cause explanation is to formulate reliable inferential methods for identifying when a diversity of contemporary traces comprises the effects of a common cause token. It is thus hardly surprising that narrative explanations and common cause explanations frequently go hand-in-hand in the historical natural sciences (Kleinhans *et al.* [2005]; Hull [1992]), with common cause explanations supplying the needed empirical warrant for key events in the narrative sequence. The increasingly detailed narrative for the end-Cretaceous mass extinction provides a salient illustration. The discovery of large quantities of rain-drop shaped, glassy spherules and extensive deposits of soot and ash in K–T boundary sediments from around the world, for

instance, supports the claim that enormous quantities of rock were liquefied and vaporized during the impact (including the entire meteorite) and injected into the upper atmosphere only to fall back (after enveloping the planet) in a global rain of fire, igniting everything that could burn on the planet's surface. Another good illustration is provided by the phylogenies (evolutionary histories) constructed by biologists for species and higher taxa. The discovery of 'molecular fossils' (genomic sequences that have changed little over the eons) in living organisms has given a tremendous boost to some phylogenies while discrediting others because they provide empirical evidence of common ancestry in addition to that traditionally obtained from morphology and the fossil record.

Although the evidential warrant for narrative explanations devolves upon common cause explanations, not all common cause explanations are deployed in support of a narrative. A good illustration is paleontologist Mary Schweitzer and colleagues' ([2005]) explanation for what appears to be medullary bone inside the fossilized leg bone of a *Tyrannosaurus rex*. Medullary bone comprises a distinctive calcium rich layer that develops in the long bones of contemporary female birds during the egg laying process, providing a readily accessible supply of calcium for building eggshells. Schweitzer and her graduate student were stunned to discover an analogous layer in the fossilized leg bone of a *T. rex*. They concluded that the bone was from a female. Significantly, they did not concern themselves with the life or death of this unfortunate *T. rex*, nor did they attempt to reconstruct any of the events in the long causal chain stretching between its death and the preservation of its bone for millions of years in the Montana desert. Indeed, detailed stories of either sort seem irrelevant to their purpose, which is to evaluate the conjecture that the fossilized bone came from a female *T. rex*. To this end they studied the detailed physical structure and chemical composition of the *T. rex* bone, comparing it to the leg bones of modern female birds and appealing to well-accepted background beliefs about the close phylogenetic relationship between modern birds and dinosaurs. The point is the common cause explanation they gave for the medullary-like bone was not used to support an event in a narrative sequence and, considered just in itself, is too minimal to meet the threshold for a narrative. This underscores the centrality of common cause explanation to the evidential reasoning of historical natural scientists.

Common cause accounts of explanation are traditionally justified by appealing to 'the principle of the common cause'. The principle of the common cause is associated with the work of Hans Reichenbach ([1956]). For purposes of this article, I take the principle of the common cause to (roughly speaking) assert that seemingly improbable coincidences (correlations or similarities among events or states) are best explained by reference to a shared common

cause.<sup>5</sup> The principle of the common cause represents an epistemological conjecture about the conditions under which a certain pattern of causation may be nondeductively inferred. According to the principle of the common cause, most seemingly improbable coincidences are produced by common causes.

The principle of the common cause presupposes an ostensibly metaphysical claim about the temporal structure of causal relations among events in our universe. Genuinely improbable coincidences are rare. Most otherwise improbable coincidences are produced by common causes. In the next section, I argue that this supposition is not merely metaphysical. It is empirically well grounded in physical theory. For purposes of this section, however, the important point is that if the temporal structure of causal relations in our universe were different—if genuinely improbable coincidences were common—one would not be justified in inferring the likelihood of a common cause from a seemingly improbable association among traces.

The principle of the common cause provides a potentially powerful tool for understanding the close relationship between explanation and confirmation in the reasoning of historical natural scientists. Ostensibly improbable associations among traces are scientifically puzzling. Attributing such associations to a common cause has great explanatory power. The common cause explains the correlation or similarity by placing the traces concerned within a unified causal framework showing that their association is not improbable after all. The mystery of their concurrency is thus resolved. Attributing a mysterious association among traces to chance, on the other hand, explains nothing; we are left with an intractable mystery. The iridium and shocked quartz in the K–T boundary provide a salient illustration. Given our current understanding of geology the only event that renders their correlation in a structurally distinctive, thin layer of sediment found all over the world scientifically explicable is the impact of a huge meteor. As a consequence, the case for a meteorite impact is currently considered scientifically overwhelming. Similarly, the best explanation for the surprising structural and chemical similarities between the fossilized leg bone of Schweitzer's *T. rex* and the long bones of modern female birds is that the former was female. In short, the more improbable an association among a collection of traces seems the more psychologically appealing the claim that it is the product of a common cause. This helps to explain why historical natural scientists have a tendency to focus their investigations on what seems in light of their background beliefs to be the most unlikely (and hence puzzling) correlations or similarities among contemporary phenomena.

<sup>5</sup> Reichenbach's principle of the common cause was stronger. He claimed that improbable coincidences among events that are not related as cause to effect or effect to cause *must* be explained by reference to a shared common cause. But as the existence of separate causes explanations for some puzzling associations among traces (Sober [1988], [2001]) underscores, this is too strong.

#### 4 Common Cause Explanation and the Asymmetry of Overdetermination

Why should we believe that most ostensibly improbable associations among traces are the result of common causes? What could possibly justify such a claim? In this section, I argue that the principle of the common cause provides a global constraint on scientific reasoning that is neither purely methodological nor strictly metaphysical. The use of the principle of the common cause by historical natural scientists rests upon a substantive thesis about the nature of the world for which there exists overwhelming empirical evidence, namely, the thesis of the asymmetry of overdetermination. The thesis of the asymmetry of overdetermination provides the needed nonlogical justification for the principle of the common cause. Among other things, the asymmetry of overdetermination explains why (Sober's and Tucker's arguments notwithstanding) scientists engaged in prototypical historical research exhibit a general preference for common cause explanations over separate causes explanations for puzzling associations among traces unless they are in possession of information specific to the case at hand suggesting that a separate causes explanation is likely.

Associated with the work of David Lewis ([1979]), the thesis of the asymmetry of overdetermination asserts that events in our universe are causally connected in time in an asymmetrical manner. As fleshed out in my ([2001], [2002]), it amounts to the claim that most *localized* events *overdetermine* their past causes (because the latter typically leave extensive and diverse effects) and *underdetermine* their future effects (because they rarely constitute the total cause of an effect). The qualification that we are dealing with localized events leaves open the possibility that the asymmetry does not exist at the global scale of our universe. What matters for our purposes is that the asymmetry of overdetermination holds for local regions of space and time—the scale of the data procured by scientists in laboratories and field studies.

The overdetermination of past events by their localized future effects is epistemic because it is inferential but not causal; effects do not bring about their causes. In contrast, the underdetermination is both epistemic and causal. As an illustration of the epistemic overdetermination of past causes by localized future effects consider an explosive volcanic eruption. Its effects include extensive deposits of ash, pyroclastic debris, masses of andesitic or rhyolitic magma, and a large crater. Only a small fraction of this material is required to infer the occurrence of the eruption. Indeed, any one of an enormous number of remarkably small subcollections will do. This helps to explain why geologists can confidently infer the occurrence of long-past events such as the massive, caldera forming eruption that occurred 2.1 mya in what is now Yellowstone National Park. In contrast, inferring the occurrence of near

future events such as the next eruption of Mt. Vesuvius is much more difficult. In the first place, the present does not contain traces (records) of future events as it does of past events. Furthermore, it is well known that there are many causally relevant conditions in the absence of which an eruption won't occur, and not all of these conditions are well understood. This makes it difficult to infer even imminent eruptions with any degree of confidence. This is the other side of the asymmetry of overdetermination: most localized events (e.g., magma rising in a volcanic chamber) do not even determine (let alone overdetermine) their future effects because they rarely constitute the total cause of an effect. Viewed from this perspective the historical natural sciences seem to be epistemically advantaged vis-à-vis classical experimental science.<sup>6</sup>

As discussed in my ([2001], [2002]), the asymmetry of overdetermination is familiar to physicists. Examples such as explosive volcanic eruptions are commonly attributed to the second law of thermodynamics. The natural processes that produce volcanic eruptions are irreversible; volcanoes are never observed to swallow up the debris they produce and restore the environment surrounding them to its pre-eruption condition. The asymmetry of overdetermination also applies to wave phenomena, which do not admit of an obvious thermodynamic explanation. Although traditionally associated with electromagnetic radiation (light, radio waves, etc.), the radiative asymmetry (as it is sometimes known) characterizes all wave-producing phenomena, including disturbances in water and air. It originates in the fact that waves (whether water, sound, light, etc.) invariably spread outwards, as opposed to inwards, as time progresses, which means that the effects of a cause become increasingly widespread in space. Between the second law of thermodynamics and the radiative asymmetry, all physical phenomena above the quantum level are subject to the asymmetry of overdetermination. While it is tempting to suppose that they are somehow related—Horwich ([1989]) and Albert ([2000]), for instance, attribute them to the initial conditions at the time of the 'big bang'—the important point for our purposes is that there is overwhelming empirical evidence for both the second law of thermodynamics and the radiative asymmetry, and hence for the thesis of the asymmetry of overdetermination.

The asymmetry of overdetermination provides a nonlogical, objective foundation for the epistemic principle of the common cause. According to the thesis of the asymmetry of overdetermination, most localized cause and effect relations in our universe form many-pronged forks opening in the

<sup>6</sup> As I discuss in my ([2001], [2002]), the underdetermination of the localized future by the localized present explains why classical experimentalists spend so much time controlling for potentially interfering factors in the laboratory. There is always the threat that an experimental result represents a false positive or a false negative regardless of how carefully the experiment is 'controlled'. When one moves from the sterile, artificial environment of a laboratory to the messy uncontrollable world of nature, and tries to infer events such as the eruption of a volcano, the threat becomes even more difficult to surmount.

direction from past to future. As a consequence the present is filled with epistemically overdetermining traces of past events. This means that it is likely (but not certain) that a seemingly improbable association (correlation or similarity) among present-day phenomena is due to a common cause. If the temporal structure of causal relations in our universe were different—if most causal forks opened in the opposite direction (from future to past), or most cause and effect relations were linear (one-to-one) instead of fork-like, or most events were chance (uncaused) occurrences—one would not be justified in inferring the likelihood of a common cause from an ostensibly improbable association among traces. It isn't clear whether the asymmetry of overdetermination represents a contingent or necessary *a posteriori* truth about our universe. On some causal theories of time, for instance, forks running in the opposite direction, from future to past, are metaphysically impossible. The important point for our purposes is that there is very strong *empirical* evidence for its truth. It follows that Turner ([2004]) is wrong in claiming that the thesis of the asymmetry of overdetermination is 'strictly metaphysical' (p. 210).

The search for a smoking gun, which I have argued lies at the heart of the methodology of prototypical historical natural science, is a search for telling empirical evidence for a common cause. The overdetermination of the past by the localized present insures that such evidence is likely to exist if the traces concerned truly share a common cause. For insofar as past events typically leave numerous and diverse effects, only a small fraction of which are required to identify them, the contemporary environment is likely to contain many potential (as yet undiscovered) smoking guns for identifying the common cause of a puzzling association among traces. Because the significance of a smoking gun can be recognized only in the context of an appropriate common cause hypothesis, historical scientists proliferate alternative common cause hypotheses, rather than (as in classical experimental science) focusing on a single hypotheses, and search for telling empirical evidence showing that one of the hypotheses explains the total body of evidence available better than the others. A common cause hypothesis that explains the total body of evidence available better than any of its scientifically plausible rivals is judged the most likely to be true. Like all scientific verdicts about hypotheses, however, this judgment is defeasible in light of new theoretical or empirical developments.

Not just any common cause of a puzzling body of traces can explain it. For every collection of traces shares some common cause (e.g., the big bang of cosmology), and most subcollections of traces share many common causes. The scientifically most fruitful common cause explanations appeal to *last* (proximate) common causes. A last common cause represents the causal juncture at which the items in the collection cease to share a more recent common

cause. Because they maximize causal unity last common cause explanations have greater explanatory power than other common cause explanations.<sup>7</sup>

The widely accepted hypothesis that all contemporary life on Earth descends from a last universal common ancestor ('LUCA') provides a good illustration. Prior to the mid-19th century, biologists were struck by the remarkable morphological diversity of life on Earth. The diversity was so extreme that there seemed to be few puzzling similarities or correlations to suggest a common ancestor.<sup>8</sup> In the 20th century, biologists were surprised to discover that all known life on Earth (from bacteria to mushrooms to elephants) is remarkably similar at the molecular and biochemical level. Proteins, which supply the bulk of the structural and enzymatic material for known Earth life, are synthesized from the same approximately 20 amino acids even though there are over a hundred amino acids available in nature and biochemists have shown that perfectly functional proteins (given the right organismal environments) can be constructed from alternative suites of amino acids. The hereditary material (nucleic acids) of life on Earth displays equally striking contingent molecular similarities, utilizing, for instance, the same genetic code and same four DNA bases even though these are not the only chemical possibilities (see, e.g., Benner and Switzer [1999]). The best explanation for these (and other) remarkable molecular and biochemical similarities is not that they represent chance coincidences but that all life on Earth today inherited them from a last universal common ancestor.

Scientists do not cease searching for additional traces once they have satisfied themselves that they have a smoking gun for a last common cause. Further investigations may reveal important new details about the common cause, including the events that preceded and succeeded it. By investigating molecular and biochemical similarities common to contemporary organisms one can learn a great deal about LUCA—just as one can learn a lot about the end-Cretaceous extinctions by investigating the diverse contents of the K–T boundary and the geological record on either side of it. Biologists have learned

<sup>7</sup> I am oversimplifying a bit. Historical scientists typically pick out only one or a small number of the causal factors—the 'triggering cause' (e.g., a meteorite impact)—making up the total cause of a puzzling body of traces as salient. Unfortunately, it is beyond the scope of this paper to explore the distinction between triggering causes and causal liabilities and enablers. It is clear, however, that the reasoning of historical scientists presupposes such a distinction, and that it is drawn in the context of theoretical and empirical background beliefs about the phenomena concerned.

<sup>8</sup> Darwin's 19th century theory of evolution by natural selection suggested that many quite dissimilar looking organisms (e.g., dogs and elephants) descend from a common ancestor, and hence raised the possibility that all extant life on Earth may have arisen from a common ancestor. But his reasoning was not based upon the morphology of extant organisms. It was based upon the idea that the process of natural selection can change the morphology of organisms in profound but gradual ways over long periods of time. This is why the discovery of the remarkable molecular and biochemical similarities among extant organisms is considered to provide independent evidence for his theory.

that LUCA closely resembled contemporary bacteria; the question of whether it was hyperthermophilic (heat loving) or preferred more moderate temperatures is still being debated (Boussau *et al.* [2008]). As Carl Woese ([1987]) points out, LUCA is much too sophisticated biologically to represent the earliest form of life on Earth. The incredibly complex cooperative arrangement between proteins and nucleic acids, which is mediated by ribosomes (tiny molecular machines composed of protein and RNA), is already worked out in all known contemporary bacteria, and hence in LUCA. What LUCA represents is the most recent causal juncture thus far identified at which all known life on Earth today shares a common ancestor. Scientists have yet to discover, either in the chemistry of contemporary microbes or the chemistry of ancient rocks, sufficiently compelling traces of what earlier ancestral forms of life were like,<sup>9</sup> but microbiologists and paleomicrobiologists are busy looking for them.

#### 4.1 The priority of common cause over separate causes explanation

Common cause isn't the only possibility for explaining puzzling correlations and similarities among contemporary phenomena. Separate causal processes operating independently sometimes produce them. Elliot Sober ([1988], Chapter 3, [2001], [2008]) points to evolutionary biology as a good source of examples. Bats, birds, and insects, for instance, resemble each other in having wings but do not share a common ancestor with wings; they evolved wings separately. In contrast, lions, whales, elephants, and humans, whose females have mammary glands, do share a common ancestor with mammary glands. Similarities of the former kind, which are not inherited from a common ancestor, are known in biology as homoplasies, whereas those of the latter kind, which are inherited from a common ancestor, are known as homologies. Sober argues that such cases are not limited to evolutionary biology. Indeed, he claims that they are extremely common and cites positively correlated numerical quantities such as bread prices in Britain and sea levels in Venice (Sober [2001]), which (he supposes) have been monotonically increasing during the past two centuries, as telling examples. Examples such as these pose a potential threat to the principle of the common cause.

As I have interpreted it, the principle of the common cause does not assert that all ostensibly improbable coincidences are the result of a common cause; it claims only that they are very likely to be the result of a common cause. My weaker version of the principle tracks the statistical/probabilistic character of

<sup>9</sup> The Nobel Prize winning discovery by Tom Cech and colleagues that RNA (which plays a crucial role in the hereditary machinery for all known life on Earth, with the exception of some viruses) is self-catalyzing is often cited as telling evidence for an earlier 'RNA world', but there are some serious problems with this conjecture.



the asymmetry of overdetermination. It is thus hardly surprising that historical scientists sometimes entertain separate causes hypotheses when faced with puzzling correlations or similarities. In order to assign even a rough numerical probability to the likelihood of a common cause for an ostensibly improbable, arbitrary association among traces one would need to consult the second law of thermodynamics and the radiative asymmetry, a task well beyond the scope of this article. Nevertheless, given the global reach of the asymmetry of overdetermination, it is rational for historical scientists to opt for common cause hypotheses over separate causes hypotheses in the absence of theoretical or empirical reasons for believing that a specific seemingly improbable association among traces is the product of separate causes. That is, one would expect common cause explanation to be the default mode of evidential reasoning in historical natural science.

The actual practices of historical natural scientists suggest that this is the case. Most of the examples discussed in this article (e.g., Alvarez hypothesis, snowball Earth hypothesis, LUCA, and the big bang theory) comprise last common cause explanations for puzzling associations among traces (iridium and shocked quartz, glacial debris found in ancient low-altitude equatorial deposits, molecular and biochemical similarities among extant Earth life, and the isotropic 3 K background radiation), and hence are consistent with the claim that, all other things being equal, historical scientists prefer common cause explanations to separate causes explanations.<sup>10</sup> Indeed, paleontologist Douglas Erwin makes this preference explicit in a discussion of the still mysterious end-Permian extinctions (ca. 250 mya). According to Erwin ([2006], pp. 11, 54, 58), scientists prefer ‘single’ (common) causes to ‘multiple’ (separate) causes except when faced with empirical evidence that is difficult to explain in terms of a plausible common cause.

The scientific debate over the end-Permian extinctions provides a particularly salient illustration of when all other things are not equal and scientists opt for a separate causes explanation over a common cause explanation. On the basis of an initial body of traces, paleontologists conjectured that there was a single, prolonged extinction event lasting millions of years at the end of the Permian. They proliferated rival common cause hypotheses (climate change, meteorite impact, flood, volcanism, etc.) to explain it. As they accumulated additional evidence in their search for a smoking gun to discriminate among these hypotheses, and better analytic tools became available, it became clear that there were actually two extinction pulses separated by a period of around 10 million years. A more complete fossil record, coupled with more

<sup>10</sup> Many philosophers and scientists do not explicitly distinguish last common causes from common causes, but it is often implicit in what they say. As an example, while it is a common cause of every event in our universe, the big bang of cosmology is the *last* common cause of the isotropic 3 K background radiation discovered in the 1960s.

accurate radiometric dating methods, revealed an initial collapse of the late Permian ecosystem followed by a (geologically speaking) short period of recovery followed by an even more catastrophic collapse. What initially seemed to be a single improbable correlation among traces turned out to consist of two such correlations.

Once paleontologists recognized that they were dealing with two extinction events, however, they resumed the pursuit of common cause explanations. They proliferated separate common cause explanations for each extinction pulse. And they considered the possibility that these separate common causes might share an earlier common cause, e.g., the formation of the super continent Pangaea; for the likelihood of two global extinctions of this magnitude occurring within such a geologically short period of time seems extremely low. In other words, the paleontologists and historical geologists investigating the end-Permian extinctions have consistently exhibited a preference for common cause explanation over separate causes explanation. As Erwin emphasizes, it was only when faced with the empirical inadequacy of rival common cause explanations that they turned their attention to separate causes explanations.

Sober ([1988], pp. 89–102; [2001]; [2008], pp. 274–5) and Tucker ([2004], pp. 104–10) nevertheless contend that it is a mistake for historical scientists to favor common cause explanations over separate causes explanations unless they have information specific to the case at hand favoring one over the other. On their view the default position when faced with an ostensibly improbable association among traces is neutrality.<sup>11</sup>

Sober's belief that it is a mistake for historical scientists to favor common cause hypotheses over separate causes hypotheses for explaining puzzling associations among traces may stem in part from his focus on biological examples. Lying in the background of all biological reasoning is Darwin's theory of evolution by natural selection. According to Darwin's theory, similar environments can produce similar adaptations in organisms that do not share a common ancestor with the trait concerned. Moreover, biologists are familiar with numerous cases (e.g., birds and bats) in which this has occurred. It follows that homoplasies pose a very real threat to phylogenetic inferences in biology.

The situation in the nonbiological historical sciences, however, is quite different from that in evolutionary biology. No overarching theory of geology or

<sup>11</sup> The details of their accounts differ: on Sober's view, scientists start with specific common cause and separate causes hypotheses, whereas on Tucker's view they first decide the general issue of whether a common cause or separate causes hypothesis is appropriate. I think Sober is right about scientists beginning with specific hypotheses (Cleland [2008]), but this issue is irrelevant to the question of whether they exhibit a preference for common cause hypotheses over separate causes hypotheses.

planetary science suggests that geological analogies are so widespread in nature as to pose a serious threat to common cause explanations. This is not to deny that geological analogies occur, or that we might not have good reasons in a particular case for thinking that we are confronted with one. But unlike the case in biology, there are no theoretical reasons for thinking that they are common. This helps to explain why paleontologists investigating the end-Permian extinctions began by proliferating different common cause hypotheses. It wasn't until they acquired localized empirical evidence (from specific field sites) that they were dealing with distinct extinction events that they opted for a separate causes explanation.

It is important to distinguish purely numerical correlations (such as rising British bread prices and Venetian sea levels), from truly puzzling associations such as the presence of an iridium anomaly in K–T boundary sediments from around the world or the presence of wings in birds and bats. Purely numerical correlations among quantities are a priori extremely likely given the enormous number of things that could be correlated in this way. Because no one would attribute them to common causes and there are so many of them, Sober concludes that scientists should remain neutral between separate causes hypotheses and common cause hypotheses when explaining unexpected associations among events unless they have specific background information favoring one over the other. The problem is that scientists aren't (as Sober suggests) in a neutral epistemic situation with respect to purely numerical correlations among quantities. To the extent that a correlation is thought to be purely numerical it is also thought to be not very improbable and also produced by separate causes. Sober's ([2001]) example of rising bread prices in Britain and rising sea levels in Venice provides a good illustration. That British bread prices would be either monotonically increasing or else monotonically decreasing over an interval of time that is not too long doesn't seem very improbable. Similarly, it doesn't seem very improbable that Venetian sea levels would either monotonically increase or else monotonically decrease over an interval of time that is not too long. But if this is the case, it doesn't seem very improbable that British bread prices and Venetian sea levels should both be rising together independently over an interval of time that is not too long. The latter correlation is not, however, very puzzling from a scientific point of view. Scientists may of course acquire evidence that a correlation that initially seems purely numerical is not purely numerical after all. If bread prices in Britain and sea levels in Venice were rising in lock step (in exactly the same proportion) within the interval of time concerned the correlation would seem much more improbable, and hence become scientifically puzzling. In such circumstances, however, the correlation would cease to be viewed as merely numerical, and scientists would pursue a common cause explanation until they acquired theoretical or empirical reasons for thinking that it was

produced by separate causes after all. They might, for instance, explore the possibility that global warming was melting continental glaciers and raising sea levels, while at the same time causing droughts and damaging wheat crops. In short, the intuitive plausibility of Sober's numerical counterexamples to the principle of the common cause rests upon an ambiguity: correlations among quantities that are purely numerical are common and are almost always the product of separate causes, but it might turn out that we are wrong in thinking that a correlation is purely numerical, in which case it might turn out to be very improbable after all. The problem is that correlations of this ambiguous sort don't provide authentic cases of highly improbable correlations among quantities being produced by separate causes.

To wrap up, the role of local background information is not, as Sober and Tucker contend, to discriminate among common cause and separate causes explanation but rather to undermine the default assumption that a scientifically puzzling (seemingly improbable) body of traces was produced by a common cause. In the case of purely numerical correlations among quantities the default assumption is immediately defeated because such correlations are not very improbable and are understood to result from separate causes. Special theoretical reasons (e.g., Darwin's theory of natural selection) or localized empirical evidence (e.g., radiometric dating coupled with a more detailed fossil record) may also defeat the preference for a common cause in particular cases. In the absence of such defeaters, however, historical natural scientists have very good reasons for opting for common cause explanations for puzzling associations among traces over separate cause explanations, and this is exactly what they do.

#### 4.2 The threat of information degrading processes

The thesis of the asymmetry of overdetermination does not imply that every past event is epistemically overdetermined by phenomena in the present. It is unlikely but nonetheless possible for a past event to leave no traces in the present; prime candidates are events occurring before the big bang of cosmology. More significantly, with the passage of time, the causal information carried by traces becomes increasingly degraded, and eventually may disappear altogether. It is for this reason that a significant portion of historical research is devoted to analyzing and sharpening attenuated traces so that they can be identified and properly interpreted; this often requires the development of sensitive new technologies.<sup>12</sup>

<sup>12</sup> As discussed in my ([2002]), the laboratory work involved in analyzing and sharpening degraded traces is quite different from the testing of hypotheses that goes on in classical experimental science.

Following Sober ([1988], pp. 2–4), Derek Turner ([2004], [2007], Chapter 2) contends that ‘information destroying processes’ are so pervasive in nature that no interesting epistemological conclusions of the sort that I draw follow from the thesis of the asymmetry of overdetermination. It is important to distinguish information destroying processes from information degrading processes. The extent to which information is completely destroyed by natural processes isn’t clear, and there is reason to believe that it is much less than Turner and Sober believe. Scientists have become increasingly adept at extracting information once thought to be unobtainable from traces of the past. Meteor craters, for instance, become slowly buried over time until they are no longer detectable from surface features. But contemporary geologists have developed sophisticated instruments for detecting them underground. The Chicxulub crater, thought to be ground zero for the impact responsible for the K–T extinctions, provides a good illustration. It was identified by means of aerial surveys of the northern coast of the Yucatan Peninsula utilizing sophisticated geophysical instruments that revealed a gigantic (at least 170 km in diameter) circular gravity anomaly buried a kilometer beneath younger sedimentary rock. As another example, speculation that life on Earth goes back 3.8 billion years rests upon laboratory analyses of carbon isotope ratios in grains of rock as small as 10  $\mu\text{m}$  across weighing only  $20 \times 10^{-15}$  g. Analyses of these grains reveal an enrichment of the lighter isotope of carbon, which is preferred by life, over the heavier isotope, a correlation that is difficult to explain in terms of nonliving processes (Mojzsis *et al.* [1996]). Who would have thought that compelling evidence of long dead microscopic life could be extracted from material of this antiquity! Similarly, before Schweitzer and colleagues’ discovery, who would have dreamt that one could infer the sex of a dinosaur from its fossilized remains? As these and other examples illustrate, our ability to extract information about the past from contemporary phenomena is rapidly increasing, so much so that I suspect the 21st century may become the age of historical science!

Turner ([2004], [2007], Chapter 2) nevertheless insists that such cases are the exception rather than the rule. He cites the colors of dinosaurs as an example of something that paleontologists will never be able to discover. But a remarkable recent discovery suggests that he is wrong about this. While examining a fossilized bird feather under an electron microscope Jakob Vinther and colleagues ([2008]) identified preserved melanin granules; melanin is a natural pigment that gives color to bird feathers as well as to human skin and hair. Further studies revealed that the feather was color-banded. The fossilized feather was from a bird that lived during the Cretaceous, the last age of the dinosaurs. As they discuss, the implication of their discovery go far beyond the study of ancient birds. Paleontologists have discovered that many dinosaurs were feathered. Some like velociraptor, made famous in the movie Jurassic

Park, had full plumage whereas others were merely fuzzy; fossilized skin from a close relative of *T. rex* has been found with tiny fossilized feathers ('dino-fuzz'). This opens up the very real possibility that paleontologists will soon be able to infer the colors of some dinosaurs from their fossilized remains.<sup>13</sup> This stunning scientific development underscores my central point: The overdetermination of causes by their effects is extensive and pervasive in our universe, and this means that historical scientists can never rule out the possibility of discovering a smoking gun for any hypothesis about the past, however far fetched this possibility may currently seem.

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<sup>13</sup> Science moves quickly: while this article was under review two teams of scientists reported in *Nature* (Zhang *et al.* [2010]) and in *Science* (Quanguo *et al.* [2010]) being able to infer color patterns of two small feathered theropod dinosaurs from melanosomes found in exceptionally well preserved specimens discovered in China!

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