Common Cause Explanation and the Asymmetry of Overdetermination

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1 Introduction

As inspection of any recent issue of *Nature* or *Science* quickly reveals, historical research is common in natural science, occurring in fields as diverse as paleontology, geology, biology, planetary science, astronomy, and astrophysics. Some of the more 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 ancient igneous rocks; the Alvarez meteorite-impact hypothesis (for the extinction of the dinosaurs), which explains the 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 puzzling isotropic, 3° Kelvin, background radiation first detected by satellites in the mid 1960s. As these examples illustrate, the focus of most research in historical science is on explaining puzzling contemporary phenomena (e.g., an iridium anomaly) in terms of unobservable hypothetical causes (meteorite impact) in the remote past. 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 investigating its nature. This is particularly puzzling when one considers that the methodology of historical natural science does not seem to closely resemble that of stereotypical experimental science, the latter of which is commonly held up as the paradigm of 'good' science.

I addressed this issue in two earlier papers (Cleland [2000], [2001]), identifying fundamental differences in the practices of 'prototypical' historical science and 'classical' (stereotypical) experimental science. I argued that these differences could be explained in terms of a pervasive time asymmetry of causation, more specifically, 'the asymmetry of overdetermination.' The asymmetry of overdetermination underpins the objectivity and rationality of prototypical historical research, explaining why it is not, as sometimes supposed, 'inferior' to experimental science. This essay further develops this analysis, exploring the intimate connection between explanation and confirmation in the historical natural sciences, and placing it within the context of other philosophical accounts of historical explanation. As will become apparent, my account of explanation in historical natural science is a version of common cause explanation. Unlike other theories of common cause explanation, however, my account can explain why, other things being equal, historical scientists exhibit a preference for common cause explanation over separate causes explanation and chance explanation; the exception is when (as in evolutionary biology) they have special theoretical or empirical reasons for believing that a body of traces could have been produced by separate causes. The asymmetry of overdetermination underwrites the default preference in historical natural science for common cause explanation.

2 The Structure of Historical Natural Science

In my ([2001], [2002]) papers, I argued that most historical research in natural science exhibits a distinctive pattern of evidential reasoning characterized by two interrelated stages: (1) the proliferation of multiple, competing, alternative hypotheses to explain a puzzling body of traces encountered in fieldwork; (2) a search for a "smoking gun" to discriminate among them. A smoking gun discriminates among rival historical hypotheses by showing that one or more provides a better explanation for the total body of evidence available than the others. As I emphasized ([2002]), this pattern of evidential reasoning is not always found in the historical natural sciences, and it is sometimes found in (non-classical) experimental research. Which pattern of evidential reasoning is exhibited depends upon a scientist's epistemic situation. It is because scientists engaged in historical research typically find themselves in a different epistemic situation than classical experimentalists 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. Furthermore, an investigation may be quite dynamic. 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 hypotheses, 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 there isn't a guarantee that the correct hypothesis is among those being entertained, or for that matter, that it will ever be entertained by humans; historical scientists are just as limited by their imaginations as experimentalists. Besides, 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. In the absence of the requisite technology, historical scientists have little choice but to resign themselves to a collection of equally viable, rival hypotheses.

2.1 A Case Study: The Alvarez (Meteorite-Impact) Hypothesis

The scientific debate over the end-Cretaceous mass extinction, which famously killed off the dinosaurs, along with what is now estimated to be 75% to 85% of all species then on

Earth, provides a particularly good illustration of the dynamic interrelation between proliferating alternative hypotheses and searching for a smoking gun to discriminate among them. Prior to 1980, many different explanations were taken seriously by paleontologists, including pandemic, evolutionary senescence, climate change, nearby supernova, volcanism, and meteorite impact (Powell [1998], p. 165). Most of these hypotheses explained the fossil record of the dinosaurs by postulating mutually incompatible common causes. None of the evidence available at the time, however, 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 Triassic. It consists of a very 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 the 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 nuclear reactor (particle accelerator), which Luis had access to at Berkeley; when bombarded with energetic neutrons, an isotope of iridium emits distinctive gamma rays, allowing the amount of iridium in the sample to be determined by counting the number of rays. 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.

Luis and Walter knew that they were in possession of a smoking gun for the mysterious end-Cretaceous mass extinction. Earth's crust is 'low' in iridium because iridium is (like iron) 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. On the other hand, as volcanologists (e.g., Officer and Drake [1985]) pointed out, volcanism brings mantle material to the surface. Moreover, there is evidence of extensive volcanism (spread over an area of at least 1 million km^2) in the Deccan traps region of India approximately 65 mya (million years ago). Accordingly, extensive volcanism provides an alternative possibility for explaining the iridium anomaly in K-T boundary sediments. None of the other competing hypotheses for the end-Cretaceous mass extinction could explain the excess iridium. The Alvarezs' 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 rift volcanism could produce a global iridium anomaly similar to that found in the K-T boundary (e.g., Schmitz and Asaro [1996]). More importantly, however, further 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 (crosshatched, 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 volcanic eruptions produce 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 meteorite hit Earth 65 mya. Since this time more evidence of meteorite impact (microspherules, fullerenes containing extraterrestrial noble gases, and extensive deposits of soot and ash) 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. [i]

The iridium and shocked minerals weren't enough, however, to convince most paleontologists that the second prong of the Alvarez hypothesis is true—that the impact caused the mass extinction. 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 under way 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 grace*. Additional fieldwork was needed to establish a more convincing causal link between the impact event and the extinction event.

Paleontologists went to work, closely 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 of the boundary, 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 evaluating philosophical theories of historical natural science. For this reason I appeal to it extensively in subsequent discussions.

3 The Role of Prediction and Explanation in Confirmation

Unlike confirmation in classical experimental science, which is grounded in prediction, confirmation in prototypical historical science depends upon explanatory power. It is important to keep in mind that this difference is not definitive of historical science and experimental science; to reiterate, it reflects a difference in the evidential situations in which historical scientists and experimental scientists *typically* (but not always) find themselves. The iridium anomaly, which played such a pivotal role in the acceptance of the first prong of the Alvarez hypothesis, provides a salient example. The Alvarezs' didn't predict excess iridium in the K-T boundary, and then set out to find it. They literally stumbled upon it while exploring a different question: How long did it take for the boundary layer to be deposited? The significance of the iridium anomaly for the Alvarez hypothesis lies in the fact that in the context of the scientific 'knowledge' available at the time the latter (with the possible exception of the volcanism hypothesis) provides a better explanation for the former than any of the competing hypotheses.

It is important to appreciate that no one could have *predicted* (logically inferred) an iridium anomaly from the conjecture that a gigantic meteorite struck Earth 65 mya on the basis of the scientific knowledge available at the time. Indeed, even today there aren't any widely accepted, background auxiliary hypotheses that could warrant such an inference. Our current understanding of earth and planetary science informs us that there are 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. Peter Ward's pivotal studies of Cretaceous ammonites provide a good illustration of the threat posed by unrepresentative samples. Exposed outcrops of the K-T boundary are very rare, many are still buried, and of those that have been exposed, the majority has long since been removed by erosion. Ward was working 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 that he could find to the lower level of the boundary was 10 meters beneath it, leading him to suspect that they had become extinct tens of 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 second prong of) the Alvarez hypothesis after all!

Ward nonetheless characterized his fieldwork as testing a "prediction" of the Alvarez hypothesis. The question is what sort of a prediction could it be? As the discussion above makes clear, it cannot be a *precise* prediction to the effect that ammonite fossils will be found extending right up to the K-T boundary on the Spanish side of the Bay of Biscay. At best, it may be interpreted as a vague prediction to the effect that it is *likely* that there are rock sequences *somewhere* on Earth with ammonite fossils immediately below the lower edge of the boundary. Viewed from this perspective, Turner ([2007], Ch. 5) is correct when he says that historical scientists sometimes infer novel predictions from hypotheses about the past. But the fact that they do so does not, as he suggests, show that prediction plays the same role in prototypical historical science as it does in classical experimental science. The problem with vague prognostications like Ward's is that they are virtually immune to failure. Failure to find ammonite fossils in any particular location or in a number of particular locations doesn't bear on the possibility of finding them elsewhere, in some as yet unexplored rock record of the K-T boundary. This would be true even if he had failed to find them on the French side of the Bay of Biscay. This is in contrast to classical experimental science where failed predictions have as much if not more weight than successful predictions.

Hypotheses in classical experimental science are concerned with regularities, as opposed to singular events. Moreover the spatio-temporal gap between cause and effect is small. As a consequence, they can be repeatedly "tested" in localized laboratory settings by varying specific conditions while holding others constant. This makes it easier to identify or eliminate potential interfering conditions, and thus more difficult (but *a la* Duhem not impossible) to explain away failed predictions. The situation in prototypical historical natural science is quite different. Historical hypotheses are concerned with singular occurrences and the causal chain extending from cause to present-day effects is typically extremely long, complex, and convoluted. This makes prediction a poor tool for guiding a search for telling empirical evidence. Historical scientists know that there are many poorly understood (including unknown) background conditions and potentially interfering factors that could defeat a prediction independently of the truth of the hypothesis, and they don't have a good way of screening for which ones might have been operative in a given case. The upshot is that novel predictions in historical science are typically much less risky (in Popper's sense) than those in classical experimental science.

Even when historical scientists make fairly precise predictions, predictive failure rarely results in the decisive rejection of the pertinent hypothesis by the scientific community because in hindsight it is usually fairly easy to think of plausible scientific explanations for the failure. Somewhat ironically, this point is underscored by the examples of failed novel predictions cited by Turner ([2007], Ch. 5). Consider, for instance, his discussion of the 'snowball Earth' hypothesis. According to Turner, the snowball Earth hypothesis holds that the entire planet was completely 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 prediction: geological sections of the pertinent age should reveal periods during which no sediments were formed (because no weathering occurred). Leather et al. (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 et al.'s discovery by rejecting the snowball Earth hypothesis, and for very good reasons. First, the snowball Earth hypothesis was never as specific as Turner suggests. 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_2 levels, extent of sublimation processes over sea-ice, thickness of sea-ice cover, thickness of continental ice sheets, varieties of non-hydrological 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 (see 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 shutdown) water cycle.

Predictions that succeed, in contrast, sometimes carry great weight in prototypical historical natural science. But it is not in virtue of representing a successful novel prediction that they do so. Regardless of the circumstances in which it is acquired, evidence functions as a smoking gun if it shows that one hypothesis provides a better explanation than its rivals. If Ward had accidentally stumbled upon ammonite fossils just below the K-T boundary in France, as opposed to having gone looking for them there, his finding wouldn’t have been any less significant. This explains why so many of the high profile achievements of historical science have the character of serendipitous discoveries even when they can be interpreted as involving novel predictive successes. In this context it is important to keep in mind that the evidence that makes a vague prediction successful may itself be quite precise. Ward’s discovery in France was not vague: He found abundant ammonites within a meter of the lower edge of the K-T boundary in a well-preserved outcrop of the pertinent geological section. This discovery provides much better evidence for the conjecture that the ammonites did not go extinct before the impact than his failure to find ammonites in an analogous rock record in Spain provides evidence that they went extinct.

As Turner admits, 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 doing so, he implicitly endorses the widely accepted assumption that the practices of stereotypical experimental science provide the prototype for all of science. It is thus hardly surprising that he concludes that historical science is epistemically disadvantaged vis-à-vis experimental science (Turner [2004], [2007]). But as I have argued ([2001], [2002]), the actual practices of historical natural scientists provide little support for this assumption. Most historical hypotheses are not rejected on the basis of failed predictions but rather because another hypothesis does a much better job of explaining the total body of evidence available in the context of our scientific background knowledge. As an example, the contagion hypothesis for the extinction of the dinosaurs cannot be viewed as refuted by the discovery of the iridium anomaly 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 massive volcanism or a gigantic meteorite impact, 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 question of whether massive flood volcanism or a gigantic meteorite impact provides the best explanation for the iridium anomaly. The point is in historical science a hypothesis may be rejected on the basis of evidence that does not refute (a.k.a. falsify) it.

To wrap up, unlike classical experimental science, historical natural science is not a prediction-centered enterprise. Hypotheses in prototypical historical natural science are 'confirmed' by evidence in virtue of the power of the hypothesis to *explain*, as opposed to successfully predict, the evidence. This is not just a matter of accommodation. The evidence (smoking gun) that cinches the case for an historical hypothesis over its rivals is typically discovered after the hypothesis was formulated. The Alvarez hypothesis for the end-Cretaceous extinctions provides a good example. The hypothesis did not originate with the Alvarezes, despite the fact that it now bears their name. It was propelled from the backburner to the frontburner of geological science with the discovery of positive evidence that such an event actually happened. A scientific consensus on the meteorite-impact hypothesis for the K-T extinctions was achieved because it explains an otherwise puzzling body of traces, many of which (e.g., iridium, shocked quartz, glassy spherules, etc., and fossil records of ammonites, foraminifera, plant pollen, fern spores, etc.) were discovered after the hypothesis had been formulated, better than any of its competitors. The appearance of these disparate traces in geological strata of the same age is deeply mysterious; they are individually unexpected and their joint occurrence is even more enigmatic. The Alvarez hypothesis explains this double mystery better than any of its currently available, scientifically plausible competitors. It is for this reason that it is currently widely accepted by the scientific community.

3.1 The Covering Law Model

At one time the emphasis on explanation over prediction in the confirmation of prototypical historical hypotheses wouldn't have been viewed as significant. For explanation is just 'retrodiction' on the traditional covering law model of scientific explanation (Hempel [1965]). Hempel's 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; prediction and explanation thus have the same logical structure. 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; Hempel assumed that there are 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. Laws (whether deterministic or statistical/probabilistic) that are strong enough to license logically 'valid' deductive or inductive inferences must be universal (within the pertinent domain of discourse) and exceptionless. Explanations in historical science 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 chains, to be captured in a plausible generalization of the kind required by the covering law model; 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 this difficulty. His solution was to demote historical explanations to mere 'explanatory sketches', thus reinforcing the prevalent 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 unspecified natural laws, a view which still attracts adherents, e.g., Ereshefsky [1992]). But this represents little more than an ad hoc attempt to force historical explanations to conform to a favored but inadequate model of scientific explanation.

Common strategies for dealing with the restricted, exception-ridden generalizations of the 'special sciences' are to tack on *ceterus paribus* clauses. As Sandra Mitchell ([2000], [2002]) argues, however, this strategy is not very satisfactory. To be compelling *ceterus paribus* laws require approximate generalizations coupled with knowledge of some contingencies (interfering factors); the *ceterus paribus* clause magically absorbs any additional, unknown dependencies. But even approximate generalizations are strikingly absent from most explanations in historical science. Scientists just don't know enough about all the things that might happen in the spatio-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 *ceterus paribus* clause.

Kleinhans and colleagues ([2005]) embrace a reductionist solution to this difficulty. On their view, the extremely rough generalizations of contemporary geology (historical and nonhistorical) are '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* [italics are mine] translated in physical and/or chemical terms' (p. 295). But what evidence (other than blind faith) is there for this? Geologists are notoriously bad at predicting earthquakes even for extensively studied, local regions of well-mapped fault systems such as the San Andreas Fault. Moreover, even supposing that it is *in principle* possible to 'reduce' generalizations distinctive of earth science to laws of physics and chemistry, no human being knows how to do the reductions. This means that the conjectured reducing laws do not play an actual role in explanations given by contemporary earth scientists. Besides, as Nancy Cartwright (e.g., [1983]) has argued, it isn't even clear that the laws of fundamental physics are universal and exceptionless. Kleinhan's and colleague's proposal amounts to little more than a return to Hempel's faith-based explanatory sketches.

In light of these and other considerations, Mitchell proposes modifying the traditional concept of law of nature to include degrees of contingency or, in her words, 'stability over changes in context' (Mitchell [2002], p. 334). The laws of fundamental physics exhibit the greatest (but not perfect) stability and the laws of the special sciences the least. In this way she hopes to preserve the central role of laws of nature in scientific reasoning.

In a recent paper on the methodology of historical science, Ben Jeffares (2008) endorses Mitchell's proposal and argues that the investigation of rough generalizations is just as central to prototypical historical natural science as the search for a smoking gun. In his words, 'the historical sciences also seek regularities in the world and have to [italics are mine] in order to secure their claims about the past' (p. 470). 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 is crucial to the acceptance or rejection of historical 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 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. It is clear, however, that this regularity is being pursued as a means to an end, as opposed to ‘an end in itself’ (p. 470). As Jeffares concedes, archaeologists are interested in discriminating marks on bones left by human tools from those left by canine teeth for the purpose of interpreting marks found on ancient bones; the purpose of the experimental work is to procure a tool (analogous to radiometric dating methods) for analyzing evidential traces discovered in the field.

The question is whether Jeffares is correct in claiming that historical scientists investigate special primary generalizations--generalizations ‘directly’ linking long past causes to their contemporary traces—and utilize them for purposes of prediction. As the discussion in the preceding section underscores, this is a very problematic claim. Regularities holding between cause and effect event types separated by protracted intervals of time are exceedingly 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. The upshot is that it is not only difficult to identify generalizations capable of ‘directly’ linking long past causes to their present day traces, any generalizations that are identified are extremely unstable (in Mitchell’s sense). One cannot infer predictions capable of playing pivotal roles in the evaluation of hypotheses from generalizations with this degree of contingency. Jeffares’s mistake is in thinking that he can retain the explanatory power of prediction (*a la* the covering law model) with a much weaker notion of natural ‘law’.

The burden of this discussion has been to show that explanations in prototypical historical natural science cannot be viewed as ‘confirming’ historical hypotheses in virtue of functioning as (precise or vague) predictions or retrodictions. At one time this would have been considered grounds for thinking that historical science is inferior to experimental science. But the covering law model has fallen on hard times in recent years; it is no longer widely accepted even in physics. New theories of explanation have been proposed in its stead. In the historical sciences the dominant theories of explanation place the explanatory burden on causal features of the world, as opposed to generalizations. The two main modes of causal explanation in evolutionary biology, which dominates philosophical thinking about explanation in historical science, are narrative explanation and common cause explanation. In the following subsections I compare and contrast these modes of explanation. I argue that the identification of common causes is more fundamental to the practices of historical natural sciences than the construction of causal narratives. This sets the stage for the discussion in Section 4 in which I argue that the emphasis on common cause explanation in historical natural science is underwritten by a pervasive, physical feature of our universe, the asymmetry of overdetermination.

3.2 Narrative explanation

Narrative explanations are causal stories. The basic idea is to construct a coherent, intuitively continuous, causal sequence of events culminating in the event to be explained. Because much is unknown about the events in the sequence, narrative explanations have a significant fictional component, involving both omissions and additions. Narrative accounts dominate thinking about explanation in human history, where human intensions play key explanatory roles. Because explanations in evolutionary biology are historical and often involve appeals to natural ends (e.g., explaining the ability of bats to echo locate as an adaptation to a nocturnal, aerial, life style), some philosophers, e.g., Ruse ([1971] and Hull ([1992]) contend that historical explanation in natural science also fits the narrative model.

The problem with narrative accounts of explanation is the stress placed upon formulating a coherent story over empirically validating it: Since many of the events in the narrative sequence are invented to provide coherence and continuity, justification is relegated to a minor role. This conflicts with the traditional emphasis in natural science on evidential warrant. The problem is exacerbated by the crucial role of explanation in the confirmation 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 contrast to the narrative model, the emphasis in common cause explanation is on evidential warrant. The basic idea is to formulate reliable inferential methods for identifying when a diversity of contemporary traces are the effects of a long past, common cause token. Although some proponents of narrative explanation (Hull [1992]; Kleinhans et al. [2005]) embrace the search for common causes as part of the project of formulating compelling narratives, the former frequently has a life of its own. A salient example is paleontologist Mary Schweitzer and colleague's ([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 when they discovered an analogous layer in the leg bone of a T. Rex. They concluded that the bone was from a female T. Rex. Significantly, they did not attempt to reconstruct any of the antecedent events in the long causal chain stretching between the death of this unfortunate T. rex and the preservation of its bones for millions of years in the Montana desert. Indeed, a detailed story of this sort seems irrelevant to their purpose, which was to evaluate whether the fossilized bone in question really came from a female T. rex. To this end, they studied the detailed structure 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 explanation they gave for the medullary-like bone did not resemble a story: It was simply too minimal to meet the threshold required for a genuine narrative.

To the extent that evolutionary biologists must cope with an overwhelming number of historical contingencies in their inferences about phylogeny, narrative explanations are perhaps unavoidable, but they are hardly desirable. Indeed, the discovery of 'molecular fossils' in living organisms (e.g., genomic sequences that have changed little over the eons) has given a tremendous boost to some historical narratives in evolutionary biology while discrediting others because they provide an independent empirical check on evidence derived from more traditional sources, e.g., morphology and the fossil record. This underscores my central point. From the perspective of historical natural scientists, the crucial issue is figuring out how to validate inferences to long past common causes from collections of contemporary traces independently of the details of the long causal chains lying between them.

3.3 Common Cause Explanation

Elliot Sober ([1988]), and Avi Tucker ([2004]) are perhaps the best-known proponents of common cause accounts of historical explanation in natural science. Common cause theories of explanation are founded upon Hans Reichenbach's ([1956]) 'principle of the common cause', which, roughly speaking, asserts that seemingly improbable coincidences (correlations or similarities among events or states[ii]) are best explained by reference to a shared common cause. Although Reichenbach articulated it in terms of probabilities, the principle of the common cause is not a logical consequence of the mathematical theory of probability. It is an epistemological conjecture about the conditions under which a certain pattern of causation may be non-deductively inferred. According to the principle of the common cause, coincidences are rare. Most coincidences are produced by common causes.

Underlying the principle of the common cause is an ostensibly metaphysical claim about the temporal structure of causal relations among events in our universe: most events have multiple effects—form causal forks that open from past to future. In Section 4, I argue that this presupposition is not merely metaphysical; it is empirically well grounded in physical theory. For the purposes of this section, however, the important point is that if the temporal structure of causal relations in our universe were different—if most correlated events were chance occurrences, or 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—one would not be justified in inferring the likelihood of a common cause from an improbable association among traces.

It is important to keep in mind that the principle of the common cause does not guarantee that every *seemingly* improbable association is likely to be the result of a shared common cause. This is true only for *genuinely* improbable associations. How improbable we take a given association to be depends upon our background beliefs.

There are no guarantees that our background beliefs (which include supporting theories from other scientific disciplines) are correct. They are just as subject to revision in light of theoretical advances and new empirical discoveries as the target historical hypothesis under investigation. As a consequence, one may be wrong in thinking that a given association among traces is highly improbable, and hence wrong in inferring that it is more likely than not the product of a common cause. The challenge of course is determining when a seemingly improbable association is genuinely improbable. Unfortunately a general solution to this problem is unlikely to be forthcoming because it presupposes a solution to the problem of induction as applied to probable inference. This does not mean, however, that one cannot have highly plausible reasons for believing that a particular association among traces is genuinely improbable. The key is to rely upon background beliefs that are well supported by evidence while keeping in mind that there is no such thing as conclusive proof in any field of science; all scientific conjectures, however well supported by evidence, are subject to revision in light of new information.

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. Attributing puzzling similarities and correlations among phenomena to a common cause has great explanatory power, for it makes their joint occurrence credible. Attributing their concurrency 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 example. Given our current understanding of geology, the only event that renders their global concurrence in a structurally distinctive, thin layer of sediment found all over the world explicable is a massive meteorite impact. As a consequence, the case for a meteorite impact is currently overwhelming. Similarly, the best explanation for the truly astonishing 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 other words, the more improbable an association among a collection of traces *seems* the more psychologically convincing the claim that it is genuinely improbable, and hence the more compelling (assuming the truth of the principle of the common cause) 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 to them (in light of their background beliefs) to be the most puzzling correlations or similarities among contemporary phenomena.

As Elliot Sober ([1988], [2001]) observes, common cause and mere chance aren't the only possibilities for explaining puzzling correlations and similarities among contemporary phenomena. Separate causal processes operating independently also produce them. Sometimes these processes are of the same type, representing independent instances of the same causal mechanism, and sometimes they are of different types but just happen to produce similar traces. In either case, the results are the same, a spurious correlation or similarity due neither to chance nor a common cause. As Sober points out, evolutionary biology is 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 human females, which have mammary glands, do share a common ancestor with mammary glands. Examples such as these pose a challenge to the principle of the common cause. The question is how does one discriminate similarities of the former kind (known in biology as 'analogies'), which are not the result of a common ancestral cause, from those of the latter ('homologies'), which are the result of a common ancestral cause?

As discussed below, the asymmetry of overdetermination not only provides the needed justification for the principle of the common cause, it also illuminates the conditions under which historical scientists opt for separate cause explanations over common cause explanations. As will become apparent, historical scientists prefer common cause explanation except when (as in evolutionary biology) they have special theoretical or empirical reasons for believing that a body of traces could have been produced by separate causes.

4 The Asymmetry of Overdetermination

In my ([2001], [2002]), I argued that the distinctive methodology of prototypical historical natural sciences (proliferating alternative hypotheses and searching for a smoking gun) is best understood in terms of a pervasive physical feature of our universe, a time asymmetry of causation dubbed the 'asymmetry of overdetermination' by the late

David Lewis ([1991]). The asymmetry of overdetermination consists in the fact that most local events epistemically *over*determine their past causes (because the latter typically leave extensive and diverse effects) and *under*determine their future effects (because they rarely constitute the total cause of an effect). As an example of the epistemic overdetermination of past causes by their future effects consider an explosive volcanic eruption. Its effects include extensive deposits of ash, pyroclastic debris, masses of andesite 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 of effects 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, predicting even the near future eruption of a volcano, such as Mt. Vesuvius, is much more difficult. There are too many causally relevant conditions (known and unknown) in the absence of which an eruption won't occur.

The physical source of the asymmetry of overdetermination is controversial. Examples such as an explosive volcanic eruption are commonly attributed to the second law of thermodynamics, which (statistically interpreted) says that the natural tendency of physical systems is to move from more to less ordered states. As I discuss in my papers, however, the asymmetry of overdetermination also encompasses wave phenomena, which do not obviously admit of a thermodynamic explanation. Although traditionally associated with electromagnetic radiation (light, radio waves, etc.), the 'radiative asymmetry' 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 (particle and wave) are subject to the asymmetry of overdetermination. While it is tempting to suppose that they are somehow connected—that one is derived from the other, or they are both derived from some third feature of the universe, e.g., its initial conditions at the time of the big bang (Horwich [1987])—the important point, for our purposes, is that they represent objective and pervasive physical features of our universe. It follows that Turner

([2004]) is wrong in claiming that the asymmetry of overdetermination is 'strictly metaphysical' (p. 210); it is based in physical theory

The asymmetry of overdetermination provides the needed empirical grounding for the principle of the common cause. According to the asymmetry of overdetermination, the vast majority of causal forks open in the 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 puzzling association (correlation and/or similarity) among present-day phenomena is due to a *last* common cause.[iii] If the temporal structure of causal relations in our universe were different—if most correlated events were chance occurrences, or 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—one would not be justified in inferring the likelihood of a common cause from an improbable association among traces. The quest for a smoking gun is a search for additional evidential traces for distinguishing which of several rival hypotheses provides the best explanation for the available body of traces. The overdetermination of the past by the localized present, a physical fact about our universe, ensures that such traces are likely to exist if the initial collection of traces shares a last common cause. For insofar as past events typically leave numerous and diverse effects, only a small fraction of which is required to identify them, the contemporary environment is likely to contain many, as yet undiscovered, smoking guns for discriminating among rival common cause hypotheses.

The asymmetry of overdetermination does not guarantee that every mysterious association among traces is due to a last common cause. As Sober notes, historical scientists sometimes entertain separate causes hypotheses when faced with a puzzling body of correlations or similarities. They rarely do so, however, unless they have special reasons for thinking that a separate causes explanation poses a genuine competitor to a common cause explanation; in keeping with the asymmetry of overdetermination, the default preference is for common cause explanations.

In the case of biological analogies and homologies the special circumstances are theoretical. 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 know of numerous cases (e.g., birds and bats) in which this has occurred. It follows that analogies pose a very real threat to phylogenetic inferences.

The situation in earth history, however, is quite different. No overarching general theory of geology or planetary science suggests that geological analogies are so widespread as to pose serious threats to common cause explanations for improbable associations among traces. Nevertheless, a search for a smoking gun for a common cause may turn up empirical evidence that a body of traces was produced by separate causes. A good example is radiometric and fossil evidence that the great Permian extinction of approximately 245 mya consisted of two distinct extinction events separated by about 10 million years (Erwin [2006], p. 7). But even in this case the search is ultimately for common causes. Having subdivided the original body of traces into those pertaining to the first extinction pulse and those pertaining to the second extinction pulse, scientists now seek their separate common causes. They also explore the possibility that the two pulses might be causally related through an earlier common cause, e.g., the formation of the supercontinent Pangaea. The point is in the absence of theoretical or empirical reasons for believing that a puzzling association among traces was produced by separate causes, historical scientists opt for common cause explanation; their reasoning about the past on the basis of the present tracks the general causal structure of our universe.

This brings us to another caveat. The asymmetry of overdetermination does not guarantee that every past event can be identified from contemporary traces. It is unlikely but nonetheless possible for an event to leave no traces; 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.

In recent work, Derek Turner ([2004], [2007]) argues that ‘information destroying processes’ are so pervasive in nature that no interesting epistemological conclusions, of the sort that I draw, follow from the asymmetry of overdetermination. He is assuming, however, that such processes completely destroy information, as opposed to merely render it difficult to extract. There is good reason to believe that the possibilities for extracting useful information from degraded traces are much greater than he believes. Ancient meteorite craters, for instance, become slowly buried over time until they are no longer detectable from surface features. The Chixulub crater, thought to be ground zero for the impact responsible for the K-T extinctions, was identified by means of aerial surveys of the northern coast of the Yucatan Peninsula utilizing sophisticated geophysical instruments, which revealed a gigantic (at least 170 km in diameter), circular, gravity anomaly buried a kilometer beneath younger sedimentary rock. Analogously, 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 μm across weighing only $20x10^{-15}$ g (Mojzsis et al. [1996], p. 56). Remarkably, these analyses 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 non-living processes. Who would have thought that convincing evidence for long dead microscopic forms of life could be extracted from rocks this old? As these examples illustrate, our ability to extract information about the past from contemporary phenomena is rapidly increasing, so much so that I suspect the twenty first century may become the age of historical science!

Turner nevertheless contends that such cases are the exception rather than the rule, citing speculation about the colors of the dinosaurs as an example of something that we will never be able to discover (Turner [2004], pp. 217-8, and [2007], Ch. 2). Admittedly we currently don't know how to determine the color of a dinosaur from its fossil remains. But this doesn't mean that the information isn't there. Indeed, this example bears an uncanny resemblance to the claim that one cannot sex a dinosaur from its fossilized remains. A few years ago this claim would have been just about as plausible but, as Schweitzer recently demonstrated for a certain T. rex, it is false. In other words, even though information-degrading processes are common, there is little evidence that they completely destroy information as opposed to merely make it difficult to extract. The extent to which information degrading processes remove identifying information about long past causes from their traces with the passage of time is an empirical question. If recent technological advances provide any guide, it may be much less than Turner believes. And this brings us to a stunning recent paleontological discovery. While examining a fossil bird feather under an electron microscope Jakob Vinther and colleagues (2008) stumbled upon preserved melanin granules; melanin is a natural pigment that gives color to bird feathers as well as to human skin and hair. The feather was from the Cretaceous, the last age of the dinosaurs. Because many dinosaurs had feathers, including close relatives of T. rex, the team speculates that they may eventually be able to interpret the color of many dinosaurs as well as of ancient birds. The discovery of melanin granules in a fossilized bird feather 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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[i] 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. It is difficult to connect even a gigantic, local impact crater with a global extinction. In contrast, the global distribution of 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 since seventy percent of Earth's surface is covered by ocean, making an ocean impact more probable than a land impact, and an ocean impact crater would almost certainly have been obliterated by now the active geology of the seafloor, 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; they didn't view the case for a catastrophic meteorite impact as resting upon the discovery of an appropriate crater. This is not to deny the many scientists view the discovery of the crater as augmenting an initially strong case. Significantly, however, a few geologists ( ), who are also convinced by the iridium and shocked quartz that a catastrophic impact occurred 65 mya, insist that the Chicxulub crater isn't the right one.

[ii] Reichenbach emphasized correlations but many contemporary philosophers also emphasize similarities.

[iii] The ‘big bang’ of cosmology is of course the earliest common cause of every contemporary phenomenon in our universe, but many subcollections of these phenomena share more recent common causes, including a last common cause.