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Common cause explanation and the search for a smoking gun

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ABSTRACT

The hypotheses of historical natural science are typically concerned with long past, singular events and processes, e.g., what caused the end-Cretaceous mass extinction. Evidence for such occurrences is acquired through field studies in the messy, uncontrollable world of nature. Because hypotheses about the remote past cannot be directly tested in the classical manner of experimental science, historical science is sometimes judged inferior. Building on earlier work, this essay explains the motivation for such arguments and why they are fundamentally mistaken. Traditional versions of the scientific method (inductivism and falsificationism) are based upon a deeply flawed, one-size-fits-all, logical analysis of the evaluative relation between hypothesis and observation. The distinctive methodologies of historical and experimental science, however, reflect pervasive causal differences in their evidential situations. The evidential reasoning of historical scientists is founded upon the principle of the common cause, which asserts that seemingly improbable associations among present-day traces of the past are best explained in terms of a common cause. The truth of the principle of the common cause rests upon a physically pervasive, time asymmetry of causation: In a nutshell, the present contains records of the past but not of the future. Viewed in this light historical scientists actually have an evidential advantage over classical experimentalists.

INTRODUCTION

Experimental science has long been held up as the paradigm of "good" science. Yet many scientific hypotheses cannot be tested in the classical manner of experimental science, namely, by conducting controlled experiments within the artificial confines of a laboratory. Hypotheses about long past, natural events—e.g., the hypothesis that the continents were united in a supercontinent 250 Ma and the hypothesis that the end-Cretaceous extinctions were precipitated by the impact of a massive meteorite—provide salient illustrations. The observational data supporting these hypotheses (respectively, patterns of frozen magnetism in igneous rocks, and high concentrations of iridium and shocked quartz in K-Pg [Cretaceous-Paleogene] boundary sediments) are collected in the messy, uncontrollable world of nature through field studies. Because such investigations do not closely resemble those of classical experimentalists they are sometimes judged inferior, not only by politicians and laypersons but also by other scientists. In the somewhat jarring words of Henry Gee (1999), a senior editor of *Nature*, "… they can never be tested by experiment, and so they are unscientific" (p. 5).

In earlier work (Cleland, 2011, 2009, 2002, 2001) I argue that while it is true that there are fundamental differences in methodology between the historical natural sciences and classical experimental sciences, it is a mistake to conclude that the scientific status of the former is inferior to that of the latter. In the first place, as discussed in the next section, many doubts about the scientific status of historical research are rooted in a one-size-fits-all account of the methodology of science that is deeply flawed, both logically and as an account of the actual practices of

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scientists (including experimentalists). The third section sketches an account of the methodology of the historical sciences and how it differs from that of classical experimental science that is more faithful to the actual practices of scientists. This brings us to the scientific status of the historical sciences: Is Gee correct about the historical sciences being inferior to the experimental sciences? The answer is *no*. As discussed in the fourth section of this essay, the objectivity and rationality of the historical natural sciences is underwritten by a physically pervasive, time asymmetry of causation known to philosophers as the "asymmetry of overdetermination" (Lewis, 1979). The asymmetry of overdetermination explains a number of otherwise puzzling features of the evidential reasoning of historical researchers: Why they exhibit a preference, all other things being equal, for common cause explanations over separate causes explanations; Why the acceptance and rejection of hypotheses about long past events and processes is based upon inference to the best explanation, as opposed to predictive success and failure, which dominates the evidential reasoning of classical experimental scientists. I conclude that the putatively problematic differences in research strategies between historical scientists and experimentalists reflect pervasive *causal* differences in their evidential situations; the methodology of each domain is designed to accommodate and exploit causal, as well as logical, characteristics of the evidential relation between hypothesis and observation. The view that historical science is somehow inferior to experimental science is based upon a mistaken account of scientific methodology that reconstructs scientific reasoning entirely in terms of purely formal, logico-mathematical considerations.

THE SCIENTIFIC METHOD(S) OF YORE

In traditional discussions of the scientific method, hypotheses are portrayed as timeless generalizations and this reveals an implicit bias toward experimental science. The target hypotheses of classical experimental science are concerned with regularities among *types* of events, e.g., for *every* action there is an equal and opposite reaction (Newton's third law of motion). In contrast the target hypotheses investigated by historical scientists are typically concerned with *particular,* dated, past events, e.g., the end-Cretaceous mass extinction that occurred 65 Ma, as opposed to mass extinctions in general. There are of course exceptions. Sometimes historical scientists explore more general hypotheses, e.g., the conjecture that the five largest mass extinctions on Earth were caused by comets in the Oort cloud being slung into the inner solar system every 26 million years or so by a small faint companion star (red or brown dwarf) in a highly asymmetrical orbit around our sun (Raup and Sepkoski, 1984). But even in this case the generalization is highly constrained (namely, to Earth and the peculiarities of our solar system) compared to the universal generalizations of physics and chemistry tested in classical experimental research.

The scientific status of historical research is commonly attacked on the grounds that, unlike experimental science, it does not conform closely enough to the "scientific method." To properly evaluate this claim one needs a good understanding of the concept of scientific methodology upon which it is based. Traditional accounts of the scientific method divide into justificationist (confirmationist) theories and falsificationist theories. Justificationists and falsificationists concur that the scientific method begins with the derivation of a test implication (I) from a hypothesis (H). The following "toy" example provides a good illustration: Given the hypothesis (H) 'all copper expands when heated' one can deduce the following test implication (I): if a piece of copper is heated (C) it will expand (E). Test implications provide the basis for testing a hypothesis; they amount to conditional predictions. The basic idea is to bring about condition C in a laboratory, or alternatively search for it in nature, and then look for an instance of E.

Both justificationist and falsificationist theories reconstruct the key evidential relation between target hypothesis and observation in purely formal, logico-mathematical terms. Appealing to inductive logic (Thomas Bayes' theorem or the axioms of the mathematical theory of probability), justificationists argue that while hypotheses cannot be conclusively proven, their probability can nonetheless be raised by enough successful predictions. Unfortunately, theories of justificationism face the probabilistic version of the hoary problem of induction: No finite body of positive evidence, however large, can rule out the possibility of discovering an exception to a universal generalization about the natural world. A salient illustration is the claim that all swans are white. Before the sixteenth century this conjecture enjoyed the support of an enormous number of observations from all over the known world with no known exceptions. Any probability assigned to it on the basis of observation would thus have been very high. Yet we now know that this conjecture is false—Australia and New Zealand are teeming with black swans—which means that its *objective* probability is actually zero. Faced with this conundrum, Karl Popper counseled that the probabilities assigned to hypotheses in light of empirical evidence are best interpreted as subjective—as measuring the degree of psychological confidence that scientists have in a hypothesis, as opposed to how likely it is to be true (Popper, 1959).

Convinced that the problem of induction is intractable, Popper developed falsificationism as an alternative account of the methodology of science. Unlike justificationism, falsificationism receives support from an inference rule of deductive logic, *modus tollens*. According to modus tollens, all that it takes to *conclusively* disprove a universal generalization is a *single* counterexample. The logical form of the inference is as follows:

(A) 1. If H, then I
 2. It is not the case that I
 3. (Therefore) it is not the case that H

The hypothesis that *all* swans are white is thus falsified by the discovery of a single black swan. Similarly, the hypothesis that *all* copper expands when heated is falsified by a case in which copper is heated and fails to expand. Popper developed this logical insight into an austere ideal for scientific practice: Scientists should focus their efforts exclusively on trying to disprove hypotheses by subjecting them to "risky tests"—tests that are judged (in light of their background understanding of the phenomena involved) highly likely to result in failed predictions. If the prediction fails, modus tollens is invoked, and the hypothesis is ruthlessly rejected. If the prediction succeeds, the hypothesis is retained and subjected to further risky tests, but no conclusion as to its probable truth can ever be drawn on logical grounds, regardless of how many risky tests it has survived.

Philosophers have known for more than half a century that falsificationism is deeply flawed logically. Falsificationism treats hypotheses as if they were being tested in isolation from nature—as if a prediction involves no assumptions about a particular test situation other than those explicitly endorsed as boundary or initial conditions of the hypothesis. But as Pierre Duhem (1954) pointed out some time ago, hypotheses and theories never stand alone when tested in real-world scenarios. Whether conducted in the lab or the field, a concrete test of a hypothesis involves an enormous number of auxiliary assumptions (a_1, a_2, …, a_n) about the test situation. These include assumptions about instrumentation, pertinent conditions, and the absence of potentially interfering factors, many of which are highly theoretical, poorly understood, or simply unknown. The application of modus tollens, upon which the rejection of a hypothesis in the face of a failed prediction is founded, implicitly presupposes the truth of all of these assumptions. When they are explicitly conjoined to the target hypothesis H the first premise of inference schema (A) changes from 'If *H*, then *I*' to 'If *H* and (a_1 and a_2 and … and a_n), then *I*'. And this profoundly changes the logical form of the inference:

(B) 1. If *H* and (a_1 and a_2 and … and a_n), then *I*
2. It is not the case that *I*
3. (Therefore) it is not the case that *H*, *or* it is not the case that a_1, or it is not the case that a_2, or …, or it is not the case that a_n

The upshot is that logic does not sanction the rejection of a hypothesis in light of a failed prediction. The most that logic can tell us is that either the hypothesis or one or more of the auxiliary assumptions about the test situation are false. This is terrible news for falsificationism because it means that hypotheses cannot be conclusively falsified on the basis of failed predictions after all.

This helps to explain why scientists, and this includes experimentalists, rarely if ever follow Popper and reject hypotheses in the face of failed predictions. Instead, they embark on a sustained search for a false auxiliary assumption that might be responsible for the failure. Every student of science knows that repetitions of classic experiments in laboratory exercises often go wrong not because the hypothesis being tested is false but because something has gone wrong in the experimental setup. They are taught to respond to predictive failure by searching for problematic auxiliary assumptions, e.g., perhaps the equipment malfunctioned or the sample was contaminated. This training prepares them for careers as professional scientists. When faced with failed predictions scientists typically search for problematic auxiliary assumptions rather than reject a target hypothesis. As Thomas Kuhn (1962) famously demonstrated, the history of science is replete with examples. Nineteenth-century astronomers had difficulty reconciling the orbit of Uranus with what Newton's theory of universal gravitation predicted. Instead of rejecting Newton's theory they explained the anomaly by adjusting their background assumption that Uranus is the outermost planet in the solar system. Using Newton's formulae they calculated where the unknown planet should be, trained their telescopes on the spot, and discovered Neptune.

Admittedly, such a strategy does not seem unreasonable (even from a falsificationist perspective) because astronomers were well aware of the limitations of their telescopes. They knew that there were many as yet to be discovered celestial bodies in the solar system, and hence were open to the possibility that deviations from orbits predicted by Newton's theory were caused by gravitational influences of as yet unknown objects. But it is difficult to run this line of argument in the case of the anomalous orbit of Mercury. Adjustment of the analogous auxiliary assumption—Mercury is the closest planet to the Sun—bore no fruit at all. Astronomers searched in vain for a planet (christened "Vulcan" in anticipation of its discovery) between Mercury and the Sun that could explain Mercury's problematic orbit (Baum and Sheehan, 1997). At this stage, a good falsificationist would surely have rejected Newton's theory of universal gravitation. This did not happen. Astronomers turned their attention to other auxiliary assumptions, conjecturing, for instance, that the Sun's mass might not be homogenous. Indeed, up until the development of Einstein's theory of general relativity, which solved the problem by dispensing with Newton's theory of gravitation, astronomers persisted in trying to explain the deviations in Mercury's orbit in terms of a false auxiliary assumption and their speculations grew increasingly implausible. In short, astronomers did not behave like falsificationists. They consistently opted for the logically permissible option of denying an auxiliary assumption in the face of repeated failed predictions.

In summary, traditional accounts of the scientific method (justificationism and falsificationism) are logically flawed and moreover do not provide faithful reconstructions of the evidential reasoning of either experimental or historical scientists. It follows that appeals to the "scientific method" cannot be used to undermine the scientific status of the historical sciences. The following section sketches an analysis of the methodology of historical scientists that is more closely tailored to actual practice than the idealized, logico-mathematical reconstructions that have traditionally dominated thought about scientific methodology. I argue that the inadequacies of the traditional accounts are at least in part due to a failure to recognize that non-logical, causal considerations also play a central role in the evidential reasoning of scientists; as I argue in my (2002, 2001) papers, causal considerations also play a central role in the methodology of the experimental sciences.

THE METHODOLOGY OF HISTORICAL NATURAL SCIENCE: SEARCHING FOR A SMOKING GUN

In my (2011, 2002, 2001) papers, I argue that scientists investigating long past events and processes exhibit a distinctive pattern of evidential reasoning characterized by two interrelated stages: (1) the proliferation of rival hypotheses to explain a puzzling body of traces (effects of past events) discovered in the field, and (2) a search for a "smoking gun" to discriminate among these hypotheses. A smoking gun discriminates among competing hypotheses about long past, particular events by showing that one or more provides a better *explanation* for the total body of evidence (traces) available than the others. In contrast, the acceptance and rejection of hypotheses in classical experimental science depends primarily upon the success or failure of *predictions* inferred from a single hypothesis or set of mutually compatible hypotheses that are tested by artificially manipulating conditions within the sterile confines of a laboratory.

In order to avoid misunderstandings that have occurred in the literature, three clarifications are in order. First, the pattern of evidential reasoning described above does not "define" historical science. Historical scientists sometimes operate like experimentalists, and vice versa (Cleland, 2002). Which pattern of evidential reasoning is used depends upon the evidential situation in which a scientist finds herself. It is because scientists investigating long past, particular events typically find themselves in a different evidential predicament than experimentalists exploring timeless generalizations that the former pattern predominates. Second, the stages identified above for prototypical historical science are not, as Kleinhans et al. (2005) assert, in conflict (Cleland, 2011). Rival hypotheses are formulated on the basis of a body of traces that doesn't include a smoking gun. The discovery of a smoking gun changes the evidential situation by revealing that one or more of these hypotheses provide a better explanation for the total body of evidence *now* available. Considered in isolation, independently of the other lines of evidence, few traces would unambiguously count as a smoking gun for a hypothesis. A smoking gun for a hypothesis is a capstone piece of evidence; it can only be judged as a smoking gun when combined with the rest of the evidence available. Finally, it is important to keep in mind that the findings of historical scientists are just as tentative and subject to revision as those of experimental scientists (Cleland, 2011). The collection of rival hypotheses may be culled and augmented repeatedly in light of the discovery of new evidence or advances in theoretical understanding. And even supposing that a scientific consensus is reached on a single hypothesis its plausibility may be undermined by subsequent research.

The scientific debate over what caused the end-Cretaceous mass extinction illustrates the dynamic interaction between proliferating alternative hypotheses and searching for a smoking gun to discriminate among them that characterize prototypical historical research; see my (2011, 2002, 2001) papers for additional illustrations. Prior to 1980, paleontologists entertained many different hypotheses for the end-Cretaceous extinctions, including climate change, extensive volcanism, pandemic, evolutionary senescence, nearby supernova, and meteorite impact (Powell, 1998, p. 165). Most paleontologists viewed these hypotheses as rivals. None of the evidence available at the time provided strong support for any of them, however, and many paleontologists suspected that we would never know what happened. It thus came as quite a surprise when the father-and-son team of Luis and Walter Alvarez (Alvarez et al., 1980) stumbled onto an iridium anomaly in the K-Pg boundary separating the end of the Cretaceous from the beginning of the Paleogene (previously known as the Tertiary). Subsequent field studies confirmed that the anomaly was a global phenomenon; at some sites in North America levels of iridium were 1000 times higher than background levels (Orth et al., 1981).

The Alvarezes believed that they were in possession of a smoking gun for the mysterious end-Cretaceous extinctions. Earth's crust is depleted in iridium because most of it sank with other heavy elements, like iron, into the mantle and core during planet formation. Although not all meteorites are rich in iridium, those left over from the formation of the solar system typically have higher concentrations. The impact of an asteroid or comet was thus a very promising candidate for explaining the anomalous levels of iridium in K-Pg boundary sediments from around the world. But as volcanologists quickly pointed out, so was volcanism, which brings mantle material to the surface (Officer and Drake, 1985). This alternative was especially attractive given that there is a vast region of flat-lying basalt lava flows in west central India (the Deccan Traps) of approximately the same age as the extinctions. Because none of the other hypotheses could explain the excess iridium scientific attention quickly converged on meteorite impact or volcanism as the most likely cause of the end-Cretaceous mass extinction. In other words, the iridium anomaly functioned as a smoking gun for discriminating meteorite impact and volcanism from their pre-1980 rival hypotheses.

Further investigations supported meteorite impact over volcanism. Field studies undercut the claim that volcanism could produce a global iridium anomaly (e.g., Schmitz and Asaro, 1996). Even more importantly, large quantities of mineral grain, predominately quartz, exhibiting a distinctive pattern (cross-hatched, parallel sets) of fractures were found in K-Pg boundary sediments from around the world (Bohor et al., 1984). It takes enormous pressures to facture minerals in this way, and 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 conjecture that massive volcanic eruptions fracture minerals in this manner (Kerr, 1987; Alexopoulos et al., 1988). The combination of an iridium anomaly and shocked quartz in the K-Pg boundary was enough to convince most members of the geological community that a huge meteorite struck Earth 65 Ma. Additional evidence of meteorite impact—e.g., microspherules (Smit and Klaver, 1981), fullerenes containing extraterrestrial noble gases (Becker et al., 2000), and a huge crater of the right age straddling the Yucatan Peninsula (Hildebrand et al., 1991)—has since been discovered. It is generally conceded by planetary and earth scientists, however, that the combination of an iridium anomaly and shocked quartz cinched the case for meteorite impact over volcanism.

The iridium and shocked minerals weren't enough to convince most paleontologists that the impact caused the mass extinction. The extinctions had to be global 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 paleontologists to suspect that something else (climate change, evolutionary senescence, or extensive volcanism were some popular conjectures) initiated it and that the impact (at best) delivered the coup de grâce. Additional field studies were needed to establish a more convincing causal link between the impact, which was now considered well established, and the extinction event.

Paleontologists went to work, closely studying the fossil records of different kinds of organisms on either side of the K-Pg boundary. Peter Ward (1990) established that the fossil record of the ammonites goes right up to the K-Pg boundary and suddenly disappears. Field studies also documented substantial changes in the morphology of the calcareous shells of tiny planktonic foraminifera on either side of the K-Pg boundary (e.g., Smit, 1982). Paleobotanists made some of the most important discoveries. Using high-resolution techniques, they discovered abundant fossilized angiosperm pollen right up to the lower level of the boundary, at which point it disappears and is replaced on the other side by abundant fossilized fern spores (Johnson and Hickey, 1990). From experience with modern catastrophes, such as the explosion of Mount St. Helens, botanists know that ferns are opportunistic, rapidly colonizing devastated areas. Studies of the fossil record from around the world indicated that the extinction was massive, rapid, and catastrophic. Most paleontologists were won over to the meteorite impact hypothesis, illustrating that a smoking gun may consist of a large and diverse body of new evidence.

Explanatory power, as opposed to prediction, plays the central role in the evidential reasoning of historical scientists. The Alvarezes didn't predict an iridium anomaly in the K-Pg boundary, and then set out to find it. They literally stumbled upon it while trying to figure out how long it took for the boundary layer sediments to be deposited. Indeed, even today there aren't any widely accepted, background assumptions that could logically 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 massive but ancient meteorite impact; these circumstances include an iridium poor meteorite and the possibilities of unrepresentative samples due to, e.g., dispersal of an initial iridium anomaly by geological processes. The significance of the iridium anomaly for the Alvarez hypothesis lies in the fact that (with the possible exception of the volcanism hypothesis) the latter provides a better explanation for the former than any of the competing hypotheses.

Nonetheless, as Derek Turner (2007, 2005) points out, historical scientists often speak of investigating novel "predictions" of their hypotheses about long past events and processes. The fact that they do so does not, however, show that their predictions play the same role in their evidential reasoning as those of classical experimental scientists. Peter Ward's "prediction" (his term) that (if the Alvarez hypothesis is true) ammonite fossils will be found immediately below K-Pg boundary sediments provides a salient illustration, underscoring the constant threat of unrepresentative samples. Exposed outcrops of the K-Pg 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 and well-preserved outcrops of the geological section containing the K-Pg boundary in the world. The closest ammonite that he could find was 10 m beneath the lower level, leading him to suspect that they had become extinct tens of thousands of years earlier (Ward, 1983). When he moved a short distance up the coast to France, however, he found ammonite fossils immediately beneath the lower edge of the boundary. Apparently the ammonites in what is now northern Spain had 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 meteorite-extinction hypothesis after all.

As the above discussion reveals, Ward's "prediction" lacks the precision of those of classical experimental science: It can't be interpreted as asserting that ammonite fossils will be found immediately below the lower edge of the K-Pg boundary in a specific location. At best it may be interpreted as a vague prognostication to the effect that it is *likely* that there are rock sequences *somewhere* on Earth with ammonite fossils immediately below (but not above) the lower edge of the boundary. This point is underscored when one considers that failure to find them in the sea-cut cliffs of southern France wouldn't have counted much against the Alvarez hypothesis either; the local ecological crisis that decimated them in Spain could easily have extended that far north. Paleontologist James Powell provides yet another revealing illustration of the vagueness infecting the predictions of historical scientists. In a popular book, Powell tries to reframe the debate over the Alvarez hypothesis in terms of falsifiable predictions (Powell, 1998, his Chapter 4). One of these predictions is: "A huge impact crater formed 65 million years ago; if it has not disappeared, it may yet be found" (p. 61). As the quote reveals, however, Powell takes back with one hand what he gives with the other: Immediately after making the prediction he concedes that failure to find a crater of the right size and age isn't enough to undermine the Alvarez hypothesis. Seventy percent of Earth is covered by water, which means that the probability that the K-Pg meteorite struck ocean instead of land is very high. If this happened, it is likely that the crater would no longer exist, having been subducted by the active geology of the ocean floor. In this context, Powell's caveat makes good sense, but it undermines his claim to be making a falsifiable prediction.

The problem with most so-called predictions inferred from hypotheses about long past events and processes is that they are too vague to fail. Failure to find ammonite fossils sufficiently close to the lower level of the K-Pg boundary in a particular location doesn't preclude the possibility of finding them elsewhere in some as yet unexplored rock record of the K-Pg boundary, and this would be true even if Ward had failed to find them on the French side of the Bay of Biscay. Similarly, failure to find an impact crater of the right size and age does not count much against the Alvarez hypothesis because it may have been erased in the intervening years by the active geology of the ocean floor. This is in stark contrast to classical experimental science, where failed predictions are viewed as a very serious threat indeed. A good illustration is the intense flurry of activity among physicists last year that greeted experimental evidence suggesting that neutrinos were traveling faster than light, which violates Einstein's theory of special relativity. The anomalous results were fairly quickly attributed to highly specific flaws in the original experimental setup, e.g., a fiber-optic cable improperly attached (Cartlidge, 2012).

As Turner (2007) 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 difficulties faced by historical scientists in identifying plausible interfering conditions that might lead to false negatives. This amounts to implicitly endorsing the widely accepted view that the practices of classical experimental scientists provide the prototype for all of science. As just discussed, however, such an interpretation is belied by the actual practices of historical scientists. Not only are historical hypotheses rarely rejected on the basis of failed predictions, they are sometimes rejected on the basis of evidence that has little or no direct bearing on their truth or falsity. The fate of the contagion hypothesis for the end-Cretaceous extinctions provides a salient illustration. The discovery of a global iridium anomaly has no evidential bearing on whether a contagion extinguished the dinosaurs; a pandemic could have destroyed them shortly before or after the impact. Yet the contagion hypotheses ceased to be taken seriously by paleontologists after the discovery of the iridium anomaly. Why? The iridium anomaly provided strong *positive* support for either the impact of a huge meteor or massive volcanism, either of which has the capacity to produce a mass extinction under the right circumstances. Viewed from this perspective, it is hardly surprising that scientists did not speak of the contagion hypothesis being "falsified" by the newly discovered iridium anomaly; it wasn't. Instead, they focused on the question of whether volcanism or meteorite impact provides the *best explanation* for the iridium anomaly. The point is most historical hypotheses are not rejected on the basis of failed predictions but rather because another hypothesis does a better job of explaining the total body of evidence available.

Predictions that succeed, in contrast, sometimes carry great weight in prototypical historical science. But it is not by virtue of representing a successful novel prediction that they do so. If Ward had stumbled upon ammonite fossils immediately below the K-Pg boundary in France, as opposed to having gone looking for them there, his finding wouldn't be 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 reconstructed in hindsight as novel predictive successes. 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-Pg boundary in a well-preserved outcrop of the pertinent geological section. Furthermore, it is clear that Ward's 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. The point is regardless of the circumstances in which it is acquired, whether a result of "prediction" or serendipity, evidence functions as a smoking gun if it establishes that one hypothesis provides a better explanation than its rivals.

In short, the logical character and evidential role of prediction in historical and experimental science are quite different. The predictions of historical scientists are too vague to specify precise conditions for testing and evaluating hypotheses. They function more as educated guesses—based informally, as opposed to logically, on empirical and theoretical background knowledge—about where additional evidence (ideally a smoking gun) *might* be found and perhaps even what form it *might* take. Ward's vague prediction suggested where to look for evidence that a meteorite impact caused the end-Cretaceous extinctions (the sea-cut cliffs of the Bay of Biscay) and what form it might take (the presence of ammonites immediately below but not above the K-Pg boundary). Even the Alvarezes' discovery of the iridium anomaly can be interpreted as being guided by an extremely vague prediction. For Walter Alvarez, like many geologists, suspected that crucial evidence for the cause of the end-Cretaceous extinctions might be found in the K-Pg boundary, although he had no idea what form it might take. Interpreting the "predictions" of historical scientists as educated guesses about where telling evidence for a target hypothesis might be found helps to explain why successful predictions carry much more weight than failed predictions.

To wrap up, unlike classical experimental science, prototypical historical science is not prediction centered. Hypotheses are accepted and rejected by virtue of their power to explain as opposed to predict the evidence that supports them. A scientific consensus on the meteorite impact hypothesis for the K-Pg extinctions was reached because it explains an otherwise puzzling body of traces, e.g., iridium anomaly, shocked quartz, glassy spherules, etc., and fossil records of ammonites, foraminifera, plant pollen, fern spores, etc. 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 explained this double mystery better than any of its scientifically plausible, available rivals. It is for this reason that it currently dominates scientific thought about what caused the end-Cretaceous mass extinction.

As mentioned earlier, it is important to keep in mind that there are no guarantees that the meteorite impact hypothesis is the final word on the end-Cretaceous mass extinction. All scientific hypotheses and theories are tentative and subject to revision in light of new empirical discoveries or theoretical advances. Just as Einstein's theory of gravity eventually replaced Newton's so another hypothesis may someday replace the Alvarez hypothesis as the best explanation of the end-Cretaceous mass extinction. At the present time, however, the Alvarez hypothesis still dominates scientific thought about the cause of the end-Cretaceous extinctions: In March 2010 an international team of 32 scientists reviewed 20 years' worth of research on the end-Cretaceous mass extinction for the purpose of revisiting the main competing hypotheses. They concluded that the meteorite impact hypothesis remains *the best explanation* for the total body of evidence currently available (Schulte et al., 2010).

JUSTIFICATION IN HISTORICAL SCIENCE: COMMON CAUSE EXPLANATION

The dominant form of explanation in the historical natural sciences is common cause explanation. The basic idea is to attribute a puzzling collection of traces to a common cause. The common cause hypothesis that does the best job of explaining the total body of traces available is judged the most plausible. As philosopher Hans Reichenbach (1956) pointed out some time ago, however, common cause explanations are not grounded in formal logico-mathematical considerations. Whether it is truly rational to infer common (versus separate) causes from puzzling bodies of traces depends upon the truth of the *principle of the common cause*. Roughly speaking, the principle of the common cause asserts that improbable associations (correlations or similarities) are best explained in terms of a shared common cause. The principle of the common cause makes a statistical claim about the temporal structure of causal relations in our universe (Cleland, 2011): The majority of localized cause-and-effect relations form many pronged forks opening in the direction from past to future; the principle of the common cause asserts that most events affect their environments in numerous and diverse ways, producing multiple lines of potential evidence (in the form of correlations and similarities) that persist into the future. If causal relations were structured differently in time—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 ostensibly improbable associations among traces.

The principle of the common cause holds forth the promise of making good sense of the close relationship between explanation and confirmation in the evidential reasoning of historical scientists (Cleland, 2011). Attributing puzzling similarities and correlations among traces to a common cause has great explanatory power because 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 anomaly and extensive quantities of shocked quartz in the K-Pg boundary provide a good illustration. 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 the impact of a huge meteorite. This helps to explain why historical scientists have a tendency to focus their investigations on what seems to them (in light of their background knowledge) to be the most puzzling associations among traces. The question is why should we believe that the principle of the common cause is true?

The answer is because there are compelling empirical reasons for believing that localized events in our universe are causally connected in time in an asymmetry manner; this thesis is known as the "asymmetry of overdetermination" (Lewis, 1979). According to the asymmetry of overdetermination, most local events evidentially *over*determine their past causes (because the latter typically leave numerous and diverse effects) and *under*determine their future effects (because they rarely constitute the total cause of an effect). As an illustration consider an explosive volcanic eruption. Extensive deposits of ash, pyroclastic debris, masses of andesitic or rhyolitic magma, and a large crater are produced. Inferring that the eruption occurred after the fact does not require recovering it all. Any one of an enormous number of remarkably small subcollections will do. This helps to explain why volcanologists can confidently infer the occurrence of a massive caldera-forming eruption 2.1 Ma in what is now Yellowstone National Park. In stark contrast, inferring the occurrence of near future events such as the next eruption of Mount Vesuvius is much more difficult. For 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 an imminent volcanic eruption with any degree of confidence, which brings us to 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. Put provocatively, the present does not contain traces (records) of future events as it does of past events. Viewed from this perspective the historical sciences have an advantage over classical experimental science.

The asymmetry of overdetermination is very familiar to physicists. Examples such as explosive volcanic eruptions fall under the second law of thermodynamics (statistically interpreted). The natural processes that produce volcanic eruptions are irreversible. Volcanoes never swallow up all the debris that they spew out. The asymmetry of overdetermination also applies to wave phenomena, which do not admit of an obvious thermodynamic explanation. Although traditionally associated with electromagnetic radiation, the radiative asymmetry characterizes all wave-producing phenomena, including disturbances in water and air. Waves invariably spread outward, as opposed to inward, as time progresses. Light waves reaching us from a distant star never reverse themselves, contracting back upon their source until they are finally reabsorbed. Between the second law of thermodynamics and the radiative asymmetry, all physical phenomena above the quantum level are subject to the asymmetry of overdetermination. The asymmetry of overdetermination is thus empirically well grounded in physical theory.

The asymmetry of overdetermination physically underwrites the principle of the common cause, and hence the distinctive methodology of prototypical historical research. Because most localized events have numerous and diverse effects, most local cause-and-effect relations form many-pronged forks opening in the direction from past to future. As a consequence it is likely (but not certain) that a seemingly improbable association among traces found in the contemporary environment is due to a common cause, as opposed to separate causes. The search for a smoking gun, which lies at the heart of the methodology of historical science, is a quest for telling empirical evidence for a common cause hypothesis. Such evidence is likely to exist *if* the traces truly share a common cause. For the contemporary environment is likely to contain many potential (as yet undiscovered) smoking guns for identifying the common cause of an otherwise puzzling association among traces. Because the significance of a smoking gun can only be recognized in the context of an appropriate common cause hypothesis, historical scientists proliferate alternative common cause hypotheses for a given body of evidential traces (rather than, as in classical experimental science, focusing on a single hypothesis), and subsequently search for a smoking gun to discriminate among these hypotheses. Following the principle of the common cause, the hypothesis (or hypotheses) that best explains the total body of evidence available (which includes the newly discovered smoking gun) is judged most likely to be true.

The principle of the common cause does not assert that every ostensibly improbable association among traces is the result of a common cause; the claim is only that this is highly likely to be true. It should thus come as no surprise that scientists sometimes entertain separate causes hypotheses for a puzzling body of traces. Nevertheless, in light of the causal structure of our universe, one would expect common cause explanation to be the default mode of evidential reasoning among historical scientists. As paleontologist Douglas Erwin counsels in a discussion of rival common cause hypotheses for the end-Permian mass extinction, scientists prefer "single" (common) causes to "multiple" (separate) causes except when faced with evidence that is difficult to explain in terms of a plausible common cause (Erwin, 2006, p. 11, 54, 58).

Recent field studies of the end-Cretaceous extinctions provide a revealing illustration of the conditions under which common cause explanation may be supplanted by separate causes explanation. Fossil evidence discovered on Seymour Island off the Antarctic Peninsula suggests that there may have been a mass extinction of marine life 200,000 years earlier than the K-Pg meteorite strike (Tobin et al., 2012). The authors of the study concede that the evidence for the impact of a massive meteorite 65 Ma is overwhelming but conjecture that it may not have been responsible for the marine extinctions. They hypothesize that the end-Cretaceous marine extinctions were caused by the Deccan Traps flood volcanism and the terrestrial extinctions were caused by a subsequent meteorite impact; the authors caution that additional field studies are needed to confirm that the marine extinction was a worldwide (versus local) phenomenon. At this time it is unclear whether this separate causes hypothesis will eventually supplant the Alvarez hypothesis. The important point for our purposes is that it illustrates Erwin's point about scientists preferring common cause hypotheses *except* when faced with definite empirical evidence (or theoretical developments) suggesting separate causes. This reflects the statistical character of the principle of the common cause: Most (but not all) puzzling associations among traces are best explained in terms of common causes.

The thesis of the asymmetry of overdetermination does not imply that every past event is overdetermined by traces in the present-day environment. It is unlikely but nevertheless possible for a past event to leave no traces in the present. Events occurring before the big bang of cosmology are prime candidates. Furthermore, the causal information carried by traces becomes increasingly degraded with the passage of time. A major focus of historical research is thus on analyzing, sharpening, and interpreting traces of long past events and processes (Cleland, 2011, 2002). The extent to which information carried by traces becomes unrecoverable or even completely destroyed, however, is an open question. Following Sober (1988), Turner (2007, 2005) contends that the threat is so severe as to undermine the evidential significance of the asymmetry of overdetermination. It is important, however, to distinguish information that is currently inaccessible to scientists from information that is completely lost. The development of increasingly powerful analytical tools for extracting information from traces of the past underscores this point. The discovery of an iridium anomaly in K-Pg boundary sediments couldn't have been made before the development of particle accelerators. Similarly the Chicxulub crater, currently thought to be ground zero for the meteorite impact implicated in the end-Cretaceous extinctions, was first detected by means of aerial surveys of the northern coast of the Yucatán Peninsula using a gravimeter, which revealed a gigantic (at least 170 km in diameter), circular gravity anomaly buried a kilometer beneath younger sedimentary rock (Penfield, 1991). Other illustrations include evidence that life on Earth goes back at least 3.8 billion years, which rests upon laboratory analysis of carbon isotope ratios in minuscule grains of rock that reveal an otherwise inexplicable enrichment in the lighter isotope of carbon (C-12), which is preferred by life, over the heavier isotope (C-13) (Mojzsis et al., 1996).

Turner (2007, 2005) nonetheless contends that such cases are the exception rather than the rule, citing the colors of dinosaurs as an illustration of something that paleontologists will never be able to discover. His choice of illustration is somewhat ironic, however, because scientists have since been able to reconstruct the color patterns of a couple of small feathered theropod dinosaurs through the discovery and analysis of preserved melanin granules found in exceptionally well-preserved specimens from China (Zhang et al., 2010; Li et al., 2010). The point is that one can never rule out the possibility of discovering a smoking gun for a conjectured, long past historical event however far-fetched this possibility may currently seem. The challenge of course is recognizing a trace for what it represents, and this often requires the development of sophisticated analytic methods; in the absence of such methods, historical scientists may have little recourse but to resign themselves to a collection of equally viable, rival hypotheses. Nevertheless, as the illustrations just discussed underscore, the ability of historical scientists to extract information about the past from traces is rapidly increasing, so much so that I suspect that the twenty-first century may become the age of historical science!

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Common cause explanation and the search for a smoking gun

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Notes