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Culture's Impact on the Historical Sciences

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Abstract

In this paper I introduce the thesis of cultural readiness about science found in the historical analysis of the Alvarez impact hypothesis of the end-Cretaceous mass extinction. Cultural readiness posits that in some scientific domains, there are scientifically apt questions, methodologies or theories that are only developed, considered, and adopted by a scientific community once some combination of empirical *and* cultural factors obtains within and without that domain. I demonstrate that 21st century philosophy of the historical sciences has been motivated by a commitment to legitimization and has prioritized epistemic ingenuity and has not addressed cultural readiness. I then argue that one vehicle for cultural readiness in the historical sciences is their use of narrative explanatory forms. Narratives offer an arena to blend cultural and empirical phenomena by their characteristic elicitation of familiarity and emotionality.

Keywords

cultural readiness – end-Cretaceous extinction – epistemic ingenuity – geosciences – narratives – paleontology – scientific success

1 Introduction

The account of the historical sciences that has emerged from the *philosophy* of science in the 21st century has been the result of a legitimizing project that has emphasized epistemic ingenuity.[1] That is, according to this received view, the

1 See, e.g., A. Bokulich, "Using models to correct data: Paleodiversity and the fossil record", *Synthese* 198 (2021), 5919–40; A. Currie, "Marsupial lions and methodological omnivory:

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legitimate status and success of sciences like paleontology, archaeology, and geology is owed to clever evidential reasoning and borrowed methods carefully deployed to explain and reconstruct earth's past.

Recent *history*[2] and *anthropology*[3] of the historical sciences paints a different picture of their success, however; one which ties some significant discoveries to the broader cultural context of the time.[4] David Fastovsky argues that palaeobiological theories, "gained a foothold not only because the interpretations were supported by discoveries, but because the social climate was ripe for these kinds of inferences."[5] He further argues that the broader socio-political contexts influenced the culture of geoscientific communities to such a degree that they were (or were not) "culturally ready"[6] to accept some historical scientific conclusions.

In this paper I argue that, by focusing on the legitimizing project and emphasising epistemic ingenuity, philosophers have overlooked another distinguishing feature of the historical sciences noticed by historians: cultural readiness. The purpose of this paper is to characterize what it means for science to be culturally ready in the context of the end-Cretaceous impact hypothesis episode in the history of the geosciences. Interestingly, both philosophers and

Function, success and reconstruction in paleobiology", *Biology & Philosophy*, 30(2) (2015), 187–209; A. Currie, *Rock, Bone, and Ruin: An Optimist's Guide to the Historical Sciences*, (Cambridge, MA: The MIT Press, 2018); C.E. Cleland, "Methodological and Epistemic Differences between Historical Science and Experimental Science", *Philosophy of Science*, 69(3) (2002), 474–496; C.E. Cleland, "Prediction and Explanation in Historical Natural Science", *The British Journal for the Philosophy of Science*, 62(3) (2011), 551–582; P. Forber, & E. Griffith, "Historical Reconstruction: Gaining Epistemic Access to the Deep Past", *Philosophy and Theory in Biology*, 3 (2011); B. Jeffares, "Testing times: Regularities in the historical sciences", *Studies in History and Philosophy of Science Part C: Studies in History and Philosophy of Biological and Biomedical Sciences*, 39(4) (2008), 469–475; but, for an exception see, D.D. Turner, *Paleoaesthetics and the Practice of Paleontology* (Cambridge: Cambridge University Press, 2019).

2 See, e.g., D. Sepkoski, *Catastrophic Thinking: Extinction and the Value of Diversity from Darwin to the Anthropocene* (Chicago: University of Chicago Press, 2020) and L. Rieppel, *Assembling the Dinosaur: Fossil Hunters, Tycoons, and the Making of a Spectacle* (Cambridge, MA: Harvard University Press, 2019).

3 See, e.g., B. Noble, *Articulating Dinosaurs: A Political Anthropology* (Toronto: University of Toronto Press, 2016).

4 For less recent accounts see also, M.J.S. Rudwick, *The Great Devonian Controversy: The Shaping of Scientific Knowledge among Gentlemanly Specialists* (Chicago: University of Chicago Press, 1988) and W.J.T. Mitchell, *The Last Dinosaur Book: The Life and Times of a Cultural Icon* (Chicago: University of Chicago Press, 1998).

5 D.E. Fastovsky, "Ideas in Dinosaur Paleontology: Resonating to Social and Political Context", in D. Sepkoski and M. Ruse, eds. *The Paleobiological Revolution* (Chicago: University of Chicago Press, 2009), 240.

6 Ibid., 249.

historians use this case as an exemplar of epistemic ingenuity and cultural readiness, respectively. Cultural readiness, which emphasises the role of cultural context as it bears on scientific discovery, is a different kind of thesis than the thesis of epistemic ingenuity put forward by philosophers. More specifically, cultural readiness posits that a combination of empirical and cultural factors is a prerequisite to account for episodes of historical scientific success. I claim that what Fastovsky is offering is a thesis about culturally ready theory change that the philosophical project has yet to develop.

I then propose one mode of action for cultural readiness in the Alvarez episode: distinctive kinds of narrative explanation sometimes effectively draw in cultural cues which influence the assessment of theories.[7] But not only that, culture can play a role in the questioning of long held scientific assumptions that then influence theory uptake by a scientific community. By paying attention to the ways that culture influences science – especially when we deem that science to have been successful, as we do in the end-Cretaceous mass extinction case – we get a better picture of the way science changes and adapts over time.

In the next section I begin by describing the Alvarez hypothesis and its uptake by the scientific community. After that, (sections 2.1 and 2.2) I engage with the philosophical and historical descriptions of the Alvarez episode and highlight the differences. Then, in section 3 I suggest a possible mode of action for the kind of cultural influence that Fastovsky outlines which relies on the fact that historical scientists typically engage in the construction of narrative explanations in practice.

2 A Two-Faced Exemplar: The End-Cretaceous Impact Hypothesis and Historical Scientific Success

> In brief, our hypothesis suggests that an asteroid struck the earth, formed an impact crater, and some of the dust-sized material ejected from the crater reached the stratosphere and was spread around the globe. This dust effectively prevented sunlight from reaching the surface for a period of several years, until the dust settled to earth. Loss of sunlight

7 See, D. Turner and A. AboHamad, "Narrative Explanations and Non-epistemic Value," (this issue) for more on the ways narrative explanations are infused with non-epistemic values. Relatedly, metaphors may be another candidate to account for cultural readiness as Currie demonstrates with the use textual metaphors across time in the historical sciences in A. Currie, "Of Records & Ruins: Metaphors about the Deep Past," (this issue).

> suppressed photosynthesis, and as a result most food chains collapsed and the extinctions resulted.
>
> A second food chain is based on land plants. Among these plants, existing individuals would die, or at least stop producing new growth, during an interval of darkness, but after light returned they would regenerate from seeds, spores, and existing root systems. However, the large herbivorous and carnivorous animals that were directly or indirectly dependent on this vegetation would become extinct.[8]

The above presentation of a catastrophic asteroid impact which resulted in a mass extinction event ~66 million years ago was published in *Science* by Luis and Walter Alvarez – a father and son scientist duo – and their colleagues in 1980. Paleontologists had known for a long time that the non-avian dinosaurs (and other groups) went extinct around 66 million years ago as their fossils abruptly disappear from the fossil record after the Cretaceous. Alvarez et al. discovered that the thin section of rock at the Cretaceous-Paleogene (K-Pg) boundary contained a high concentration of the element iridium. Iridium is known to fall to earth from space at a constant rate, so the detection of some amount of iridium is expected in any rock formation. However, the K-Pg boundary had about 30 times the normal amount, so they inferred that an extra-terrestrial impact was responsible for depositing large quantities of the element resulting in the high concentration. They also inferred that this impact caused a mass extinction.

Alvarez et al.'s contribution turned what had been a free-for-all debate about mass extinctions, wherein many kinds of hypotheses were considered, into a more focused investigation.[9] The debate before 1980 was a free-for-all because the hypotheses on the table were largely equal in measure – climate change and volcanism being the two main contenders – finding only small evidential support, and nothing to suggest one over the other. The Alvarez et al. paper made waves and distinguished itself by providing a novel inference which solidified the impact hypothesis as a main cause of the K-Pg mass extinction after about a decade of debate. The question of intrigue to many philosophers is, given that the historical sciences seem at an epistemic disadvantage compared to experimental sciences, relying heavily on degraded traces of long past events, what accounts for the success of this hypothesis over the others on

8 L.W. Alvarez, W. Alvarez, F. Asaro, & H.V. Michel, "Extraterrestrial Cause for the Cretaceous-Tertiary Extinction", *Science*, 208(4448) (1980), 1105–1106.

9 Thanks to an anonymous reviewer for pointing out the free-for-all nature of these debates.

offer? Also, we might ask more generally, what accounts for success in the historical sciences?

Philosophers of science interested in the historical sciences – e.g., geology, paleontology, archaeology, etc. – have offered answers mainly by analyzing the empirical practices of the scientists involved in the reconstruction of long past events. Carol Cleland says a historical causal hypothesis wins out over others once a crucial piece of evidence distinguishes it as the best causal story (more on this below).[10] Ben Jeffares argues that the historical sciences are just as good at reasoning about regularities and the causal nature of the world as experimental sciences with the aim to close the conceptual divide between them.[11] Further, Adrian Currie writes an "Optimist's Guide to the Historical Sciences" in which his aim is to convince his readers of the methodological ingenuity of historical scientists, and thus to be optimistic about their ability to know about the entities and events of the deep past.[12] His general answer to the question of success is that historical scientists engage in careful evidential reasoning and adopt diverse methodologies to arrive at explanations of events in the deep past. In Cleland's, Jeffares', and Currie's work the success of the historical sciences is owed to the carefully tailored epistemic practices of the scientists engaged in this kind of theorizing about the past.

However, the paleontologist and historian, David Fastovsky has a different kind of answer to the question of the success of historical hypotheses (specifically, the impact hypothesis): "The most important answer to this question is that it withstood tests, subsequently adduced, that might have falsified it. *But it also worked because the geosciences as a discipline were culturally ready for an idea like this*."[13] Fastovsky's answer contains the kind of philosophical epistemic ingenuity outlined above, but also includes an additional cultural component that provides a new spin on philosophical accounts of success in historical science.

My aim in the next subsections is to narrow focus to the philosophical and historical analyses of the impact hypothesis for the K-Pg mass extinction. First, the impact case has been used in service of the philosophical project, so section 2.1 surveys a handful of philosophers who discuss it as an exemplar of

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10 Cleland, "Methodological and Epistemic Differences between Historical Science and Experimental Science," and Cleland, "Prediction and Explanation in Historical Natural Science."

11 Jeffares, "Testing times: Regularities in the historical sciences."

12 Currie, *Rock, Bone, and Ruin: An Optimist's Guide to the Historical Sciences* (Cambridge MA: MIT Press, 2018).

13 Fastovsky, "Ideas in Dinosaur Paleontology: Resonating to Social and Political Context," 249; emphasis added.

successful historical scientific reasoning. Second, the impact hypothesis case has been used by historians as an exemplar of the thesis of cultural readiness that I will introduce in section 2.2. I seek to describe how the Alvarez episode is used in service of distinct theses about scientific success.

2.1 *Epistemic Ingenuity and the End-Cretaceous Impact Hypothesis*

Historical scientists study and reconstruct the distant past (at scales ranging from thousands to billions of years ago), so the questions they pursue, the methods they employ, and the explanations they offer are thought to be different in kind from the paradigmatic experimental sciences. Some have taken the difference in target and methodology to be suggestive of a difference in quality of results – that is, the epistemic grounds on which historical sciences stand is quite shaky, or so it was argued.[14] In the last two decades or so, the historical sciences have become more frequently studied by philosophers of science. Much of the motivation behind part of the philosophical project has been to legitimize the historical sciences and bolster their scientific status. This has revealed that historical scientists are epistemically ingenious in the face of seeming disadvantages owed to time's destructive forces and that the epistemic foundation the historical sciences find themselves upon is, in fact, quite sturdy.

Authors of the legitimizing project employ a common argumentative structure to demonstrate the epistemic ingenuity of historical scientists. Usually, a disadvantage facing the historical sciences is highlighted and a solution to that disadvantage is offered accompanied by the analysis of salient features of an exemplar case. Cleland and Forber & Griffith both deploy this kind of structure in their projects, and both use the impact hypothesis episode as their case of exemplary historical scientific reasoning and success.

To start, Cleland and Forber & Griffith agree that the problems facing historical sciences are basically rampant underdetermination problems since historical scientists are closed off to performing controlled and repeatable experiments on past entities and events.[15] Instead, historical scientists must rely on the uncovering of traces of past events (e.g., fossils, minerals, craters, etc.). However, because the time scales between an event and its traces are vast, the traces are subjected to biasing by degradation and manipulation leading to data loss. This places historical scientists at an epistemic disadvantage.

14 See, e.g., H. Gee, *In Search of Deep Time* (The Free Press, 2000).

15 For a defense of diverse experimental methods, especially as the historical sciences deploy them, see E. Desjardins et al., "On the ambivalence of control in experimental investigation of historically contingent processes," (this issue).

Cleland further argues that a problem of time asymmetry is responsible for the distinct methodologies adopted by historical and experimental sciences. This is all to say that, according to the legitimizing project, historical sciences face a different suite of problems, generated by underdetermination and other metaphysical constraints. Forber & Griffith call this a problem of epistemic access.

Cleland's solution to these problems of access is to notice that historical scientists discriminate between explanatory hypotheses by searching for "smoking guns." A smoking gun is a trace that "unambiguously discriminates one hypothesis from among a set of currently available hypotheses as providing 'the best explanation' of the traces thus far observed."[16] Cleland describes the iridium anomaly that Alvarez et al. discovered as a smoking gun piece of evidence that elevated the extra-terrestrial impact and the volcanism hypotheses, discriminating them from the others on offer. Then, in 1984 a team of scientists[17] discovered significant levels of shocked quartz in the K-Pg boundary, which is only found in two places on earth: the sites of nuclear detonation and in asteroid impact craters. The shocked quartz "clinched the case"[18] for an asteroid impact, and geologists began their search for an impact crater, to be found a short time later at the tip of Mexico's Yucatan Peninsula.

Forber & Griffith take Cleland's analysis further, rejecting the view that "smoking gun" reasoning is constitutive of successful historical scientific work.[19] Their view, while still a unificationist view, prioritizes the consilience of independent evidential inferences over the mere unifying of individual traces. While they agree that iridium did play a "smoking gun" kind of evidential role, the real strength of the Alvarez et al. inference was in its drawing together of multiple lines of evidence. For example, Forber & Griffith make note of the fact that Alvarez et al. predictions about the size of the impacting body were made on the basis of two separate evidence streams. First, based on the high concentration of iridium in the K-Pg boundary and the known concentration other iridium-rich bolide objects, they were able to generate a predicted diameter range between 6.6 and 14 kms. A second size prediction was made based on the observed 1 cm thick boundary layer, and an assumption that it was composed of ejecta from an impact. This resulted in an estimate of a diameter of 7.5 kms.[20]

16 Cleland, "Methodological and Epistemic Differences between Historical Science and Experimental Science", 481.

17 B.F. Bohor, E.E. Foord, P.J. Modreski, & D.M. Triplehorn, "Mineralogic Evidence for an Impact Event at the Cretaceous-Tertiary Boundary", *Science*, 224(4651) (1984), 867–869.

18 Cleland, "Methodological and Epistemic Differences between Historical Science and Experimental Science", 482.

19 Forber & Griffith, "Historical Reconstruction: Gaining Epistemic Access to the Deep Past."

20 Ibid., 7.

Thus, there emerged an agreement between the estimates based upon observations of traces and assumptions about their origins. The agreement between them – and the distinctness of the auxiliary assumptions – serves to reinforce the strength of the hypothesis about the causes of the traces. For Forber & Griffith, this kind of consilience of distinct evidential streams is constitutive of successful scientific work to generate robust historical reconstructions.

Cleland and Forber & Griffith use the Alvarez case to argue that the historical sciences, with their distinctive forms of empirical reasoning, are epistemically legitimate. Forber & Griffith dedicate their entire analysis to a discussion of the nature of successful historical scientific reasoning. However, Cleland takes the discussion further and dedicates some time to talk about the acceptance of the impact hypothesis into the scientific community. Cleland explains,

> The asteroid-impact hypothesis became the widely accepted explanation for the extinction of the dinosaurs. For of the available hypotheses, *it provided the greatest causal unity to the diverse and puzzling body of traces* (fossil record of the dinosaurs, fossil record of the ammonites, etc., and iridium anomaly, shocked quartz, Chicxulub Crater, etc.).[21]

Cleland points out that, while the geological community was largely united in accepting that an extra-terrestrial impact had occurred 66 million years ago, many paleontologists would not adopt the idea that it had caused a mass extinction – Cleland calls this the "second prong of the Alvarez hypothesis."[22] She describes a massive cross-disciplinary expedition to investigate the plausibility of the second prong, which successfully unearthed many discoveries including the discovery of the extinction of the ammonites at the K-Pg, changes in morphology of bivalves at the K-Pg, and changes from angiosperm to fern pollen fossils at the K-Pg, all suggestive of a mass extinction event for various reasons. Cleland's conclusion, based on the above discoveries, is that "[m]ost 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."[23]

The investigations inspired by both prongs of the Alvarez hypothesis resolved when scientists were won over by the evidence. This story of scientific success and of theory adoption seems good on its face as it holds up a rationalist

21 Cleland, "Methodological and Epistemic Differences between Historical Science and Experimental Science", 483; emphasis added.

22 Cleland, "Prediction and Explanation in Historical Natural Science", 557.

23 Ibid.

picture of science: a healthy skepticism is maintained at first but undone only when new evidence is discovered and unified. Cleland describes historical scientists in the business of evidential reasoning and formulating explanations; a wholly epistemic enterprise that often results in the successful explanations of long past events. It is for this reason that Cleland places historical and experimental sciences on equal epistemic footing.

While Forber & Griffith ultimately disagree with Cleland's analysis, they can all can be seen to be engaging in the same philosophical project of legitimization. The purpose of the preceding exploration of this project is not to offer any sort of criticism – I see it as a fruitful and successful example of philosophical analysis. It makes perfect sense that a project legitimizing the historical sciences in the face of a skepticism about their epistemic status would result in the analysis of epistemic practices and their merits. Cleland's and Forber & Griffith's aims were clear, and they delivered. However, I want to propose that by focusing philosophical attention on purely epistemic features, perhaps another kind of distinguishing feature of the historical sciences has been overlooked. I turn now to a discussion of cultural readiness as it has been discussed by historians.

2.2 *Cultural Readiness and the End-Cretaceous Impact Hypothesis*

In this section I introduce the idea of cultural readiness as it appears in the historical analysis of the Alvarez impact hypothesis. I argue that cultural readiness posits that in some scientific domains, there are scientifically apt questions, methodologies or theories that are only developed, considered, and adopted by a scientific community once some combination of empirical *and* cultural factors obtains within and without that domain. Conversely, in those same domains scientists fail to notice the significance of discoveries – and may even actively resist them – when these dual factors have not obtained. The Alvarez impact episode is taken to be an instance of an apt theory whose significance was noticed when a combination of cultural and empirical factors obtained. In what follows I describe the historical analysis which supports this view.

The aim of the historical project seeks to align trends in culture with changes occurring in the sciences. Fastovsky notices that one direction of this relationship – the influence of paleontology on culture – has been explored in depth, but not the vice versa: culture on paleontology.[24] Fastovsky begins

24 Other interesting work in the sociology of science has illuminated the interaction of paleontology and the wider culture. See, e.g., E.D. Jones. *Ancient DNA: The Making of a Celebrity Science* (New Haven and London: Yale University Press, 2022), in which she argues that the release of *Jurassic Park* in the 1990s (the novel and the film) so influenced the research

his exploration of the acceptance of the Alvarez hypothesis by the scientific community with a question that he seems puzzled by: "In short, despite the absence of any real data about dinosaurs and the pace of their extinction, the theory invoked a deus ex machina ending for dinosaurs (and other organisms) at the end of the Cretaceous. On the face of it, it was absurd – so why did it catch fire?"[25] Fastovsky here is asking mostly about the second prong of the Alvarez hypothesis that describes a sudden extinction event that many paleontologists denied even while accepting an extra-terrestrial impact had occurred.

Cleland's answer to this question as to why the hypothesis caught on so quickly is that a multi-disciplinary effort uncovered traces which offered smoking gun evidence for the second prong – the impact-induced extinction hypothesis. Fastovsky agrees but also adds another component: "The most important answer to this question is that it withstood tests, subsequently adduced, that might have falsified it. But it also worked because the geosciences as a discipline were culturally ready for an idea like this."[26] As Fastovsky presents it, the Alvarez hypothesis was adopted in the context of two distinct but interrelated cultural contexts interacting with one another. There is the broader socio-political context of the time before and during the Alvarez proposal, and nested within it is the scientific culture (as a whole and as scientific subcultures). To understand Fastovsky's thesis, it is important to understand what the wider culture was before and during the Alvarez hypothesis debates. I will then fill in the account and describe how the broader societal culture influences the nested scientific cultures.

The broader cultural context that Fastovsky takes to have been influential on the development and acceptance of the Alvarez hypothesis is the Cold War setting that it was developed and situated within. Specifically, Fastovsky recalls that,

> In 1977, the movie *Star Wars* hit the theaters and rapidly attained cult status. "Star Wars" became the popular name of the antimissile defense program instituted by Ronald Reagan for the protection of the United States from intercontinental missile attacks. The idea, therefore, that destruction could come from above – even space – had reached popular radar as of the 1980s.[27]

program of ancient-DNA that a focus on the research as data-driven or theory-driven misses key elements of the practice, and it should instead be understood under the title of what she calls "celebrity-driven research."

25 Fastovsky, "Ideas in Dinosaur Paleontology: Resonating to Social and Political", 249.

26 Ibid.

27 Ibid., 250.

Further, Fastovsky notes a few other connections between the Cold War and paleontological research about the dinosaur extinction: the first Snowbird Conference of 1981, while largely inspired by an interest to interrogate the Alvarez hypothesis, many were also interested in determining the effects of large-bodied impacts with earth; the language of the magnitude of nuclear detonations – kilotons and megatons – began to be used to describe potential and historic meteor impacts with the earth; and Carl Sagan's "nuclear winter" scenario devised in 1983 invoked the same kind of imagery that the Alvarez hypothesis did. The connection between the state of the world during the Cold War, under threat of nuclear annihilation, became tied into the extinction of the dinosaurs even in political speech as the West's nuclear arsenal was referred to as a 'dinosaur' to indicate its outdatedness.[28]

More recently, David Sepkoski complements Fastovsky's thesis with an in-depth exploration of the evolution of the interaction between science and society in extinction debates, largely converging on the same kind of reasoning Fastovsky presents. Sepkoski describes in great detail what he coins, an "extinction imaginary." Sepkoski explains,

> The complex web of values and beliefs associated with extinction at any given historical period forms what I will call, to use an academic term of art, an extinction "imaginary." The way we understand extinction – the extinction imaginary of any given time – is ultimately tied to the way we conceive of the basic stability and security of the continued existence of our own species.[29]

28 Ibid. Fastovsky also details the case of the description and redescription of the *Tyrannosaurus Rex* in palaeobiological work through its original discovery in 1902 through the 1980s after more samples had been collected. Social change over this period saw with it the description of *T. Rex* moves from domineering to maternal, matching periods of heightened imperialism and then bourgeoning feminism, respectively. Also, Fastovsky illustrates the paleontological interpretation by Jack Horner of *Maiasaura*, or "The Mother Dinosaur." Based on the discovery of fossilized eggs in a clutch in a nesting site in 1978, Horner interpreted the Maiasaura in a new way as caring for their young in complex social structures. Interestingly, pre-1978, the evidence was quite clear that dinosaurs laid eggs, and lived in social groups, but it took the emergence of a wider culture that valued family and socialization post-1950s to arrive at this unified explanation, or so Fastovsky claims. For a more detailed exploration of both of these cases – the paleontological reconstruction of *T. Rex* and *Maiasaura* – see Brian Noble's *Articulating Dinosaurs: A Political Anthropology.*

29 Sepkoski, *Catastrophic Thinking*, 6.

The ways that scientists and society at large think about concepts like extinction change through time based upon scientific discoveries, but also by way of socio-cultural attitudes and preferences, and the interaction of the two domains. Sepkoski uses these insights to explain how views of catastrophic mass extinctions emerged after nearly two centuries of views of extinction as inevitable and gradual. He writes, "I came to be convinced that it was no accident that catastrophic mass extinction became an object of scientific study and popular fascination at precisely the moment when we imagined a similar fate for ourselves,"[30] echoing Fastovsky.

Sepkoski begins his analysis of extinction imaginaries as they emerge in the Victorian period, as scientific views of evolution endorsed a model of improvement of the fittest animals. Extinction was just one mechanism in play weeding out the unfit animals, and humans more importantly. Intertwined with the scientific understanding was the political context of imperialistic endeavors into Africa and North and South America. The scientific understanding was used to justify cultural and physical genocide, as it was just the "normal" progression of the stronger vs. the weaker. Further, scientists "confirmed" their theories of evolutionary change by witnessing the collapse of the Native populations of these regions (seemingly without an awareness of the real causes). Extinction, for the Victorians, was just a thing in nature that acted almost of its own volition, and the colonialists just happened to be on the beneficial side of it. Sepkoski writes, "extinction was understood to be nature's way of strengthening and improving itself by weeding out the unfit, and competition was celebrated as the source of natural progress. This view supported Victorian ideologies of social progress and imperial expansion and justified a lack of concern about the inevitable victims of progress."[31] The extinction imaginary of the Victorian age was a complex intertwining of the cultural setting and the scientific understanding of the day, both feeding off of one another.

The imaginary took a sharp turn after the destruction of the World Wars and ensuing flirtation with nuclear annihilation by the world's superpowers during the Cold War period. No longer was extinction viewed as a passive, inevitable force of nature, but a looming catastrophe brought about by human means. The popular culture of the post-war period reflected this anxiety. Publications with a post-apocalyptic theme burst into what was a mostly devoid genre. This had an impact on science too as "it opened the door for a reconsideration of … extinction, as a potentially catastrophic threat of vital personal concern to every member of the human species."[32] Sepkoski continues,

30 Ibid., 8.

31 Ibid., 81.

32 Ibid., 129.

> On the one hand, nuclear annihilation provided a vivid image of the reality of world-altering physical cataclysm; on the other, empirical recognition of the reality of geological mass extinctions, which began to take hold in the late 1950s, gave historical validation to doomsday prophecies. And as time went on, models of the mechanisms and ecological consequences of catastrophic extinctions became the basis for predicting the effects of nuclear and ecological catastrophes of the present or future.[33]

Sepkoski shares with Fastovsky the view that the air of looming nuclear annihilation influenced the K-Pg mass extinction theory. Sepkoski's descriptions of the extinction imaginary directly preceding the introduction of the K-Pg Mass Extinction theory demonstrate a handful of cultural cues. That extinction was no longer a distant-from-human phenomenon caused a re-think of the value of the future and preserving it at all costs, and deterrence became a popular social goal.

It is here that we can jump into the second way of understanding cultural readiness, as internal to science as an institution. The geosciences were undergoing a slow internal revolution, overthrowing old views for new; a new catastrophism was replacing facets of the old gradualism. Catastrophism is a view often associated with theologically motivated scientists of the 19th century. They theorized, in opposition to those deemed gradualists, that the earth we see today is the result of punctuated, catastrophic events (like the Noachian flood).[34] The "new catastrophism" emerged in the post-war era as concerns about the destructive forces of humans became increasingly salient. Notably, Stephen Jay Gould explains the pervasiveness of gradualism in paleontological circles at the time influencing the initial uptake of Alvarez's theory:

> the extra terrestrial impact theory soon proved its mettle in the most sublime way of all – by Darwin's criterion of provoking new observations *that no one had thought of making under old views*. The theory, in short, engendered its own test and *broke the straitjacket of previous certainty.*[35]

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33 Ibid., 132.

34 A notable exception to the association between theological motivations and a catastrophist interpretation of earth's deep past is Georges Cuvier, who was a devoted catastrophist but also an open secularist. Thank you to an anonymous reviewer for this insight.

35 S.J. Gould, *Dinosaur in a Haystack: Reflections in Natural History* (Harmony, 1995), 152. Also, for more on the cultural background of Gould's own views concerning evolutionary theory see A. McConwell, "George G. Simpson and Stephen J. Gould on Values: Shifting Normative Frameworks in Historical Context," (this issue).

Behind the picture that we get in Cleland's analysis – that of rigorous evidential reasoning convincing the scientific community as a whole – are the smaller subdisciplines within paleontology that had different standards for what counted as evidence of a mass extinction. In the early days of the debates, vertebrate paleontologists were far less likely to endorse the extinction prong than their counterparts in paleobotany and invertebrate paleontology. This had to do with the relative completeness of their respective fossil records and the more easily detectable effects of the K-Pg extinction on them. Invertebrate paleontologists, for instance, knew full well that their fossil samples did indicate a cataclysmic event at the boundary that wiped out many genera of invertebrates.[36]

My interpretation of Fastovsky's argument about cultural readiness places the broader culture in a relationship with the scientific subcultures nested within it. The wider societal culture acts as an influence on the theoretical commitments of the smaller sub-disciplines within the geosciences. As we see from Sepkoski and Gould, the theoretical commitment of gradualism, a component of uniformitarianism as it was used to frame a new geologic science by James Hutton and Charles Lyell, and the norms suggestive of it, were being put under considerable strain by trace evidence *and* by the cultural experiences of the time. My point here is that, before many scientists were willing to adopt the hypothesis proposed by Alvarez et al., a change in their prior theoretical commitments was necessary. This means that – against some rationalistic tendencies – it was not *merely* evidence that played the distinguishing role between the merits of the hypotheses, but evidence in a complex relationship with a broader socio-political landscape.

Some philosophers have responded negatively to a kind of culturally motivated theory change, even in the context of the Alvarez hypothesis. They prefer to account for changes in science by appealing to the exceptional individual rationalism of the scientists themselves. That is, scientists adopt new theories based on evidence and sound reasoning alone. For instance, Keith Parsons' reason for exploring the impact hypothesis is to combat the urge by some scholars to interpret instances of theory adoption as ideologically and sociologically motivated, impugning the objectivity of science. Parsons relies heavily on *The Nemesis Affair*,[37] which is David Raup's personal recounting of his experience

36 W. Glen, "How science works in the mass-extinction debates", in W. Glen ed., *The Mass Extinction Debates: How Science Works in a Crisis* (Stanford: Stanford University Press, 1994), 51–2.

37 D.M. Raup, *The Nemesis Affair: A Story of the Death of Dinosaurs and the Ways of Science* (New York and London: W.W. Norton & Company, 1999).

engaged in the impact hypothesis debates in the 1980s. Parsons rejects the view that the Raup's experience maps onto Thomas Kuhn's revolutionary account of science,[38] developed by William Glen. Glen notices that some of the language that Raup uses to account for his experience tracks well with the psychologism of Kuhn's view. Raup mentions that he "was finding fault in what was pretty surely a classic reactionary mode"[39] as he reviewed the Alvarez et al. paper. This signaled to Glen that perhaps Raup's initial dismissal of the Alvarez et al. paper was because of "prejudice due to the theoretical blinders he wore at the time."[40]

Parsons admits that Raup "seems to be self-consciously describing himself in Kuhnian terms,"[41] however, he argues that Glen's reading of Raup is too surface level and that a deeper analysis of Raup's reasons for initially rejecting the Alvarez impact paper are a lot more rational than it first seems. Raup offered critiques in line with a scientist who has adopted standards that allow for the objective assessment of the evidence. Parsons writes, "What I *am* defending is the claim that in situations of theory choice scientists typically have at their disposal a wide array of *broadly shared* and *deeply grounded* standards, criteria, methods, techniques, data, etc., and that these are sufficient to permit fully rational decisions about theory acceptance or rejection."[42]

I see the dialogue between Parsons and Glen and their respective accounts of theory adoption and progress to be incomplete as they rely too heavily on the individual scientist and their reasoning. According to cultural readiness the scientific community is the unit of change and is not reducible to the choices or attitudes of any individual scientist. Progress is not measured by one individual's adoption of a theory, but by the community's. In the case of cultural readiness, the combination of culture and empirical conditions among the community fosters a readiness for the adoption of a theory communally.

Fastovsky's claim, then, is that the broader social context of the Cold War influenced the culture of the geosciences by making plausible the catastrophism that had been fought against for so long. Along with a change in theoretical commitments comes new standards of evidence and other changes to epistemic norms. Suddenly, the idea of a cataclysmic event was not only possible, but plausible and imaginable because of the political context and because

38 T.S. Kuhn, *The Structure of Scientific Revolutions* (Chicago: University of Chicago Press, 1962).

39 Raup, *The Nemesis Affair*, 205–6.

40 K.M. Parsons, *Drawing Out Leviathan: Dinosaurs and the Science Wars* (Bloomington and Indianapolis: Indiana University Press, 2001), 67.

41 Ibid.

42 Ibid., 78.

evidence was considered differently under a catastrophist framing than it was under a gradualist framing. The anxieties produced by the politics and events of the Cold War made this increasingly salient. Scientists, as members of the Cold War society, experienced the culture firsthand which influenced their theoretical commitments such that a hypothesis that lined up more with a catastrophist interpretation of dinosaur extinction than with gradualism become ever more enticing. Thus, cultural readiness is a thesis about the ways in which culture influences certain commitments that scientific cultures have, such that a new hypothesis will not seem so implausible. The discoveries play a role, sure, but the culture plays a significant role too in providing a contextual framing.

You may be thinking at this point that this is all an interesting story, but why should I believe that culture plays this kind of role in theory choice? Afterall, this kind of cultural influence on science has been challenged as somewhat pernicious in various domains of science.[43] In the next section I discuss the use of narrative explanations and how their distinctive features may be a vehicle for cultural readiness in the historical sciences.

3 Culture and the Relatability and Emotionality of Narratives

In this section I discuss narratives as a vehicle for cultural readiness as it relates to Fastovsky's claim that the "geosciences were culturally ready for an idea like this" in reference to the adoption of the impact hypothesis of K-Pg mass extinction. I argue that his claim makes sense when we consider that the historical sciences often rely on constructing narrative forms of explanation and adopting them requires being compelled by them. First, I offer a brief illustration of narratives in historical reconstruction. Part of the motivation for maintaining a distinction between the historical and experimental sciences is based the structure of the explanations they offer.

Work on narratives began largely in response to Carl Hempel's deductive-nomological (D-N) model of explanation that became quite popular. Philosophers of science noticed that not all scientific explanations take the D-N form because not all sciences have access to the kinds of laws that Hempel thought were constitutive of good explanations. Some philosophers, like W.B. Gallie defended the use of narratives as characteristic of some domains of inquiry.[44]

43 See, e.g., D.J. Haraway, *Primate Visions: Gender, Race, and Nature in the World of Modern Science* (London: Routledge, 1990).

44 W.B. Gallie, *Philosophy and the Historical Understanding* (New York: Schocken Books, 1964).

David Hull noticed that some sciences – particularly historical sciences like geology, paleontology, and evolutionary biology – more commonly utilized narrative forms of explanation.[45] Hull maintained a distinction between sciences which had access to laws and were suited to utilize D-N explanations (and its variants) and the historical sciences which made use of narrative explanatory forms.

According to Marc Ereshefsky & Derek Turner, narrative explanations have three core components: a central subject, a trajectory, and a directionality noticed retrospectively.[46] The central subject is the component of the explanation that unites the narrative around a single entity or event to which all other components will be connected in some causal relationship.[47] Narrative explanations also feature a trajectory and not a mere chronology. They are often represented as branching diagrams with nodes at which different paths may unfold, beginning with a singular event and arriving at many outcomes.[48] Narratives also get a directionality retrospectively. This is because, during the unfolding of some event, the beginning and end are not yet known. It is only after the event has concluded and its outcomes are known that the relevant intermediary events can be elaborated upon, suggesting one led to another. In other words, it is hard to say event *x* led to outcome *y* while you are experiencing event *x* and do not yet know if outcome *y* is coming. Only retrospectively can that be known.

Recently, philosophers have also asked questions about what makes for better or worse narrative explanations and how they contribute to successful historical work. Currie argues that we can have simple or complex narratives; which one is deemed appropriate will be determined by the explanandum.[49] Some explananda require simple narrative explanations and others require more complex narratives. Simple narratives are characterized by their lack of detail and their embeddedness into a larger model that encapsulates the event being explained. For instance, the explanation that accounts for the immense size of the fossa – an island dwelling predator – can embed the details in a larger model of island biogeography which accounts for many instances of

45 D.L. Hull, "Central Subjects and Historical Narratives", *History and Theory*, 14 (3) (1975), 253–274.

46 M., Ereshefsky & D.D. Turner, "Historicity and explanation", *Studies in History and Philosophy of Science* Part A, 80 (2020), 47–55.

47 See also, Hull, "Central Subjects and Historical Narratives."

48 See also, M.S. Morgan, "Narrative ordering and explanation", *Studies in History and Philosophy of Science* Part A, 62 (2017), 86–97.

49 A. Currie, "Narratives, mechanisms and progress in historical science", *Synthese*, 191(6) (2014), 1163–1183.

large predators in islands.[50] Complex narratives include a vast amount of detail but fail to be embedded because there is no general model in which to embed the details. Currie uses the example of the explanation of sauropod gigantism which uses a diverse set of models of different organisms (e.g., use of birds for the respiratory system and giraffes for their structural morphology) to unite the details into a plausible story. Progress in the historical sciences is thus characterized as the uniting of simple narratives into complex ones.

Ereshefsky & Turner also argue that what makes a historical narrative a distinct kind of explanation is not what makes for a good historical narrative and provide criteria for better or worse historical explanations. Specifically, they describe practices of thickening and tightening narratives as two ways of making better explanations. Thickening a narrative adds in more details, and tightening adds in more connections between those details. Both reinforce one another because, as they put it, "the thickening and tightening of a historical narrative happen 'at the same time.' In thickening the narrative, we fill in the gaps of the narrative with more events that the outcome depends on" thus tightening it as well.[51] Good historical narratives, then, incorporate lots of causal details in ways that make an event understandable to its audience.

Alvarez et al. constructed a narrative of the K-Pg extinction. The quoted passage from the 1980 paper that I provided at the beginning of section 2 details, not only the asteroid impact itself, but the cascading ecological effects that ultimately led to the mass extinctions it was proposed to explain. We may even say, to use Ereshefsky and Turner's framework, that the narrative is a good one because it is thick and tight, meaning that there are a great many details linked up in such a way that the outcome and alternative possibilities becomes clear. However, as Fastovsky points out, the larger narrative of mass extinction had very little actual trace evidence to suggest it and was composed of two horns. As we saw in section 2.2, Fastovsky himself notes this and suggests cultural readiness as one component of the hypothesis' success. The iridium was collected from only three places on earth, two of which were quite near to each other, and was used to construct the detailed causal story of the extinction at the end of the Cretaceous. But, as Cleland points out, further evidence was collected to add support to the second prong of the Alvarez hypothesis. One way of interpreting this move to collect more evidence is that the significance of the discovery, embedded in a narrative about mass extinction, was attributed by the plausibility of the narrative.

50 Ibid., 1169.
51 Ereshefsky & Turner, "Historicity and explanation", 53.

The thesis of cultural readiness implies that at one time (perhaps prior to World War II) the Alvarez hypothesis would have failed to be plausible at all and would not have been adopted by the scientific community. The thesis of cultural readiness also suggests that, had the Cold War context outlined by Sepkoski and Fastovsky not come to be (nor something similar enough to it) then the Alvarez hypothesis may be stuck in a kind of limbo. In other words, the empirical evidence must be accompanied by the right cultural setting to be thought worthy of investigation and adoption.

One thing that is missing from the discussion of narratives is whether and how a narrative is compelling or not. How do narratives like the Alvarez hypothesis get uptake in scientific communities? Afterall, a key part of the discussion of narrative explanations is their plausibility, but plausibility may fail to be compelling. Arguably, a narrative explanation can be a good one, but still fail to be adopted by the scientific community – i.e., it can have progressed from simple to complex and be thickened and tightened but fail to be adopted as the correct account. This kind of scenario seems to be at play in the Alvarez case wherein the narrative about the ancient impact causing the K-Pg mass extinction saw a trajectory through stages of implausible, then plausible but not compelling, and then compelling, not entirely based on new discoveries, but based on cultural attitudes internal and external to science.

One way to make sense of this is to ask, what else makes narratives valuable as explanations? David Velleman asks just this question and has a nice model for understanding how narrative explanation may interact with culture to produce a readiness to adopt theories. Velleman describes a good narrative as one which organizes events into an intelligible whole and the power of a narrative is in its ability to elicit an emotional response in its reader. For Velleman, what makes a narrative distinct as an explanation is that it "completes an emotional cadence in the audience."[52] Velleman also draws on Schank's[53] idea that what narratives do effectively is to help "understand events by assimilating them to familiar scenarios."[54] These two features of narratives – that they evoke familiarity and an emotional cadence – complement the thesis of cultural readiness which posits that the culture of a given time influences the adoption of historical scientific theories.

The culture can be seen as providing the audience with a relatable medium and a series of emotions or states that make the Alvarez narrative more compelling. In the case of the impact hypothesis debates, Sepkoski points to the

52 J.D. Velleman, "Narrative Explanation", *The Philosophical Review*, 112:1 (2003), 6.

53 R. Schank, *Tell Me a Story: A New Look at Real and Artificial Memory* (Scribner's, 1990).

54 Velleman, "Narrative Explanation", 18.

general anxieties of cataclysm that pervaded society in the Cold War. Take Sepkoski's description of the cultural moment and its manifestation in the scientific culture,

> This resulted in what would ultimately be described in the 1980s as the emergence of a "new catastrophism" that took hold in mainstream science, but it has clear origins in the culture and science of the decades immediately following the Second World War. As in the previous examples I have presented, this was not a straightforward matter of cause and effect; cultural anxieties did not "produce" a scientific catastrophism any more than new ideas about mass extinction generated social and political unease. Rather, the extinction imaginary of the 1950s and 1960s presents us with a tapestry in which a number of key themes are interwoven. These included, but were not limited to, the threat of sudden catastrophe (nuclear or otherwise), large-scale social unrest, increased awareness of environmental degradation, a discourse of cultural pessimism in the arts and humanities, the emergence of ecological theories that highlighted interconnectedness and fragility in ecosystems, and a scientific (and pseudoscientific) "catastrophism" around extinction.[55]

This passage provides a nice arena to talk about the ways that the culture did not produce the theory but influenced its development and adoption. Perhaps early skepticism about catastrophist hypotheses in earth's past are due to a lack of embeddedness (of the kind Currie notices) that plays a role in simple narrative explanations. It may be easier to be compelled by a narrative when it is subsumed under a more general model of the phenomena being explained. The narrative lacks a compelling unity because our imaginations cannot fathom such a narrative being true, or we fail to be compelled because of lack of emotional connection. However, when the Alvarez hypothesis was proposed, there was an available model to relate the scenario that they described, and the narrative form aided this relation. Namely, the scientific communities comprising the geosciences were embedded in the culture of the time which was experiencing an anxiety about the very same kind of thing that doomed the dinosaurs. The Cold War provided some sort of additional material to make the narrative a compelling one without the direct traces that Cleland emphasizes as playing the definitive role. It was the indirect influence of cultural context that played a significant role in the scientific community's adoption of the Alvarez hypothesis, that was not based merely on the direct traces of

55 Sepkoski, *Catastrophic Thinking*, 130.

the events in the past. Further, the narrative contained in Alvarez et al. tells the story of cataclysmic destruction; the emotional cadence of stories about nuclear annihilation and catastrophic asteroid impacts were the same.

In section 2.2, I claimed cultural readiness posits that in some scientific domains, there are scientifically apt questions, methodologies or theories that are only developed, considered, and adopted by a scientific community once some combination of empirical *and* cultural factors obtains within and without that domain. Conversely, in those same domains scientists fail to notice the significance of discoveries – and may even actively resist them – when these dual factors have not obtained. Narratives as described above blend together culture and empirical phenomena to make science culturally ready to see the significance of discoveries and adopt the theories that are constructed about them.

4 Conclusion

Before I properly conclude I would like to address a potential problem for the analysis contained above that will also serve as a sort of summary of the reasoning of the paper. One might worry that reading cultural readiness off of the Alvarez impact hypothesis is not representative of the historical sciences more generally, specifically because of the proximity to the study of the dinosaurs. Dinosaurs are a cultural phenomenon of their own because of their status as the most charismatic of the extinct animals (and perhaps even of all animals). Many of the books I cited earlier in the paper about cultural readiness and the historical sciences center their analysis on dinosaur paleontology, most notably Fastovsky himself. Why then should we think that a cultural readiness as it is explained above is a feature of historical sciences generally, and not merely of dinosaur paleontology?[56]

To answer, I offer the following thread of reasoning: If we acknowledge that the historical sciences – because of the nature of their subject of study – rely more heavily on narrative forms of explanation than their experimental counterparts, and we accept that narratives weave together cultural and empirical phenomena in the way explored in section 3 allowing for cultural readiness, then I think it safe to generalize about cultural readiness' effects are likely felt in the broad domain of the historical sciences. Dinosaur paleontology may be a smaller domain in which cultural readiness is a more operative thesis of scientific success because of its more explicit ties to culture. However, this does not

56 I thank an anonymous reviewer for offering me this challenge.

bar cultural readiness from being operative in other historical sciences. Also, the very same charisma that might make cultural readiness more operative in dinosaur paleontology, might be what is driving historians to the study of dinosaurs and culture at the expense of other historical sciences. My reasoning, at the very least, makes this objection a matter of soundness of the two antecedents, and not one of validity.

More work can be done to reveal other modes of action of the thesis of cultural readiness, which posits that broader cultural contexts play an influencing role on historical scientific development. In this paper I have introduced the notion as it appears in historical examinations of the Alvarez impact hypothesis episode and discussed how narrative explanatory forms may be a vehicle for cultural readiness. It is my contention that cultural readiness provides a fuller account of success in historical scientific domains.

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