Introduction: Scientific Knowledge of the Deep Past

Adrian Currie and Derek Turner

Penultimate Version, forthcoming in Studies in the History and Philosophy of Science A.

Historical scientists, from cosmologists to archaeologists, tackle important but difficult tasks: reconstructing the events and entities which populate the deep past, understanding their formation and development, and learning how to see our contemporary world in terms of its long history. Of late, philosophers have paid increasing attention to these epistemic challenges and the nature of such sciences (see Turner 2014). The papers collected here offer both a (by no means exhaustive) look at the variety of epistemic practices and targets found in the historical sciences and illustrate new directions in the philosophy of historical science[1]. We take 'historical scientists' to be those involved in the scientific investigation of the deep past.

Maureen O'Malley focuses on how molecular data has revolutionized phylogenetic reconstruction – and the epistemic challenges bred by that very success. Lindell Bromham uses a series of case studies to demonstrate the comparative method's (see below) power in investigations of macroevolution. Both discuss how our incapacity to experiment directly on past subjects can be mitigated—mitigated in ways highly reminiscent of experimental method. Adrian Currie identifies a connection between the use of comparative data in biology and archaeology, which underwrites a re-evaluation of evidence in the latter. Derek Turner revisits inferences about extinct lineages based on fossilized remains, using a mistaken prediction of his own to discuss the difficulty, but necessity, of making predictions about future scientific success or otherwise.

Somewhat serendipitously, each paper explores past targets at different scales. Currie looks at the archaeological challenge of interpreting rock art and the use of the 'comparative method' in paleoanthropology. Such methods involve comparing different cases, and using those comparisons to make empirical inferences. For instance, Currie discusses the use of island dwarfism in elephants and hippopotami to test theories of the same in hominids. He covers the shallow end of the deep past: a few thousand years for archaeology, a little over ten thousand for paleoanthropology. Wading deeper, Turner looks at recent efforts to extract information about dinosaur coloration from the fossil record—on a scale of tens of millions of years. Bromham discusses larger-scale questions, for instance the frequent evolution, but short lifetime, of salt-tolerant plant lineages. Such investigations not only expand our temporal scale to many millions of years, but our scope of concern: as opposed to considering the color of a few lineages, salt-tolerance is examined across the plant kingdom. Finally, O'Malley is interested in the use of molecular data to tackle questions about phylogenetic relationships among the earliest eukaryotes: the deep oceans of time, hundreds of millions of years ago (still

[1] They are a subset of the papers presented in the 2014 "Rocks, Bones & Ruins" workshop at the University of Sydney.

barely up to the cosmologist's ankles, of course). Taken as a group, the papers offer a look at historical reconstructions of varying temporal depth and varying scales.

Furthermore, the papers give us a picture of how philosophical reflection on historical science is developing. Three broad themes that stand out are:

- An increasingly nuanced appreciation of the role that underdetermination plays in the practice of historical science.
- A rejection of methodological monism. The historical sciences employ a diversity of methods, inference patterns, and models. One can appreciate methodological pluralism while at the same time noticing important cross-disciplinary patterns.
- A lack of concern—and in some cases, skepticism—about the project of demarcating historical science from other kinds of science. This goes hand in hand with a cautious attitude about making epistemic generalizations about historical science.

A broad consensus on these issues leaves plenty of room for productive disagreements about detail. Philosophers are moving away from earlier comparisons of historical and experimental science to finer-grained investigations of different modes of historical reconstruction. We provide context for the special section by expanding on those three themes.

Underdetermination & Success

At base, some hypothesis is underdetermined when we lack (perhaps in principle, or perhaps as a matter of contingent fact) sufficient empirical evidence to discriminate between it and a competitor, that is, the evidence we have (or any evidence we could have!) doesn't decide between competing hypotheses. Because historical science is often marked by degrading signals, and thus incomplete data, it provides excellent source material for enquiring after the nature of underdetermination and how scientists respond to it[2]. For instance, the fossil record is commonly understood as 'gappy': it in no way represents a trustworthy or unbiased sample of the history of life. This is because the conditions required for fossilization are highly specific, and the survival of subsequent fossils (not to mention their eventual discovery by paleontologists!) is highly fragile. Under such conditions, underdetermination is a major concern. Previously, philosophers were concerned with the overall prevalence of underdetermination in historical science, how big a problem it presented, and what it meant for the status of historical science *vis-à-vis* experimental science. This is becoming more focused on practice and more localized.

[2] Underdetermination is typically a problem for science in 'hypothesis-testing' mode, and we don't mean to suggest that trying to discriminate between explicit hypotheses is all scientists do—far from it! We do suspect however that because historical scientists often face, and are very overtly concerned with, degraded and incomplete data, much of their thinking is perhaps more geared towards hypothesis testing.

It is natural to think of "success" in the context of historical science as the overcoming of underdetermination problems[3]. New evidence arises which sharpens our picture of the past. Philosophical discussion has turned to the various means by which scientists do succeed in overcoming underdetermination, when they do. What explains that success? What works, what doesn't? And just as importantly, what are the consequences of epistemic success? Sometimes a bit of epistemic progress can have surprising methodological and theoretical repercussions.

As O'Malley tells it, molecular techniques did not merely provide deeper knowledge of the tree of life's structure, but challenged some of the assumptions that underlay previous reconstructions. Most strikingly, the idea that evolutionary processes move from simple forms to more complex ones, which played an important role in earlier reconstructions based on morphology, was questioned. Discovering that, say, the assumption that simple forms are not typically, or even often, basal is undoubtedly new knowledge. However, it is knowledge that kicks away the foundations of a large body of previous reconstructions. This tells us something about how science sometimes progresses: *n* steps forwards, *m* steps back[4]. Success sometimes brings deeper uncertainties in tow.

There has been another shift: away from thinking of underdetermination primarily as a philosophical problem, a localized version of a skeptical puzzle, and towards it being an aspect of the practice of historical science. Turner's paper, in particular, moves in this direction by highlighting some of the ways in which scientific research involves betting on future evidence. Historical scientists *qua* scientists, Turner argues, must consider underdetermination issues.

One lesson is clear: explaining how historical scientists succeed in overcoming underdetermination is going to be messy and complicated. Previous accounts—those appealing to "smoking guns" (Cleland 2002), or to consilience (Forber and Griffith 2011)—capture part of the truth, but there is no one-size-fits-all solution. The key to philosophical understanding of science is to identify patterns while also attending to localized detail. In some cases, applying new technologies or evidential sources will be the decisive step—see O'Malley's discussion of the importance of molecular evidence, or Turner on recent research on the microstructure of fossils. In other cases, success has more to do with finding creative ways of reasoning around an underdetermination problem—see Currie and Bromham on the comparative method. Although underdetermination is a more or less constant challenge, epistemic success is a messy, rarely unmixed, multifarious phenomenon.

---

[3] Success in historical science could mean more than that, including occasional predictive success—think of geologists predicting the occurrence of fossil fuel deposits—but overcoming underdetermination is nonetheless a central aim.

[4] Maureen O'Malley points out to us the inaptness of this metaphor: scientific progress is a complex, multi-dimensional beast, rather than a well-behaved, linear march.

Pluralism about method

In this section we discuss the method of historical science, that is, how should we characterize knowledge-generating practices targeting the deep past?

Let's start with some historical context. Historical science played a role in mid-20th Century philosophy of explanation. Most well-known was Hempel's (1942) problematization of historiographical explanation. For Hempel, scientific explanation essentially involves the logical deduction of our target from a set of initial conditions and—importantly—general laws of nature. The narrative quality of historical explanation never seemed to conform well to his model. Historical (or "genetic") explanations appear to invoke particular—perhaps unique—events, and do not *prima facie* appeal to laws. Hempel's response was to take historical explanations as merely partial—explanation *sketches*—as opposed to the real deal. Others, such as William Dray (1957) and W.B. Gallie (1959), disagreed, arguing that there is a different mode of explanation, and thus a different way of doing science, represented by historiography. Although this debate was construed narrowly around styles of explanation, the relative status of historical science—the legitimacy or otherwise of their method—was very much in dispute. In short, Hempel held historical science to standards that were not its own, and it did not fare well.

This rich debate revealed (or perhaps contributed to!) a tension between the historical sciences, with their apparent focus on particular events, and the law-seeking (or "nomothetic") experimental sciences. The distinction itself seems to raise questions about the epistemic status of historical science. This basic tension has been revisited (and questioned) more recently (see, for instance, Turner 2005, 2007, Tucker 1998, 2004, Jeffares 2008). It also lies in the background of efforts to clarify the nature of narrative explanation (e.g. Hull 1975, Currie 2014). Most strikingly, Carol Cleland (2002, 2011) has staunchly occupied the anti-Hempelian camp, arguing that (1) historical scientists have a distinct method, that is, a distinct way of generating knowledge, and (2) that method and more familiar 'experimental' science are equally valid. Cleland, then, is a pluralist about scientific method: there is more than one way to be a successful, legitimate scientist. She uses a relatively stark distinction between paradigmatic historical and experimental methods. Cleland treats these as ideal types, allowing that real, on-the-ground scientific work often involves some blending of the two. Nevertheless, she is committed to the idea that there is a paradigmatic way of doing historical science, and moreover, that distinctively historical science is, epistemically speaking, just as good as experimental science. That is, the results of historical investigations are as plentiful and as well confirmed as those of experimental investigations.

The relationship between different modes of scientific enquiry—experimental vs. historical for instance—still matters and is still worthy of philosophical exploration. Both

O'Malley and Bromham press on distinctions like Cleland's, emphasizing how experiment-like the historical sciences can be. For instance, while allowing that historical scientists are often restricted in their capacity to conduct traditional manipulative experiments, Bromham sketches and illustrates a variety of techniques which show that experiment-like functions can be played by the historical record. Strikingly, Bromham's reflections do not point to any single way of doing this, but rather highlight their plurality. Although comparing experimental and historical science is productive, we find that the focus of debate has shifted. There is less concern with the relative epistemic status of historical vs. experimental science and more interest in understanding the practices, successes (and failures!) of historical investigation. Moreover, what Currie (2015a) calls the 'methodological omnivory' of historical science is increasingly acknowledged. That is, to understand historical science's power, we must emphasize the plurality of ways in which they generate knowledge. There is no 'The Method' of historical science[5].

Demarcation & Philosophical Method

Simple, stark and essentialist notions of demarcation—the difference between experimental and historical science, for instance—are undermined by the acceptance of a pluralistic, 'methodologically omnivorous' picture. Moreover, since there are many ways of doing historical science, there is no one kind of science whose legitimacy is up for grabs. Just as many philosophers are skeptical about the prospects for drawing a neat boundary between science and pseudoscience (a skepticism articulated especially well by Laudan 1983), we needn't draw a neat boundary between historical and other forms of science in order to say interesting things about it.

This claim about the nature of scientific method, we think, has consequences for *philosophical* method: that is, how we (*qua* philosophers) should generate knowledge about science. You might, for instance, think that our philosophical approach should be (let's call it) 'natural historical': there are no generalizations to be drawn about the historical sciences, so philosophical explanation must always be tightly constrained and local[6]. That is to say, you might think that Bromham and Currie's points about how historical scientists overcome epistemic difficulties can only tell us about those particular cases. And that the lessons we can draw from O'Malley's and Turner's discussions are highly restricted.

However, this is not what we see. Both O'Malley and Bromham draw explicit links between experimental and comparative methodology. Currie shows that, at a rough

[5] See Currie (forthcoming) for defense of this point. This pluralist theme, and its attached indifference to demarcation, we think, reflect a broad tendency in the philosophy of science since the 1980s to abandon the notion of a 'unified' science and emphasize instead its messy, muddling-through, human aspect. Much of science's power, we think, cannot be understood without understanding such disunity (Wylie 1999, Mitchell 2002).

[6] Aspects of this discussion are related to Currie (2015b)

grain at least, comparative reasoning in biology and archaeology are instances of the same strategy. Turner turns his reflections of a failed prediction into a general discussion about predicting the outcomes of scientific investigations. Things are local, *but they're not that local*. To see what's going on, we want to close with a discussion of philosophical method as it pertains to demarcation when we're dealing with messy science. The question is this: if we give up on the project of demarcating historical from other kinds of science, what kinds of generalizations can we make about scientific investigation of the deep past?

Turner characterizes previous methods of understanding historical science as being 'from the armchair': philosophers have considered the nature and especially the quality of historical evidence without much reflection on practice[7]. By contrast, consider the approach he takes in his paper. Turner starts with a mistake: his (2007) claim that we would never know the color of dinosaurs, a prediction which failed somewhat dramatically shortly after. Turner treats his failed prediction as a data point, and asks what we can learn from it. From this perspective, he is interested in when we should bet on a *particular* instance of scientific progress. And in answering such questions the action is local. In this case, we need to know what our background theories tell us about the processes which might retain information *about colour*. But note that Turner's discussion widens as he considers different kinds of bets we could make about scientific progress. There is a tension between 'armchair' methodology and the notion that we should take case studies, or groups of case studies, as somehow playing an evidential role in supporting philosophical claims about the relevant sciences. To understand historical science, we need to be sensitive to the details, and this sometimes means we must rise from the armchair.

Sandra Mitchell (1997), in a discussion of scientific laws, makes a useful three-way distinction concerning philosophical method. The first method is 'normative', which we take as closest to the armchair. Here, philosophers decide what good science is like, in some sense prior to investigating science. For instance, we might already decide what it takes to be a law, or to be a confirming test, or to be a legitimate science. Actual practice is then judged on that basis. The second she calls 'paradigmatic'. Here, views are built on the basis of 'paradigm cases'—often detailed, longitudinal studies of instances which are taken to be particularly telling, or representative, or otherwise important. An example might be discussion of the K-Pg extinction event by Cleland (2001, 2002, 2011) and Forber & Griffith (2011). Views on the nature of historical science are built from, and tested against, this important instance of reconstruction. O'Malley's paper provides a superb instance of paradigmatic method: by picking out a particular problem case in phylogenetics, the emergence of eukaryotes, she illuminates interesting properties of historical science.

---

[7] It is worth sounding a note of caution here: today's philosophers of science are often quick to accuse past philosophers of being insufficiently informed about scientific practice. However, they were surely not so scientifically naïve. Rather, we suspect that both the norms of how philosophy was presented, and the kinds of answers philosophers sought, were somewhat different.

Mitchell's third method, and the one most obviously on display in the special section, she calls 'pragmatic' (although the term shouldn't be taken too seriously: we think 'functional' might be more appropriate). Here, philosophers investigate patterns of reasoning across scientific practice and analyze concepts based on the role which they play. Mitchell, for instance, takes the primary function of laws to be the provision of expectations across cases. They thus provide the modal oomph required to ground prediction and explanation. She takes herself to learn this by examining scientific work in play. On this approach, the view from what Bromham calls 'in the trenches' becomes increasingly important.

Although conceptual analysis is not their focus, the papers herein take an allied approach to understanding epistemic issues. Historical scientists do a lot of things, they appeal to different methods in response to different epistemic situations. Part of our job in understanding such science is to examine the relationship between methods and situations: are there common features which line up with particular successes? Are there common methodological responses in the face of similar challenges? Currie's discussion of comparative data draws together some quite abstract similarities between archaeology and biology. These similarities provide a framework allowing more specific, local claims to be developed. Accepting a disunified story about the historical sciences does not preclude making normative or systematic claims.

Returning from philosophical method to scientific practice, we might wonder whether there is anything distinctive about the historical approach. Bromham's discussion, it seems to us, is readily unified with work in population-level and social sciences such as ecology, economics and psychology (which are surely not historical sciences!). There too, we see an emphasis on both methodological pluralism and the experiment-like roles which non-manipulative studies can play. We find similar emphasis both in O'Malley's paper, where she highlights the use of models in historical science (see also Turner 2009) and Currie's paper, which is in the business of drawing connections between different inference patterns in different scientific contexts.

There is demarcation of a sort here; however, it departs in two crucial ways from more traditional discussion. First, the approach is 'bottom up': methodological and epistemic divisions emerge from examination of scientific work. Second, we do not expect the relevant boundaries to track anything like our prior expectations. The unification we find could plausibly be very patchy. Some aspects of cosmology might be, philosophically speaking, more similar to some aspects of archaeology than to 'next door' parts of cosmology. The challenge is to work out which aspects of the epistemic situation faced by scientists make a difference to their practice and success, and to see what lessons we can draw.

Reflection on the historical sciences pays philosophical dividends. The issues which arise motivate work, not only on the nature of science and our epistemic access to the deep

past, but on philosophical method, the necessity and difficulty of predicting scientific success or failure, the nature of evidence—underdetermination, for example—as well as the relationship between different scientific tools, such as experiments, models and more 'passive' observations.

Acknowledgements:

Thanks to Kirsten Walsh, Maureen O'Malley, Lindell Bromham and Anjan Chakravartty for useful feedback on earlier drafts. The papers in the special section were presented at the 2014 'Rocks, Bones & Ruins' workshop at Sydney University, funded by the Sydney Centre for the Foundation of Science and the Tom Austen Brown Endowment.

Bibliography

Bromham, L. Testing Hypotheses in Macroevolution. Studies in History & Philosophy of Science A.

Cleland, C. (2001) Historical science, experimental science, and the scientific method. Geology; November 2001; v. 29; no. 11; p. 987–990.

Cleland, C. (2002), "Methodological and epistemic differences between historical and experimental science," *Philosophy of Science* 69: 474-496.

Cleland, C. (2011), "Prediction and explanation in historical natural science," *British Journal for the Philosophy of Science* 62(3): 551-582.

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

Currie, A. (2015a), "Marsupial Lions and Methodological Omnivory," *Biology and Philosophy* 30: 187-209.

Currie, A. (2015b), "Philosophy of science and the curse of the case study" In: *The Palgrave Handbook of Philosophical Method* (Daly, ed). Palgrave-Macmillan, New York. 553-572

Currie, A (forthcoming). "Hot Blooded Gluttons: Dependency, Coherency & Method in the Historical Sciences. British Journal for the Philosophy of Science.

Currie, A. Ethnographic Analogy, the Comparative Method & Archaeological Special Pleading. *Studies in History & Philosophy of Science A*.

Dray, W.H. (1957), *Laws and explanation in history*. Oxford: Oxford University Press.

Forber, P. and E. Griffith (2011), “Historical reconstruction: Gaining access to the deep past,” *Philosophy and Theory in Biology* 3. Available online at philosophyandtheoryinbiology.org, last accessed 2 September 2015.

Gallie, W.B. (1959) “Explanations in history and the genetic sciences,” in P. Gardiner (ed.) *Theories of History*. Glencoe, IL: The Free Press, pp. 386-402.

Hempel, C.G. (1942), “The function of general laws in history,” *Journal of Philosophy* 39(2): 35-48.

Hull, D. (1975), “Central subjects and historical narratives,” *History and Theory* 15(3): 253-274.

Jeffares, B. (2008). Testing times: Regularities in the historical sciences. Studies in History and Philosophy of Science Part C 39 (4):469-475.

Laudan, L. (1983), “The demise of the demarcation problem,” in R.S. Cohen ed. *Physics, philosophy and psychoanalysis: Essays in honor of Adolf Grünbaum*. Dordrecht Riedel, pp. 111-127

Mitchell, S. (1997), “Pragmatic Laws,” *Philosophy of Science* 64(4): S468-S479.

Mitchell, S. (2002), “Integrative pluralism,” *Biology and Philosophy* 17(1): 55-70.

O’Malley, M. Histories of Molecules: Reconciling the Past. *Studies in History & Philosophy of Science A*.

Tucker, A. (1998), “Unique events: The underdetermination of explanation,” *Erkenntnis* 48(1): 61-83.

Tucker, A. (2004), *Our Knowledge of the Past*. Cambridge: Cambridge University Press.

Turner, D. (2005), “Local underdetermination in historical science,” *Philosophy of Science* 72: 209-230.

Turner, D. (2007), *Making Prehistory: Historical Science and the Scientific Realism Debate.* Cambridge: Cambridge University Press.

Turner, D. (2009) Beyond Detective Work: Empirical Testing in Paleontology. In: *The Paleobiological Revolution: Essays on the Growth of Modern Paleontology,* Sepkoski & Ruse (eds.) University of Chicago Press.

Turner, D. (2014). Philosophical Issues in Recent Paleontology. *Philosophy Compass*, 9(7), 494-505.

Turner, D. Another Look at the Color of Dinosaurs. *Studies in History & Philosophy of Science A*.

Wylie A (1999) Rethinking unity as a ‘‘working hypothesis’’ for philosophy of science: how archaeologists exploit the disunities of science. *Perspectives on Science* 7(3):293–317