ROCK, BONE, AND RUIN

ADRIAN CURRIE

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Rock, Bone, and Ruin: An Optimist's Guide to the Historical Sciences
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The historical sciences appear to present a challenge for mainstream views about the epistemology of science, which have largely been developed with the physical sciences in mind. While debates over realism about microphysical entities still continue, what are we to make of the epistemic situation of historical scientists? The objects of their investigation, namely, historical entities and processes, are, like microphysical entities, not directly observable, but unlike microphysical entities, they are unmanipulable. As Derek Turner ([2007]) has argued, this appears to put historical scientists in a worse situation, epistemically, than microphysicists. But most philosophers (I presume) would not want to be anti-realists about the entities and processes of the past. What, then, is the proper attitude we should have towards the historical sciences? And might thinking about this question provide us with insights that we could direct back towards more traditional debates about the epistemology of science?

Readers who are interested in the latter question will find much food for thought in Adrian Currie's new book *Rock, Bone, and Ruin: An Optimist's Guide to the Historical Sciences*. In this book, Currie urges us to step outside the narrow confines of the realism debate, and to become optimists about the historical sciences. The problem with the debate, according to Currie, is that it focuses on a 'fairly narrow range of epistemic goods' (p. 14)—that is to say, on whether scientific theories are true, or approximately true, or whether they successfully refer. But the epistemic goods that are produced by science at its best include, in addition, 'good explanations, adequate representations, precise predictions, new technologies, successful techniques, telling interventions, effective cures, and so forth'. Optimism is supposed to go beyond realism by encompassing this wider range of epistemic goods that are now put on the table. It is evaluated by examining an 'epistemic situation', and the 'epistemic resources' available in that situation (p. 15). You are an optimist if, given a particular epistemic situation and the epistemic resources available, you believe that a scientist in that situation will 'succeed in generating a wide range of epistemic goods'. The aim of the book is to argue that we should be optimists about the historical sciences—that is, given the kinds of epistemic situations in which historical scientists find themselves, and given the epistemic resources they have, we have good reason to think that they will successfully produce a wide range of epistemic goods. Of course, a lot rides on how Currie answers the following questions: What are the epistemic situations in which historical scientists find themselves? What epistemic resources are available to them, and how do those resources allow them to deal with their epistemic situations? What kinds of epistemic goods are there, and how is the probability of successfully producing epistemic goods to be determined?

The first few chapters of the book are devoted to providing an account of the epistemic situations that historical scientists face. In Chapters 3 through 5, Currie examines earlier work in the epistemology of the historical sciences, particularly that of Carol Cleland and Derek Turner. Cleland has argued, on metaphysical grounds, that events in the deep past are overdetermined by traces in the present, while Turner has argued to the contrary that events in the deep past are underdetermined because traces tend to degrade over time. Although the views of Cleland and Turner about trace evidence appear to be *prima facie* incompatible, Currie presents a neat way of understanding trace evidence, called the 'ripple model of evidence', which in a sense unifies their views. The ripple model of evidence allows Currie to characterize what he calls 'unlucky circumstances'—cases where there is very little trace evidence or it is of poor quality. He then identifies three reasons that one might be pessimistic about historical investigations under such unlucky circumstances: (1) our available evidence about the past is limited to traces, (2) we are unlikely to recover further traces, and (3) historical scientists cannot manufacture evidence in the way that experimental scientists can in the laboratory (p. 135). Currie's argument for optimism proceeds by arguing that these three worries are unfounded, and therefore pessimism is unwarranted, even in unlucky circumstances.

In the middle sections of the book, Currie explores various epistemic resources that historical scientists use to overcome the three given reasons for pessimism. Against (1), the worry that the evidence available to historical scientists is limited to traces, Currie describes resources that provide historical scientists with non-trace evidence. One source of non-trace evidence is coherence. Evidence can come not only from the relation between past events and the traces that have been left by them, but also from inter-relationships between events, objects, and processes in the past. For example, because there is taken to be a causal dependency between the Cambrian explosion and an earlier period during which it is hypothesized that glaciers covered the earth all the way to the tropics (the so-called Snowball Earth hypothesis), evidence about Snowball Earth can indirectly be evidence about the Cambrian explosion. Another source of non-trace evidence is the use of analogues. For example, Currie describes the reconstruction of the bite mechanism and killing style of a large saber-toothed marsupial-like mammal by comparison with saber-toothed animals in other lineages. Evidence in this case comes not just from traces of this particular mammal, but through examining features common to all members of a general category like 'saber-toothed carnivores'. These examples show that historical scientists can draw on a much wider pool of evidence than one might have expected.

Against (2), the worry that we are unlikely to recover further traces, Currie offers what he calls 'investigative scaffolding'. For example, the Snowball Earth hypothesis as originally conceived in the 1990s was compatible with various degrees of glaciation of the earth, but later hypotheses differentiate between scenarios where the earth's oceans are completely covered in ice (a snowball) and scenarios where there are significant areas of open ocean left (a slushball). The original Snowball Earth hypothesis was, according to Currie, a scaffold—a coarser-grained hypothesis that was eventually replaced by finer-grained hypotheses. Once a scaffold is reached, it is taken to be relatively well supported, and then further evidence is sought to differentiate between finer-grained hypotheses. This forces a re-evaluation of the evidence available. For this reason, Currie takes scaffolding to make it difficult for scientists to predict what evidence will be available in the future, once a scaffold has been reached. This opacity with regard to future evidence is then taken to mitigate the worry that it is unlikely that we would recover further traces in the future, for Currie believes that sources of evidence will generally increase rather than decrease once a scaffold has been reached.

Against (3), the worry that historical scientists cannot manufacture evidence the way experimental scientists can in the laboratory, Currie describes the use of models and simulations for the production of evidence about the past. For example, Currie describes a computer simulation through which a hypothesis about differences in the development of Paleozoic echinoderms (sea urchin-like creatures) and modern echinoderms was tested. Currie claims that such computer simulations and models can compensate for cases where there are a lot of gaps in the trace evidence or where the evidence is biased.

Let me go back to Currie's argumentative strategy of showing that the three reasons for pessimism are unwarranted, even under unlucky circumstances. For each of those reasons, Currie provides examples of epistemic resources that can be used to overcome them. And because such resources are available, pessimism is not warranted and we should be optimists about the historical sciences. I find this argumentative strategy rather unpersuasive. Even if such additional epistemic resources can be found, there would still be no reason to be an optimist if these resources cannot provide evidence strong enough to make up for the poor quality of the trace evidence. But Currie does not provide much in the way of evaluation of the strength of the evidence that can be obtained using epistemic resources such as coherence, analogues, and simulations. Currie's discussion of coherence in Chapter 6, for example, is directed only towards the claim that coherence can provide evidence that goes beyond mere trace evidence, not the strength of the evidence that comes from coherence. Similarly, Currie discusses the epistemic status of models and simulations in Chapter 9, but only argues for the extremely modest conclusion that models and simulations can generate evidence.

There are reasons why Currie takes the approach that he does. He is deeply suspicious of abstract, general theories about scientific methodology on which one might draw to provide an analysis of the strength of the evidence. Appealing to John Norton's ([2003]) material theory of induction, and with regard to the historical sciences at least, Currie claims that 'epistemic warrant is local' and 'there is very little we can say that is both general, abstract, and explanatory' (p. 21). Even so, one could still give an analysis of the strength of evidence in a particular epistemic situation without appealing to a formal theory of induction of the type that Norton rejects.

Something similar can be said about Currie's views about investigative scaffolding. If I were a pessimist, I don't see how the mere possibility (even the high probability) that more evidence might become available after a scaffold is reached would convince me to change my mind. An account needs to be given of the likely strength of the evidence that will become available. Related to the idea of scaffolding is what Currie calls 'empirically grounded speculation'. Because some hypotheses play the role of scaffolding, from which new evidence can be gathered, Currie believes that the acceptance of hypotheses should depend not just on how strongly confirmed they are, but on their potential fruitfulness—on the possibility that such hypotheses can generate 'epistemic or empirical goods that increase epistemic traction' (p. 289). Now, similar ideas can be found in other areas of the philosophy of science, such as the notion of a 'working hypothesis', which can be found in the work of George Smith. Working hypotheses can give rise to research programmes in which extraordinarily strong evidence can emerge, as Smith has shown in the case of gravity research after Newton. On the other hand, Smith ([2002]) raises the worry that working hypotheses could, in some cases, lead to 'garden path' research—research predicated on a working hypothesis that eventually turns out to be false. In the worst-case scenario, decades of research could potentially go to waste. Currie does briefly mention the risks involved when one erects a scaffold, but his general recommendation is that 'when circumstances are unlucky, historical science should be wild, messy, and creative' (p. 290). I think a lot more needs to be said about the quality of the epistemic traction that can be obtained in historical science, and just what safeguards there might be against potentially bad outcomes, before such an optimistic prescription can be made.

With all of that said, Currie's new way of framing the epistemology of the historical sciences in terms of epistemic situations, resources, and goods is potentially fruitful. In the last chapter of the book, Currie asks us to imagine what philosophy of science would have looked like if it had taken the historical sciences, instead of physics, as its primary example. There is nothing that looks like Theory-with-a-capital-T in the historical sciences. Instead, there is a diversity of methods, models, and bits of theory that historical scientists use opportunistically to generate knowledge about the past. Currie refers to this opportunistic streak in historical scientists as 'methodological omnivory'. This fragmented view of the epistemology of the historical sciences can, I think, be extended to other scientific fields. One might even make the case that the physical sciences are actually more fragmented than is usually thought (for example, see Azzouni [2000]). If this picture is right, then perhaps more attention needs to be paid by philosophers towards understanding just what kinds of epistemic resources there are and how they work. Another promising avenue of investigation would be historical investigations of how epistemic goods (such as mathematical and statistical methods, measurement techniques, models, and so on) are generated and then get re-applied in new domains. Currie has, in this book, pointed us towards a promising way forward.

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