Review

Adrian Currie, *Rock, Bone and Ruin: An Optimist's Guide to the Historical Sciences* (Cambridge, MA: MIT Press, 2018), ix + 372 pp. ISBN: 9-780-26203-726-6. \$35 (hbk).

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The poor historical sciences! That refuse pile of time, where evidence is woefully incomplete and mostly unrecoverable, artifacts degenerate not only over the ages but before our eyes, and “the present is the key to the past” means no more than “the past is the key to the present” does. Where methods are *ad hoc*, subjectively applied, and often slipshod, and explanations are conditional and apologetic. Is this really science, or just “stamp collecting” (as physicist Luis Alvarez once referred to palaeontology)?¹

Fortunately we have Adrian Currie, riding in like a White Knight to save the respectability of the historical sciences with logic, rhetoric, strong examples, and engaging writing. In this wonderful book he summarizes much of his classic and current work, taking the view that things are not really as bad as they seem; in fact, they're getting better all the time. The reasons are that as we discover more evidence, we find that new examples show the same patterns as the old ones, establishing patterns and reinforcing our inferences. We develop better methods to analyse our evidence, some of which are logical (cladistics), some technological (CT-scanning, ancient DNA), and some conceptual (models and simulations). But how effective is all this, really, for solving problems in the historical sciences?

Currie sets up his long argument in three ways. First, he engages the views of two colleagues, Derek Turner and Carol Cleland, essentially as interlocutors. Cleland is made to represent the position that the historical sciences are *overdetermined* by knowledge of the present: essentially that “the present is the key to the past” to the extent that the past has such an incomplete record that it can provide few new insights. Turner represents the view that the past is *underdetermined*: that is, there were many possibilities that are not encompassed in the living world. For Currie, each position

is both right and wrong, and like Goldilocks's Baby Bear, he is determined to find the position that is *just right*. To negotiate these disparate views requires not only cleverness but a familiarity with both the philosophy and the science.

And that brings us to the second way that Currie establishes his long argument: with a thorough grounding in congenially loose analytics. This book will not be an easy read, as accessibly as it is written, for typical cave-man palaeobiologists like me. This world frightens and confuses me. It is filled with terminology. But Currie does not use terms for their own sake; instead, he deploys them in the service of contrasting concepts in the real world. It is a tough slog for scientists anyway, but there is the third line of argument, which makes it easier.

And that line is the actual science. Currie uses examples from palaeontology, evolutionary biology, geology, and archaeology to lay out the scope that his arguments cover. He relies on a very few of these: the large size, long necks, and metabolic rates of sauropod dinosaurs; Snowball Earth; and a giant extinct platypus relative known only from a molar, among others. He develops each example for several purposes. This is an advantage for readers who are not primarily scientists, because they can take a few papers deep rather than having to wade through dozens of examples cherry-picked for individual purposes. And it is the comfort zone for scientists, who can see how work in their field is rooted in philosophical concepts and arguments.

It is an unfortunate truism that in practice most scientists have little use for philosophy. To paraphrase Charles Peirce, everyone thinks he does not need a course in logic, because he believes that he reasons perfectly well; but he does not extend this view to his neighbours. For many historical scientists, statistical analysis takes the place of logical reasoning, and causality and correlation are often conflated if not equated. This book made me long for a "Historical Sciences for Dummies" book. Currie would be the ideal author.

And now comes the "however..." part. Perhaps the thorniest issue Currie treats is whether the historical sciences can "manufacture evidence", as experimental sciences do in devising experiments, running them, and generating data. I agree that we manufacture evidence by making more discoveries, by analysing what we have with new and different techniques, and by looking for patterns in our data. But I am conflicted about whether analogies, models, and simulations are really "evidence". They are not data, not new observations. In their different ways they make proposals about the evidence at hand, but not equally.

For example, a model (for our purposes, a quantitative treatment of variables associated with the problem at hand) can be inadequate, as Currie

points out, if there are too many variables for the technology to describe it accurately. Moreover, as Manfred Eigen once said, a hypothesis has two possibilities: it can be right or wrong; whereas a model has a third possibility: it can be right but irrelevant. And this is also a problem with simulations, which often are models that use real data, such as bootstrap treatments of taxonomic diversity data in the fossil record that ask whether, if you simply scrambled the time bins, you would get the same patterns in the data. The answer to this is probabilistic, but history is not always probable.

The sharpest thorn is the use of analogy. Currie treats it in two different ways. The more innocuous one is phylogenetic, where he does not really mean analogy but homology. That is, you attribute a feature to an extinct animal because a living relative has it. This is perilous. For example, Pian *et al.* (2013) described a single molar from the Miocene of Australia that they attributed to an extinct platypus, notable because it was a third longer than any other platypus, living or extinct (and several extinct ones were larger than our living one). Currie approaches this with a kind of palaeontological uniformitarianism: it was a platypus, so we could reasonably infer that it rummaged in the sediment in search of invertebrates and other prey, including small vertebrates. That is (misplaced) homology, but here is the real analogy. The diet was hypothesized by Pian *et al.* (2013), but they based it on different evidence: the giant platypus had relatively conservative tribosphenic molars, which are generally adapted for an omnivorous diet (slicing and dicing); and the molars were worn at the tips, which suggested a durophagous diet (hard stuff). These observations related not to the (homologous) identity as a platypus, but to the (ecologically analogous) diet observed in other mammals with this kind of tooth. But more to the point, living platypus do not have molars like this; they were lost millions of years ago. So, if we presume that tooth form determines diet (and there are many exceptions), we cannot reason that this giant Miocene form ate like living platypus. The analogy is overextended, but it is more a homology than analogy.

The other, and more common, sense of analogy is occasioned when an extinct animal bears some resemblance to a living animal (or several) and we are tempted to infer functions and behaviours based on similarity of features. Can you say *pitfalls*? Please indulge me: I work on pterosaurs. Georges Cuvier described the first known specimen in 1809 – and he was the first person to interpret their anatomy correctly – and in his *Ossements Fossiles* (1812) he said that, of all the extinct quadrupeds in his comprehensive treatise, these were the strangest; and if we were to see them alive today, we would not know what to make of them. Yet other scientists have noted their resemblances to birds, bats, lizards, and even fish (Padian 1987; Taquet and

Padian 2004). They remain the most misunderstood of extinct archosaurs, even though they are the closest major relative to dinosaurs. (I'll return to them shortly).

This brings us to sauropods, those magnificent dinosaurs, the largest creatures ever to walk the Earth. They are very tall, because they have long necks. So do giraffes, and so any number of analogies suggest themselves as hypotheses. One is that they were able to feed on higher vegetation, as giraffes do, simply because their necks are long. But the vertebrae of some sauropods suggest that they did not raise them terribly high (others did). They might have swung them side to side, like great construction cranes, to hoover up ground plants efficiently. Another is that, because male giraffes use their long necks to bump other males in competition for females, the long necks of sauropods may have evolved in the context of mate competition. Because there is no way to test this proposal in sauropods, the hypothesis has no meaning (in Peirce's sense). Here again, phylogenetic comparisons are useful. If we had a living medium-necked giraffe (longer than the okapi's), we could see if it used the neck for advantageous feeding or for attracting mates (or both). Sadly, we do not. (Apparently the living world is also incomplete).

The bottom line is that analogies (in the sense of sauropods and giraffes) are not evidence of anything. They are teaching tools, but pernicious ones: they can be persuasive rhetorically without the homologous basis necessary to propose a testable hypothesis.

What is the way out of this? Can analogies be useful? Yes, but Currie and other philosophers have not articulated how. Here is an example. Pterosaurs such as the giant *Quetzalcoatlus*, with a wing span of nearly 40 feet (12 meters), share superficial similarities with various animals but were different in important respects. In functions such as walking, launching, and flying, they have been compared to various living animals, including birds, bats, crocodiles, and gliding squirrels. But they do not take well to analogies: when you put their bones together and make them work in flying or walking or launching, you find that they cannot do a lot of things that palaeobiologists have speculated they could do, based on living animals.

There are two ways to test analogies. One way is by actually putting the bones together. Remember that these are naked bones: they lack the cartilage, muscles, tendons, ligaments, and other soft tissues that would constrain them further (Hutson and Hutson 2013). Even so, there are movements that just cannot happen, and for two reasons: there are bones in the way, or to make this motion would require a dislocation of the joint, obvious even to the least professional (Manafzadeh and Padian 2018).

That is the elimination part. But what about the possibility part? Anyone can propose that sauropods used their necks to whack rivals, and no one can falsify the idea. But for this proposition to be a testable hypothesis, there needs to be *diagnostic criteria* by which we would understand that a given structure performed a given function. For example, to function as a wing, an airfoil has to be able to execute a flight stroke that generates a vortex wake capable of moving an animal forward in the air at a given level of effectiveness (the *definition*: Padian 1985). To do this, an animal has to have an airfoil with the integrity necessary for flight, a stroke capable of generating thrust, and a neurophysiological system capable of generating that stroke (the *diagnosis*). These are the *sine qua non* of the analysis.

If we ask what there is about the anatomy of sauropod neck vertebrae – highly derived, unusual features that they also share with giraffes – that diagnoses how the vertebrae could be used to swat other giraffes away, the answer is: none. End of story. We cannot falsify the idea, but it fails the test.

This is where most analogies fail. They are based on superficial similarities instead of on *diagnostic criteria* that would cement the correlation between structure and function.

Philosophers of the historical sciences (and we scientists should be everlastingly grateful to them) get most of it right, and Currie's book is an example of what graduate students (and their mentors) should read in order to understand the logical basis of their science. But sadly, I suspect it will be an uphill climb to make scientists feel that an immersion in the philosophy of the historical sciences will be worth their investment. In an ideal world, this book would be required reading for every graduate lab group in the historical sciences. It is important to thank Adrian Currie and his colleagues for keeping the flame alive.

Note

1. See the interview in "The Bricks of Scholarship", *The New York Times*, 21 January 1988, p. 26.

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