Review of *Evidential Reasoning in Archaeology*

Adrian Currie*

Robert Chapman and Alison Wylie, *Evidential Reasoning in Archaeology*. London: Bloomsbury Academic (2016), 264 pp., $82.00 (cloth).

> And they always find in archaeology, *a series of small walls . . .* "we've found a series of small walls, we're very excited, we think this proves they had walls in olden days. They were very small, a series of small-walled people." And someone comes along, very learned with glasses and says "of course the king and queen entertained here—1500 courtiers—and there were soldiers, 20,000 soldiers in this room, and elephants dancing hopscotch over there, mad fiddler in this room, playing the banjo, viaducts and aqueducts etc. . . ." And you're just watching thinking *you're making this up, mate—you just point at a series of small walls and say* "and Tutenkhamen played the banjo in there." Don't know if it's true! (Eddie Izzard, *Glorious*)

Archaeology is hard. The remains of human cultural pasts—pottery, middens, coins, burials, 'small walls' (the leftover stuff)—potentially underwrite vivid narratives about the lives of peoples and societies long departed. But between a few scraps of bone and cloth, faded knickknacks, the subtle traces of long-departed wooden structures, and a complex, flourishing community replete with ideologies, religions, and social structures there lies a considerable inferential gap.

How archaeologists navigate this gap is the subject of Robert Chapman and Alison Wylie's new book, *Evidential Reasoning in Archaeology*. It is

a short read of just over 200 pages spanning four chapters (plus introduction and conclusion), but this brevity is no sign of a lack of subtlety, richness, or importance: Chapman and Wylie provide a sophisticated and philosophically informed account of the nature of archaeology. I will explain their central argument, provide a quick tour, and then switch to a more critical discussion.

The book is framed by the 'paradox of material evidence'. The historical record is impoverished and thus potentially egregiously theory laden. That is, our preconceived ideas about the past could determine our interpretation of material remains, leading to what Bell (2015) calls "xeroxing": our pre-understandings force interpretations of traces, which circularly support those pre-understandings. Nonetheless, Chapman and Wylie impress upon us "how stubbornly recalcitrant these data can be, no matter how entrenched their assumed meaning" (5). So, material evidence is ambiguous; nonetheless, it can be transformative.

A secondary framing concerns the 'crisis debates' which mark archaeology's theoretical history: on Chapman and Wylie's account (a history Wylie has told before; e.g., Wylie 1985) archaeologists have tended to bounce between two extremes. At one extreme is a hard-nosed superempiricism that attempts to maintain archaeology's 'objectivity' by restricting it to the mere cataloging of material remains or making the most conservative, careful inferences only. At the other extreme is a kind of freewheeling subjectivist constructivism in which interpretation reigns supreme, with few restrictions (empirical or otherwise) to constrain it. These crisis debates are related to the paradox of material evidence: on one horn of a dilemma, we demand solid foundations for archaeological evidence and thus are conservative; on the other horn, we deny solid foundations, thus opening the door to unconstrained interpretive speculation.

How is the paradox of material evidence resolved? Chapman and Wylie's diagnosis rejects the assumption that archaeological reasoning requires fundamental grounds. Instead, they argue that the very success of archaeology depends on adopting, and revising, 'provisional' grounds. This allows progress in the face of limited material evidence, while letting that evidence be transformative.

Wylie's colleague at Durham University, Nancy Cartwright, has some advice for philosophical argumentation that I think neatly captures what Chapman and Wylie think about archaeological reasoning: "from the very start of my career as an undergraduate at Pittsburgh I have opposed: Tall, skinny arguments that are sparse and tidy. In favor of arguments that are: Short, stocky, and tangled" (Cartwright 2015, 101). Tall, skinny arguments begin from some fundamental ground and move carefully and deductively to their conclusions. They are, in some sense, self-warranting. Such arguments are not robust—knock out a premise and the whole edifice collapses (and the

more premises, the more liable to collapse). Arguments that are short, stocky, and tangled, by contrast, draw on a variety of evidence with differing grounds, which are interwoven and interdependent. This makes for robustness: even if some aspects fall by the wayside, the argument marches on. These are not self-warranting, lacking a single foundation, and what foundations they do have are provisional, open to continual probing. Regardless of whether Cartwright has it right for philosophers (I reckon she has), according to Chapman and Wylie, archaeology has that same character.

"Neither these data nor the evidential claims based on them constitute a self-warranting empirical foundation, and yet they can powerfully challenge and constrain the reconstructive and explanatory claims we project onto the cultured past" (6). Foundations are provisional but nonetheless underwrite robust arguments because of their diversity and their interrelations.

In a sense, this answer to the paradox of material evidence is straightforward and not new: archaeologists uncover the past using 'variety of evidence' reasoning and by continually critiquing, developing, and improving their techniques for generating those lines of evidence. Indeed, to some extent the book summarizes and builds on many of the ideas in Chapman and Wylie's recent edited collection (2015), ideas that Wylie has explored throughout her career. But this straightforward answer belies the rich, complex reasoning and infrastructure of actual archaeology—and it is this that the book captures originally and convincingly. With vivid examples from both contemporary and historical archaeological practice, Chapman and Wylie analyze the successes (and failures) of the science, and from it they draw a series of lessons about what good archaeology looks like.

The central idea is that archaeology's success involves the construction, and ongoing development, of various kinds of scaffolds. The notion of a 'scaffold' is becoming common parlance in philosophy of science (see, e.g., Caporael, Griesemer, and Wimsatt 2013). What is a scaffold in this context? Well, scientific theories and evidence do not stand alone but rely on various kinds of institutional, material, and epistemic supports. Indeed, such supports are often contingent, and provisional, and hence subject to ongoing reevaluation and tweaking. Important for Chapman and Wylie are scaffolds as 'provisional foundations'—such scaffolds license the interpretation of a set of material remains as serious evidence for a claim about our cultural past. But there are other scaffolds too. For example, Chapman and Wylie emphasize the various techniques, best practices, and 'ways-of-seeing' that scaffold investigation (as opposed to, say, knowledge) and hence are necessary for effective field work, the preparation and analysis of material remains, pedagogy, and other features necessary for archaeology to succeed. Finally, community structures scaffold the investigation in that they facilitate the communication, trading, and 'data-journeys' (Leonelli 2016) necessary for such collaborative, non-self-warranting work: it takes a village to reconstruct a village's past.

Allow me to pause briefly and mention some disquiet with 'scaffolding' as an analogy (inspired by Walsh 2016). An architectural scaffold is not such simply because it supports but because it is not a proper part of the completed structure—often, a scaffold is removed at or before completion. However, for Chapman and Wylie it is not obvious that there is a completed product for archaeology, or at least their strong emphasis on the dynamism of archaeological theorizing and its relationship with material evidence suggests there is not. Moreover, it is not clear that archaeological scaffolds are removable, or separate to the 'building': although the scaffolds might be removed or at least altered, it is not part of their function to be so.

Regardless, the provisioning of local warrants—provisional foundations—and their continual dialogue with material evidence demonstrates how "the 'ladening' of archaeological claims with 'theory'. . . need not be viciously circular, a matter of projecting just what you want to see onto an obligingly accommodating screen of enigmatic, empirical data" (206).

The book's structure hammers these lessons home via analyses of four aspects of archaeological practice. The first chapter provides a historical overview of the crisis debates I mentioned above. Chapman and Wylie argue that such debates matter for day-to-day archaeology. However, when we consider what archaeologists actually do, I think it is striking how small the effects of these extreme quasi-philosophical views seem to be. Chapman and Wylie's book explains this: the combination of stubborn material evidence, and the ongoing reevaluating, tweaking, and critiquing of provisional foundations, leads archaeological practice to be relatively unscathed by the highfalutin methodological theorizing they sometimes engage in. That is, crisis debates have little effect on archaeological practice, and this is because they focus on the kinds of foundational issues that are not where the epistemic action is.

The second chapter considers fieldwork. Chapman and Wylie provide a clear articulation of the centrality of fieldwork to archaeology and its theoretical nature. Their discussion is important simply for covering this ground (philosophers need to think more about fieldwork). Again, the approach is historical: we are treated to a beautiful, complex discussion of the scaffolding involved in developing the techniques and standardizations required for archaeological fieldwork to, well, work. The importance and dangers of such scaffolds are highlighted, the central challenge being to keep them "accountable to the goals of inquiry and to an evolving array of conceptual and empirical constraints" (86). Given the importance of fieldwork in archaeological training, I suspect much more remains to be said about how these shared experiences structure not just how archaeologist learn "to 'see' material traces as archaeological data" (84) but how archaeologists think: theorize, explain, and interpret.

The third chapter covers legacy data: archaeologists frequently need to reexamine material evidence and data collected many years ago by workers

operating under very different conditions and with different ideas about what good data collection and evidence look like. Such practices make particularly vivid the kinds of reflective 'source-criticism' (the "dynamic process of . . . refining provisional foundations"; 10) that underwrites archaeological success. It is thus a clear illustration of Chapman and Wylie's central point about the nonfoundational nature of archaeological knowledge.

Chapter 4 examines the archaeological practice of incorporating 'external' resources from other disciplines—most obviously the use of carbon 14 dating. Here, it is argued that archaeologists do not simply adopt tools and techniques from other disciplines. Rather, there is a long, hard journey getting resources from other domains to play nice with the highly idiosyncratic, localized—difficult—stuff that is archaeological evidence. This difficulty is reflected in the historical pattern Chapman and Wylie highlight and illustrate using the case of ^{14}C. First, as the new technique is bought to archaeology, we see a period of optimism coupled with a kind of imperialism: ^{14}C dating was hailed as a way of finally providing firm—objective—grounds for what were previously speculative, messy attempts to estimate how old stuff is. Second, we enter a kind of crisis period, when the promise of the new technique is found to have been overplayed, and a process of calibrating to the local, complex requirements of archaeology begins. Finally, in the third phase, the new process is absorbed into archaeological practice. It becomes an aspect of archaeology itself and, crucially, is treated as just another line of evidence—no fundamental priority is granted. The new technique, and the new evidence it provides, shifts from being a silver bullet (phase 1), to being problematized and adapted (phase 2), to being part of the day-to-day archaeological toolkit (phase 3).

Phases 2 and 3, I think, undermine a common perception about how methods are traded between scientific fields. Old habits of hierarchical thinking die hard, and how we treat the co-option of work from physics and chemistry into the so-called softer sciences is influenced by this. It is often painted as if the physicists have uncovered some fundamental feature of the way the world works, and this luckily can be used by the grateful archaeologist to, in this instance, date various objects. Somehow ownership for the techniques still lies within physics. But recognizing how difficult this kind of trade is, and the extent to which it must be transformed, molded, and localized to archaeological contexts, challenges this picture. ^{14}C dating is not a technique from physics that archaeologists use. Rather, archaeological ^{14}C dating is an archaeological technique that was enabled—in part—by some advances in physics.

In my view, *Evidential Reasoning in Archaeology* contributes to three broad discussions. Perhaps most important, its clarity and closeness to practice should make excellent fodder for the internal discussions in archaeology into which Chapman and Wylie interject. I would hope that exposure to the ideas in this book would put to bed the dichotomizing debates that partly mo-

tivate it. Chapman and Wylie demonstrate that thinking that archaeology must embrace either the Scylla of enforced 'objectivity'—merely cataloging finds—or the Charybdis of unconstrained speculative interpretation is a deep mistake. Archaeologists successfully interpret the cultural past from meager remains because they can systematically interpret finds.

Second, Chapman and Wylie's discussion is in implicit dialogue with the philosophy of historical reconstruction. Their focus on practice—the social and technological preconditions required for archaeological work, for instance—provides an important counterpart to the more abstract approaches of philosophers such as Derek Turner and Carol Cleland. Indeed, while history's destructive tendency often leads Turner to be rather pessimistic of our capacity to uncover the past (Turner 2007), and Cleland is often rather optimistic because of the past's leaving varied, disparate evidence (2002, 2011), Chapman and Wylie provide a balanced analysis of the conditions under which success can occur. This kind of localized, context-sensitive approach strikes me as a fruitful way forward.

Third, the book is a fine example of a strategy for understanding science that emerges at the intersection of analytic philosophy, integrated history and philosophy of science, and science and technology studies. Often this philosophy identifies itself in terms of 'practice': instead of focusing on the outputs of scientific work such as theories or hypotheses, the authors investigate the processes that produce them. Chapman and Wylie draw generously from thinkers in this tradition, including Helen Longino, Hasok Chang, Bill Wimsatt, Peter Galison, and others (indeed, Wylie herself has a long history of this kind of work). Moreover, the book does not simply co-opt but often extends this philosophical program—particularly in the discussion of objectivity in the conclusion and, as I will turn to now, potentially in how trading zones reveal themselves in archaeology.

What is a 'trading zone'? Trading zones involve methodological, evidential, or perhaps institutional overlap that necessitates the passing of information (techniques, best practices, etc.) between scientists with otherwise diverging interests or specializations. As Galison has put it, a trading zone is "an arena in which radically different activities could be *locally*, but not globally, coordinated" (1997, 690). The currencies of trading zones, then, are divergent but useful techniques for a common purpose or mutually useful techniques for different purposes.

The notion of 'trading zone' is fairly amorphous, and certainly a loose, broad sense fits archaeology very well—indeed Chapman and Wylie use it to frame much of their discussion. However, it strikes me that there is a tension between the thought that trading zones are central to archaeology and Chapman and Wylie's story of how 'external' resources are used by archaeologists. Recall that this involves three stages: the new technique is heralded as capable of replacing the unstable, speculative ways of the past; the technique loses its silver sheen, and a period of calibration begins; and the technique is inte-

grated or absorbed. In the end, the technique, calibrated and adapted to the conditions of archaeology, becomes just another line of evidence to be considered. Where is the tension? The more that the second and third stages involve transforming the techniques, knowledge, or technology into the local context of archaeology, the less those techniques are the property of a trading zone—currency traded between fields—and the more they become a part of archaeology proper. In other words, the more the trade is absorbed, the less it looks like a trading zone—like an overlap between two fields—and the more it looks like co-option. By the third stage, the trading zone has disappeared, as the technique has simply become the furniture of archaeological research.

This tension, I think, brings lessons in tow. One is that—and this should be unsurprising—trading zones are often transient, they can be a stage a field moves through before full incorporation of the technique. Another is the importance of keeping communication lines open after the trading zone has lapsed. Chapman and Wylie articulate the importance of communication and respect (in Chang's [2004] sense) between different scientific domains, which is necessary for the kinds of fruitful integration characteristic of good archaeology. To the extent that some techniques such as carbon dating become divorced from their original homes while being transformed and absorbed into archaeology, the understanding that a working physicist might bring into the equation could be lost. Plausibly, practicing physicists have knowledge about some aspects of the relevant processes that archaeologists do not. It might be a good thing that the marriage of the trading zone breaks down—if the technique really must be so transformed to local archaeological conditions—but it might nonetheless be a very good idea to grant physics visitation rights to its estranged children.

Chapman and Wylie's book on evidential reasoning is very much focused on reasoning from material objects. And this is not surprising: such a perspective is writ into the very title of Wylie's earlier essay collection *Thinking from Things* (2002). The paradox that frames the collection is about the stubbornness of objects, and indeed archaeology does involve intense and careful interaction with, preparation of, and interpretation of the pasts' physical remnants. However, I am inclined to think they give the role of less tangible things—imagination and storytelling—short shrift in their account (although whether this is a sin of omission or commission I am not sure). In Chapman and Wylie's work it often comes across as if theories and hypotheses are dangerous things, threatening to lead us astray—to the sin of xeroxing—and would do so, were it not for the heroic stubbornness of physical objects and the acquiescence of archaeologists in letting them speak. And, of course, such dangers are very real.

However, I think an important—perhaps a central—scaffold in reconstructing the past involves narratives, theorizing, and scenario building. Such practices allow us to situate our lower-level interpretations of material remains—

they help us decide what is salient and what is not about those remains. And this matters for two reasons. First, working out what is evidentially relevant and what is not for a historical reconstruction is extremely tricky. By articulating and testing scenarios, we can identify evidential relevance (Currie 2015). Second, narratives—our picture of the past—can themselves act as evidence. If our understanding of the processes and events operating in the past is particularly rich, then the restrictions on what counts as a good, coherent narrative become increasingly strict. That is to say, 'coherency' becomes much more than a mere matter of logical consistency but makes the generating of a scenario that fits our preexisting knowledge a true epistemic achievement (Currie 2016; Currie and Sterelny 2017). So, I suspect that scenario building deserves a place alongside the co-option of external resources, the development of theory connecting material remains to the past, and the techniques, theory, and practices for fieldwork, as part of the essential scaffolding—in fact the furniture—of successful archaeology.

And so, although archaeology is hard, the impression that it amounts to pointing at small walls and making things up is far from accurate. Chapman and Wylie demonstrate that archaeological success is underwritten by the continual practice of developing epistemic scaffolding, and bringing those scaffolds into contact with the intransigence of material remains, and by maintaining the social and institutional structures required for the exchange of ideas and techniques. This continual tweaking and the physicality of remains allows archaeology to often succeed in the systematic—scientific—reconstruction of cultural prehistory.

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