



# Ethnographic analogy, the comparative method, and archaeological special pleading



Adrian Currie

*Philosophy Department, University of Calgary, 2500 University Dr NW, Calgary T2N1N4, Alberta, Canada*

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## ABSTRACT

Ethnographic analogy, the use of comparative data from anthropology to inform reconstructions of past human societies, has a troubled history. Archaeologists often express concern about, or outright reject, the practice—and sometimes do so in problematically general terms. This is odd, as (or so I argue) the use of comparative data in archaeology is the same pattern of reasoning as the ‘comparative method’ in biology, which is a well-developed and robust set of inferences which play a central role in discovering the biological past. In pointing out this continuity, I argue that there is no ‘special pleading’ on the part of archaeologists in this regard: biologists must overcome analogous epistemic difficulties in their use of comparative data. I then go on to emphasize the local, empirically tractable ways in which particular ethnographic analogies may be licensed.

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## 1. Introduction

There are similarities between contemporary and prehistoric human behavior, and so in principle the living can inform us about the dead. This thought underwrites ‘ethnographic analogy’: the appeal to anthropological reports of contemporary, usually hunter-gatherer behavior, in support of archaeological hypotheses. There are similarities between living organisms and past organisms, and so in principle the extant can inform us about the extinct. This thought underwrites the ‘comparative method’: the appeal to contemporary biological facts to support biological hypotheses about the past.

The comparative method is a well-developed, often quantified and rich set of epistemic techniques which are essential for reconstructing the biological past. By contrast, archaeologists often express concern, suspicion, or outright dismissal of (what they call) ethnographic analogies. Considering that (as we shall see) the two methods represent the same patterns of reasoning, this discrepancy is odd. Is there any reason for archaeologists, and not biologists, to worry about the use of comparative data? Is there

*E-mail address:* [adrian.currie@ucalgary.ca](mailto:adrian.currie@ucalgary.ca).

‘special pleading’ available to archaeologists which might justify such suspicion? I argue that no such case can be made. Just as in biology, the justification or otherwise of the use of comparative data is local and context dependent. My aim is to establish this point, and to make some progress on just what local and context dependent facts might matter.

As we shall see, some archaeologists appear to be wary of ethnographies in principle. For example,

... it can only be constantly restated that analogy does not provide answers, only models, hypotheses and ideas (Hayter, 1994, 42).

According to Holly Hayter, ethnographic analogies do not provide evidence, that is, they do not support archaeological hypotheses, but are limited to generating them. In Section 4, I will focus on Hayter’s discussion of analogy, as it is a rather explicit example of the common attitude I target. In a similar vein, Lewis Binford (1967, 1977) also took a conservative view on the role of analogy in archaeology:

Analogy serves to provoke certain types of questions which can, on investigation, lead to the recognition of more comprehensive ranges of order in the archaeological data (Binford, 1967, p10).

For Binford, then, analogy can ‘provoke questions’, but does not itself provide ‘answers’. When we see the continuities in the use of comparative biological data and comparative ethnographic data, it becomes clear that such positions are untenable. Ethnographic analogies certainly *in principle* have the goods to provide answers—that is, evidential support for hypotheses. Recent discussions of ethnographic analogy range from extreme caution or outright rejection (Bednarik, 2012; Berrocal, 2011; Hiscock, 2007; McCall, 2007), to more nuanced discussions (González-Urquijo, Beyries, & Ibáñez, 2015; Ravn, 2011; Whitaker & Tushingham, 2014). My aim is to establish a view on the nuanced end: there is no outright rejection or acceptance of ethnographic analogy to be made, rather, for each case the devil is in the details.

I’ll first introducing the comparative method with a paleoanthropological case study (Section 2), followed by a similar introduction to ethnographic analogy (Section 3). In Section 4, I argue against archaeological ‘special pleading’, that is, there is nothing different, as a matter of epistemic principle, between the biologist, paleoanthropologist, nor archaeologist when drawing such analogies. The interesting question, then, is under what conditions such inferences are licensed.

Appropriately, then, I will discuss what is required to vindicate or damn a particular use of ethnographic analogy. In Sections 4 and 5, I note that both ontic and epistemic issues can plague particular applications of comparative data. We must examine the strength and stability of our access to information, and investigate the properties of the systems we are examining: do they behave with sufficient regularity to support the inductions comparative data requires? I argue that even in troubling cases, where our information is poor and the systems behave irregularly, ethnographic evidence can still play an important role as one line of evidence involved in reconstructing the cultural past. I use recent work by Christine VanPool (2009) to illustrate how piece-meal, multi-leveled analyses of archaeological remains, drawing on ethnographic information, can lead to rich, well supported hypotheses.

Note that I am restricting myself to *epistemic* similarities and differences between archaeology and other sciences. There are important social, technological and financial differences which matter for how these sciences are practiced, but here I will focus on the patterns of reasoning involved.

## 2. Hobbits & hippos

In this section, I illustrate the comparative method with a paleoanthropological case study. *Homo floresiensis* were a surprising addition to the hominid family tree. Around 13 individuals were found at a single site on the Indonesian island of Flores, which they inhabited up until around 14,000 years ago (Brown et al. 2004; Morwood et al., 2005). Their most striking feature is diminutive size—adults reach a paltry 1 m tall—earning them the inevitable ‘hobbit’ epithet. In addition to their stature, they also sport ‘primitive’<sup>1</sup> features: low encephalization (that is, brain-size/body-size ratio), arboreal adaptations and incomplete bipedalism. *H. floresiensis*’ taxonomic grouping is mysterious: do their features signal a remarkable story of late hominid evolutionary adaptability, or a remarkable story of early hominid radiation and survival? Are the hobbits late hominids gone dwarf, or the last remnant of a hitherto unknown migration of early hominids? These hypotheses provide contrasting explanations of

*H. floresiensis*’ traits, which illustrate an essential distinction in the comparative method.

By the ‘early hominid’ hypothesis, hobbits are the ancestors of a primitive hominid radiation out of Africa, perhaps *Homo habilis* (Jungers et al., 2009; Wong, 2009). *H. floresiensis* and *H. habilis* share traits: they are small, low in encephalization, walk stooped, and suit partially arboreal lifestyles. By this hypothesis, hobbit traits are the result of retained, ancestral features. They are *homologues*. Two traits are homologous when they are inherited from a common ancestor.<sup>2</sup> By this hypothesis, then, *H. floresiensis* and *H. habilis*’ traits signal their ancestral relatedness and their similarity is explained in terms of that ancestry. What’s wrong with the ‘early hominid’ theory? There is no evidence of habiline hominids radiating into Asia: it was the taller, upright and more highly encephalized *Homo ergaster*, *Homo erectus* and *Homo heidelbergensis* who took the hominid torch out of Africa.

The ‘late hominid’ theory places the divergence between *H. floresiensis* and the hominid line much later—by this theory their ancestors were erectine and their problematic traits are adaptations to their island environment (Argue, Morwood, Sutikna, & JatmikoSapto, 2009). Like the pygmy elephants of Flores, the hobbits could be insular dwarves. By this hypothesis, the relationship between the traits of *H. habilis* and *H. floresiensis* is *homoplastic*, rather than homologous: the hobbits did not inherit their low encephalization, but rather it evolved via island dwarfism. *Homoplastic* traits are convergent: rather than tracing ancestry, they trace evolutionary pressure or other influences.<sup>3</sup> What’s wrong with the ‘late hominid’ theory? Standard models of dwarfism do not predict some hobbit features (Martin et al., 2006<sup>4</sup>; Jungers et al., 2009). For instance, it is thought that insular dwarfism is expressed developmentally via shorter growth periods. The difference between a pygmy elephant and a whopper, by this line, is growing time. However, different parts of the body complete growth earlier than others—specifically, brain development completes earlier than body growth. If dwarfism is the result of less growing time, then we should expect dwarves to be more encephalized than their bulky cousins, as the brain had time to mature while the body’s growth was cut short. On this model, *H. floresiensis*’ brain should be twice the actual size.

Which hypothesis is more likely: are the hobbit traits inherited homologues, or homoplastic? Were they habiline or erectine? Most obviously, this depends on whether a habiline ‘ghost’ radiation or an erectine dwarf with hobbit-like features is more plausible. However, there is more to this than meets the eye—let’s start with the ‘early hominid’ hypothesis.

The ‘early hominid’ hypothesis involves what has been called a *phylogenetic* (or *homologous*) inference (see Currie, 2014; Levy & Currie, 2015). In such inferences, common features are taken to be indicative of common ancestry, or common ancestry is taken to be evidence of common traits. In this case we infer from the similarities between early hominids and *H. floresiensis* to their having a shared ancestry. An example of the latter would be to appeal to other facts about early hominids to infer further hobbit traits, say that *H. floresiensis* used the stone-flake based Oldowan tool-set on

<sup>2</sup> This is a version of a *taxic* definition of homology, definitions of homology are highly contentious (see, for instance, Brigandt & Griffiths, 2007; Currie, 2014; Hall, 2003; Ramsey & Peterson, 2012), but this does not affect the nature of the inference considered here.

<sup>3</sup> Like homology, ‘homoplasmy’ definitions are contentious (see Currie, 2014; Pearce, 2012; Powell, 2012)—but again, this need not concern us now.

<sup>4</sup> Note that Martin et al. do not endorse an early hominid model, but rather argue that the features are pathological: ‘*H. floresiensis*’ are *Homo sapiens*. See also Jacob et al. 2006.

<sup>1</sup> Here, ‘primitive’ is certainly no insult to *H. floresiensis*, rather that some of its traits are associated with the base of the hominid line.

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