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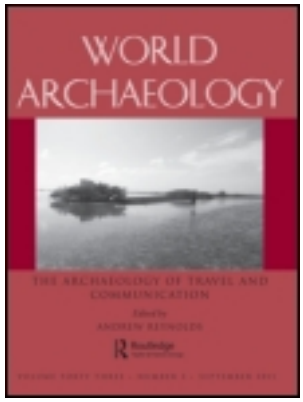

World Archaeology
Publication details, including instructions for authors and subscription information:
http://www.tandfonline.com/loi/rwar20

Ethnographic analogy from the Pacific: just as analogical as any other analogy

Mads Ravn [a]
[a] University of Stavanger E-mail: mads.ravn@uis.no

Available online: 05 Dec 2011

To cite this article: Mads Ravn (2011): Ethnographic analogy from the Pacific: just as analogical as any other analogy, World Archaeology, 43:4, 716-725

To link to this article: http://dx.doi.org/10.1080/00438243.2011.624781

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Ethnographic analogy from the Pacific: just as analogical as any other analogy

Mads Ravn

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Abstract

This paper is a comment on previous papers in *World Archaeology* by Spriggs (2008) and Roscoe (2009) on the use of Pacific analogies in archaeology. It is argued that the discussion needs to be put into a context of the general use of analogy, as Pacific analogies are just as analogical as any other analogy. Furthermore, the concern with analogy outlined by Spriggs and Roscoe is bound up in a dated definition of analogy which emphasizes similarities and assumes static societies. A better definition of analogy emphasizing not only similarities but differences and relevance is proposed. This definition makes it possible to modify analogies when confronted with refined archaeological data. Thus, we need not abolish the use of analogy, only use it in a more case-specific way, allowing for the ethnographic and archaeological record to modify the analogy and including the progress already reached through the study of analogy over the last sixty years.

Keywords

Ethnographic analogy; archaeological interpretation; Pacific archaeology; explanation.

> Model-dependent realism short-circuits all this argument and discussion between realist and anti-realist schools of thought. According to model-dependent realism, it is pointless to ask whether a model is real, only whether it agrees with observation.
>
> (Hawking and Mlodinow 2010: 45)

Introduction

It was with interest that I read the discussion on the fallacies (Spriggs 2008) and potential (Roscoe 2009) of analogies deriving from the Pacific. Both make good points. Spriggs emphasizes that anthropological analogies seem to have been mined uncritically by researchers studying the Neolithic (Spriggs 2008: 539); the argument really focuses on the

Routledge Taylor & Francis Group

World Archaeology Vol. 43(4): 716–725 *Debates in World Archaeology*
© 2011 Taylor & Francis ISSN 0043-8243 print/1470-1375 online
http://dx.doi.org/10.1080/00438243.2011.624781

need to include the history (in the case of archaeology) of the people from whom the analogies are obtained, because contact with Europeans changed many Pacific societies radically. Spriggs suggests that up to 90 per cent decline in population can be witnessed. This means, according to Spriggs, that the Big Men societies used as analogies for the Neolithic of Europe may be based on post-contact societies that are neither original nor compatible with the European past. Instead, Spriggs suggests a comparative archaeology including the long-term history and a focus on cultural sequences of the region as a better option – a point I do not disagree with. Roscoe, on the other hand, suggests that the drastic changes in Pacific societies after contact with Europeans are overestimated by Spriggs and that there still is some reliability in evidence seen to reflect the organization of pre-contact societies. Examples are emphasized from ethnographies in New Guinea. A level of much less than 90 per cent population decline is established. More likely population decline varied and certainly it was less than 30 per cent, because war among the tribes was stopped by the colonists. Roscoe questions whether the Big Men society is, as proposed by Spriggs, a postcolonial product and suggests that leadership in New Guinea was much more complex. An important additional point by Roscoe (2009: 586) taken here is that no society, neither prehistoric nor present, exists in a vacuum, so neither in the past nor in the present can we really talk of pristine pre-contact societies, as already pointed out in an interesting paper by Schrire (1984: 19).

My main concern with the discussion is that it needs to be put into a relevant context. Detached from a whole body of references central to discussions on the uses of analogy in philosophical, archaeological and anthropological scholarship, the potential for reinventing the wheel is a constant concern. Indeed my claim here is that including the general discussion on analogy is very relevant to the discussion of analogies from the Pacific, because analogies from the Pacific are just as analogical as any other analogies. So in two ways the present discussion seems a-historical: first, because the history, long-term and short-term, of the ethnographic people has apparently been ignored, a good point made by Spriggs and elaborated by Roscoe; and, second, because the historiography on the use of analogy has been overlooked by both. I argue below that the discussion on analogy in general offers answers to a number of concerns presented by Spriggs and Roscoe in their papers.

Putting the discussion of ethnographic analogy in its proper setting

The discussion on analogy took off in American anthropology and British archaeology in the 1950s and continued up through the 1980s. It somehow seemed to vanish with the advent of postmodern theory, and the subsequent reaction against it by scientifically oriented archaeologists in the 1990s and 2000s. Although it may not fully answer why this happened, it generally seems that the discussion of low-level interpretative tools such as analogy, which initiated a tremendous theoretical discussion of subjectivity in archaeology in the 1980s (Hodder 1982a), became collateral damage in the critical reaction against the use of high-level, subjective theories imported from sociology. The baby was thrown out with the bathwater. This critical reaction made one group give up theoretical interpretations altogether, concentrating on the scientific aspect of data sampling and analysis, while some of those who continued to be interested in interpretative archaeology

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lost the basic logic involved in analogy. I shall return to the content of this logic below but first we need to look at the use of analogy.

The problem of using analogy

The problem of using ethnographic analogy in archaeological interpretations goes back to the birth of archaeology and anthropology, when anthropology and archaeology were intertwined in a unilinear evolutionary framework. This approach led scientists to assume that, for example, past humans and present hunter-gatherers must represent the same evolutionary level (Ravn 1993; Trigger 1989; Wylie 1985). Here it suffices to mention that this evolutionist understanding has followed the discussion in different guises – cultural historical, functionalist, neo-evolutionist, Marxist, structuralist, post-structuralist – into the twenty-first century. The scientific reaction against it has followed the same path, reappearing in turns and apparently doing so again in the paper by Spriggs (2008).

The problem of analogy

I shall here focus on the late twentieth century. The use of ethnographic analogy was identified by Kluckhohn (1940) and DeBoer and Lathrap (1979) as a dilemma. Either you choose to use analogy and accept that you implement a level of subjectivity (and thus are unscientific) or you avoid using analogy altogether. This dilemma inspired archaeologists to develop methods and theories for applying better-founded ethnographic analogies. Many deserve mention in anthropology and archaeology for exploring the limits of ethnographic analogy (Ascher 1961; Binford 1967, 1972, 1978, 1981, 1983; Chang 1967; Freeman 1968; Gould and Watson 1982; Hodder 1982a; 1982b 1986; Hodder & Hutson 2003; Munsen 1969; Orme 1974, 1981; Ravn 1993; Sabloff and Binford 1982; Thompson 1956; Tringham 1978; Wobst 1978), historical analogy (Andrén 1998; Morris 2000; Näsman 1988; Ravn 1997, 2003) as well as the general use of analogy in philosophy (Copi 1982; Uemov 1970; Wylie 1980, 1982, 1985, 1988, 1989, 2002), but as the discussion is not new only a few instructive examples will be emphasized here.

Notably, J. D. G. Clark (1951, 1953, 1971 [1954]) used ethnographic analogy in his analyses of Star Carr – a hunter-gatherer site in Britain. He was concerned with the level of subjectivity inherent in analogy and attempted what Wylie calls 'restricting the interpretive principles' (1985: 73). This he did by emphasizing a folk-culture approach, involving direct historical continuity from past to present. The other approach was comparative in nature, where similar economic and subsistence conditions were present. In both cases he suggested that they provided a stronger, less subjective, framework for an analogy. The emphasis on a comparative approach was probably inspired by Hawkes' (1954) clear (and thus often cited) paper on the 'ladder of inference'. Hawkes, being influenced by contemporary historians and their belittling of material culture sources (Ravn 1997), suggested that without written sources we can use ethnographic analogy only where the subsistence and economic patterns are very similar between the ethnographic source and the archaeological subject. Additionally, Hawkes suggested that it is only on the lower

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rungs of the 'ladder' (subsistence and economy) that we can deduce anything about the past – here the variability is not as great. Issues higher up the ladder, such as social relations and ideology, were entirely out of reach without written sources. This rather pessimistic approach continued through the 1950s and 1960s and a number of so-called cautionary tales were presented for the anxious scholar (e.g. Orme 1981). An example was Clark's (1971 [1954]: 11) often cited contention that activity areas with scrapers must have been female activity areas, as among contemporary arctic hunter-gatherers where the scraping of skins was a female responsibility. In this way Clark not only ignored the possibility that the past may have been different from the present, but also imposed his (at that time prevalent) Western ethnocentric view on the past. Such errors caused a number of scholars to warn against the use of analogy altogether.

Binford sparked a renewed interest in the use of analogy with his optimist program: 'archaeology is anthropology or it is nothing' (1962). He did not like the word analogy, but did demonstrate in a number of cases how present hunter-gatherers and farmers could be used as a basis for postulating hypotheses which might explain past activity. Many examples could be presented here, but I shall limit myself to a few. Binford suggested, for example, that a number of smudge pits identified from the Toothsome site, Clinton County, Illinois (Binford 1967; Binford et al. 1964), were used for hide smoking rather than as smudge pits for keeping away mosquitoes, as originally proposed. By piling up numerous ethnographic examples from contemporary Native American cultures (what I below will call the 'source side' of the analogy) he demonstrated that this was, in fact, tested out and proven. With a scientific ideal derived from Hempel, and later Popper, he advocated a method of deductively drawn hypotheses, a scientific paradigm that became, and for some still is, the ideal of archaeology. This approach is the opposite of the inductive method of analogy. However, just a few years later Munsen (1969) demonstrated the weakness of this method, though it was in many respects overlooked until the 1980s. Munsen suggested that the pits might just as well have been used for smoking the inside of pottery. This function of pottery preparation was indeed better supported in the archaeological record (the subject side of analogy) than the numerous contemporary and recent parallels (the source side of the analogy) promoted by Binford. Specifically, carbonized corncobs were found in association with the pits, a feature also seen in ethnographic examples where pottery smoking was practiced; additionally the dates and locality fit the latter interpretation better. I can add a further example from my own research. Here a specific type of paddle-shaped artifact used in the New Guinea highlands as a shovel is refuted as being a shovel when found in a marine context in Denmark. Although the tool is identical in form, the archaeological context (the subject side of the analogy) deviates from the analogy (the source side of the analogy) from New Guinea, because this tool was found near a submerged canoe (Ravn 1993: 68). In other words, the context suggests that the artifact was more likely used as a paddle in prehistoric Denmark 6000 years ago, though a similar-looking tool is used as a shovel in the present New Guinea Highlands. Here both cases suggest that contemporary ethnographic evidence *can* be refuted by the archaeological record. Additionally this interesting discussion demonstrates that no matter how many contemporary ethnographic cases you may be able to present for an argument of similar use, a *relevance* and a *relationship* must be demonstrated and as many causal connections as possible assessed before the rigor of the analogy is satisfactory.

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Additionally, the discussion above presents the core problem of archaeology, which Wylie (1985) calls the problem of equifinality – that different activities may lead to the same end result in the archaeological record – a problem we cannot get beyond in archaeology. What we can do, however, is to refocus on the constellation of the archaeological record, our data and the *causal connections and relations* between the source side (the present ethnographic cases) and the subject side of the analogy (the archaeological record), as indeed Munsen did and my example showed. But, before I elaborate on this, I shall dwell briefly on the definition of analogy, which in many ways became the reason for the concern with its practice.

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Defining analogy and a critique of the processual discussion

Binford and others following him more or less overtly defined analogy as 'the inference that certain admitted resemblances imply further similarity' (Gould and Watson 1982: 371; see also Binford 1967: 2). A comprehensive discussion and analysis of analogy by Wylie (1982, 1985) suggested a better and more operational definition: 'the selective transposition of information from source to subject on the basis of a comparison that, fully developed, specifies how the "terms" compared are similar, different, or of unknown likeness' (Wylie 1985: 93).

From that definition Wylie distinguished between formal and relational analogy. A formal analogy is a 'point for point assessment of similarities or differences in the properties of source and subject' (1985: 94), whereas relational analogies assume 'a function of knowledge about underlying "principles of connection" that structure source and subject and that assure, on this basis, the existence of specific further similarities between them' (1985: 95). In other words, the weak analogies and cautionary tales presented earlier (e.g. Freeman 1968) are really formal analogies which do not take into consideration the relevance between the (ethnographic) source and the (archaeological) subject. The example above of the discussion between Binford and Munsen on smudge pits should elucidate the distinction, because Munsen lines up a number of relevant factors in the finds (corncobs), find patterns (i.e. dates) and locale that eliminate the possibility that the pits were used for hide smoking. Also important in Wylie's distinction is the emphasis on *differences* as well as *similarities*, and the reasons for the differences; as Munsen's arguments elucidate, the past may be quite different from the present. Indeed, as Munsen's and my own examples demonstrate, we may be able to discover this difference and its relevance if we look more closely at the archaeological context.

The pacification of analogy?

This is the way forward in the use of analogy, whether from the Pacific or anywhere else. Therefore elaborating the analogical (to wit ethnographic) sources and establishing a causal connection with the archaeological subject is the only way to evaluate the rigor of analogy. This is in my view what both Spriggs and Roscoe do by refining the discussion on

post-contact consequences (the source side of the analogy) and the emphasis by Spriggs on the diachronic perspective and *difference* of the archaeological record (the subject side of the analogy). Ideally, once sufficient links are established, analogy may be replaced by a theoretical explanation. The latter is, according to Wylie, not possible within archaeology: to use extrinsic theory is to extend the theory into a domain which may not be appropriate to the archaeological record, given the possibility of equifinality. Additionally, theories about prehistoric human behavior cannot easily be tested, as the above example on smudge pits demonstrates. Therefore, we should regard analogical sources from ethnography as instructive for archaeology, as inspiring ideas about processes which may have occurred in the past. One need not account for every similarity between ethnographic sources and archaeological subject, as a single property from one source or number of different sources can be tolerated as long as the *relevance* between analogical source and subject is demonstrated. Wylie suggests that in this way 'the model may be a conceptualization of a context that is substantially unlike any single accessible analog' (1985: 106).

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Discussion: the return of the inductive method?

In my view this discussion contains the potential to reinforce analogical inferences, as it is a logic that is used in all scientific reasoning – it really is the essence of logic (Copi 1982; Wylie 2002). The degree of subjectivity which was allowed for became a relieving liberation from the straitjacket of a processual objectivist-deductive archaeology (Hodder 1982a, 1982b). However, it also became embraced by a growing and diverse postmodern discourse (for an overview, see Hodder 1986; Hodder and Hutson 2003). Somewhere along the line the use of analogy got hijacked by a postmodern critique that emphasized subjectivity and similarities between the contemporary source side of the analogy and the subject (the archaeological data), rather than *differences and relevance* of those theories to the subject. This allowed for several generalizing theoretical models. Some of those postmodern scholars are in fact the very persons criticized by Spriggs (Thomas 2004; Tilley 1996) for using the Pacific models at too general a level. In other words, Spriggs' critique may be a valid one, as I read his examples, but this does not mean that we should abolish the use of analogies, or Pacific analogies specifically. In essence, the main argument of Tilley and Thomas does not distinguish itself from the argument proposed by Binford in the above example. First, a generalizing model substantiated by numerous contemporary cases and an underlying uniformitarian thinking within the theoretical paradigm promoted is made in the same way as Binford did in his presentation of numerous contemporary cases of Native Americans smoking hides. But the problem is that it accounts for only the source side of the analogy without identifying enough *relevance and/or difference* between analogical source and archaeological subject. An inductive approach that allows for both the theory-laden-ness obvious in the use of analogy and the independence needed for testing archeological interpretations in the historical sciences (e.g. archaeology) has been outlined eloquently above and elsewhere by Wylie (2002: 179). This approach seems to me quite similar to what Geertz calls 'thick description' (1973: 20).

Conclusion

With this discussion in mind, my main comment on the papers by Spriggs and Roscoe offers counsel of neither despair nor caution. Rather it offers one of prudent confidence in the inductive method and in the ethnographic and archaeological records. As shown in the examples above, we may be able to disprove present models generated from contemporary analogical sources if we are more concerned with the relevance and *differences* in both as well as the similarities. The solution is to work in a more balanced way on both sides of the equation of the analogy: for example (as in the present two papers), accounting for the historicity and relevance of the ethnographic record (expanding the source side) as well as including the singularity and detail (hereunder also historicity) of the archaeological record of the Pacific. Indeed, in some hard sciences the uses of various low-level and less rigorous methods such as analogy and rational conjecture are used extensively without discrediting the discipline. I particularly like the model-dependent realism proposed by Hawking in the epigraph to this article, which does not deviate too much from the logics of analogy (Hawking and Mlodinow 2010: 45; see also Norris 2011: 23). Also he, like Wylie, does not think that we can reach a single theoretical explanation.

Luckily, a growing battery of methods for extracting new, important, independent, multi-related and detailed information distinguishing features of the archaeological subject (GIS, relational databases, aDNA, isotopic analyses and materials science to mention a few) may in the future refine, and potentially reject, present subjectively drawn analogies. In this way, the past may be another foreign country to explore which is 'substantially unlike any single accessible analog' (Wylie 1985: 106). The task is to be wary that the scientific disciplines, whose tools we employ, do not export their natural scientific demand for final deductive proof to a discipline where we must allow some room for inductive subjectivism and where the sum of evidence may sometimes act as preliminary, but not final, proof. Wylie (1988, 1989, 1992, 1995) compares this approach with a ship tacking into the wind – an appropriate parallel, I would think, that resembles the dialectical process promoted by Roscoe (2009: 586). In this perspective I do not disagree with Roscoe's final remark that our ultimate goal

> should be to aim for models that can account for both the ethnographic *and* archaeological record, models of social and cultural *process* that allow us to understand and, at least in broad terms, forecast how humans did, do and will behave in different material, demographic and symbolic landscapes.
>
> (Roscoe 2009: 587)

To reach this goal, anthropology and archaeology should work even more closely together while archaeology additionally should concentrate more on the whole process of understanding the archaeological record and the genesis of data and our role in this process (Madsen 1995; Wylie 2002).

I find the discussion by Spriggs and Roscoe an interesting starting point for continuing the long discussion outlined here. My argument can be summed up by the following quote, supposedly from the economist John Maynard Keynes: 'Those scientists who claim to have no use for philosophy are most likely in the grip of a bad old philosophy or an

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insufficiently thought-out new one that they do not fully acknowledge' (quoted in Norris 2011: 24). Replace the word 'philosophy' with 'analogy' and my point is made.

Acknowledgements

Thanks to Sean Denham for critical comments on language and content. Potential errors are my own.

Mads Ravn
University of Stavanger
mads.ravn@uis.no

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Mads Ravn is an Associate Professor and Research Director at the Museum of Archaeology, University of Stavanger, Norway. He has conducted numerous fieldwork projects in Scandinavia and other parts of the world, lately also in the Pacific. His approach is interdisciplinary and the last twenty years he has focused on the early Medieval Period (historical archaeology), issues of ethnographic and historic analogy, the nature of written and archaeological sources. Other interests are the early Neolithic, the Pacific, themes mainly related to a number of theoretical and methodological issues, such as migration, ethnic identity, data-handling, categorization, chronology and GIS. He is currently working on how present rescue excavations in Norway may be better research oriented and is coordinator of the national program financed by the Norwegian Research Council pursuing this aim, called Development of Agrarian Societies. He is also head of the scientific research program Scientific Archaeological Laboratory Studies based at the Museum of Archaeology, University of Stavanger.