Archaeology and Anthropology – Brothers in Arms?

On Analogies in 21st-century Archaeology

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> *How can I reconcile what I see with what I know?*
> Dana Scully of the X-files

The relationship between the disciplines of anthropology and archaeology has been repeatedly debated over the years. Some scholars express a somewhat patronising attitude, like the statement of the Old Timer that "There is no 'archaeological' theory. There is only *anthropological* theory" (quoted in Flannery 1982:269), which reduces archaeology to a semi-scientific subdiscipline in the general field of humanist and social studies. Indeed, it is safe to say that the flow of ideas has mostly been one-directional. Archaeologists have frequently, and somewhat uncritically, adopted traditional, anthropological models such as "band-tribe-chiefdom-state", "lineage-based societies", "big-man systems", etc. and frequently employ practices and beliefs of contemporary low-scale societies as a way to "put flesh back on the bones". Archaeologists, it seems, lack faith in the material record as sufficient for social analysis. A somewhat strange attitude, as the past is in many respects "unknown" to us and not necessarily similar to practices of the contemporary world. In this

perspective, it seems irrelevant and somewhat unimaginative to base our models and fictions upon contemporary, cross-cultural data instead of make better use of the embedded social information in the material record. Another reason for such an *archaeological* approach is found in the crisis, the lack of credibility, that anthropology has faced the last decades. Post-colonial theorists have questioned the ethical dimension in the western studies of a "primitive other" (e.g., Bhabha 1994; Haraway 1989) and strands of post-structuralism have questioned the supremacy and validity of living informants and written accounts (Ricoeur 1986; Moore 1994). Here archaeology is better off, as we may as well regard prehistory as *free* from literary sources and living informants. Archaeology has a great potential to explore the possibilities of a *microarchaeology*; the study of *particular* material evidence in singular, time-space frames, and investigate the possibilities of a "sociology of things", both in prehistory and the contemporary world (Fahlander 2001; Cornell & Fahlander 2002a, 2002b; Fahlander 2003). We can find much inspiration from the increasing number of recent materiality-studies in other disciplines (e.g. Latour 1991, 1992; Norman 1993; Riggins 1994; Gottdiener 1995; Komter 1998; Dant 1999; Chilton 1999; Schiffer 1999; Preda 1999; Graves-Brown 2000).

These arguments call for a reconsideration of the future relations between anthropology and archaeology. We may actually find that the traditional big brother – little brother relationship between anthropology and archaeology may be turned upside down! In this text, I shall identify some main problems in the use of cross-cultural data in archaeology and anthropology and focus on other ways to employ different "knowledge" of social practices in our analysis of the past.

The problem(?) of analogies in archaeology

In archaeology, the use of comparisons between material culture and the social practices of small-scale groups is a well-established practice. At the basic level, archaeologists more or less auto-

matically categorise certain, prehistoric stone artefacts as tools based on such information. The epistemological and source-critical aspects of such cross-cultural comparisons were very much on the agenda in the 1960s to the early 1980s, but the debate more or less vanished in the later decade. It is therefore somewhat surprising to find two recently published books that concern these issues: Chris Gosden's (1999) *Anthropology and Archaeology. A Changing Relationship,* and the BAR volume, *Vergleichen als archäologische Methode: Analogien in der Archäologien,* edited by Alexander Gramsch (2000a). The books differ in many respects. Gosden, arguing from an Anglo-Saxon perspective, suggest a closer relation between the two disciplines, whilst many authors in the BAR volume express more of a heterogeneous view on how anthropological data can, or ought to, be employed in archaeology. It is not very surprising to find the most explicit discussion about analogies in a German anthology. While the debate has more or less vanished from Anglo-American archaeology, it has continued to be discussed in the German-speaking context (Holtorf 2000; Reybrouk 2000).

Gramsch (2000b:4) suggests that the lack of a cultural anthropology in Germany might have resulted in a greater interest in justifying the relevance of ethnoarchaeology to archaeology. German archaeology has a slightly different tradition regarding that question; it may suffice to mention the con-troversial theories of Gustav Kossinna, who refused to compare German prehistoric culture with any of the "lower races" (Kossinna 1911). In addition, the Austrian Oswald Menghin had a very curious way of looking at other cultures. He proposed a simple view, in which a culture either filled the criteria or not. Menghin was not concerned whether or not a "traditional" culture was mixed with Western or European influences. In his view, a specific culture is either a culture or not a culture at all (Menghin 1934). As far as I know, there are no similar statements in the Anglo-American debate.

American archaeology, unlike German and general European archaeology, has not studied 'its own' heritage, but primarily studied the prehistory of the *Other:* the Indian. The fact that most

American universities put archaeology and anthropology in the same department is perhaps an effect of these special conditions (cf. Hodder 2004:93-6). Despite the disparate theoretical heritage and disciplinary circumstances the differences are not that significant. The Anglo-Saxon perspective is generally more liberal, seeking to avoid the differences and controversies. Gosden's book is one of many examples of that tradition (despite his Australian descent). His primary argument for a closer relation between archaeology and anthropology is their "overlapping subject matter", their common history and similar relations to colonialism (1999:9). He aims to provide a bridge between the two disciplines by "…seeking conjunction between cultural anthropology and archaeology, around issues of agency and practice in relation to the material world" (1999:119).[1]

Gosden argues that anthropology needs the temporal information from history and archaeology to understand better the present situation. That is perhaps true in a general sense, but to employ archaeological data to give a temporal depth to present-day studies, those data cannot be only influenced by social models based on contemporary anthropological or ethnographical data. This is a paradox that Gosden and others do not fully recognise. If archaeologists employ and filter their data through models (fictions) constructed only on present or historical data, then prehistory will be very familiar and the practice more or less self-fulfilling. One such example is found in Tilley's book *An Ethnography of the Neolithic* (1996). Tilley presents a critical stance towards inference by analogies: "The past thirty years of research and debate in archaeology have more than amply demonstrated that there are no cross-cultural generaliz-ations going beyond either the mundane or the trivial" (Tilley 1996:1). Tilley argues that we should think in differences rather than in similarities and that we should do it through the archaeological evidence (Tilley 1996:1f.). Nonetheless, as Gosden ironically notes, Tilley's Scan-

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[1] Still, despite his attempt to treat both disciplines equally, the anthropological references in Gosden's book are twice as many as the archaeological.

dinavian Neolithic still has a "Melanesian feel to it" (Gosden 1999:8). This particular case illustrates the "problem" of anthropological and ethnographical data. They seem to have a habit of being transformed from illustrations to the status as facts at the same time as they prohibit other, maybe more relevant, models to be explored. A central issue is hence how information or data are supposed to complement each discipline, that is, in which ways archaeology can employ *any* kinds of historical or contemporary social theory and data.

Comparing apples to oranges?

One area where the fissions are explicitly present is the question regarding analogies and the use of social models in both disciplines. The debate regarding analogical inference has been very much an archaeological issue, although it certainly is central to anthropology as well. This particular issue also transgresses the traditional division between processualism and post-processualism; there are no major differences other than terminology and rhetoric. In a general sense, middle-range theory and ethnoarchaeology are pretty much the same practice. In the BAR volume, Reybrouck argues that the differences are a mere question of *causality* (2000:48); a shift from emphasising imperative factors and adaptation to less determinist perspectives, focusing on how humans relate to their world and how they bestow meanings on it.

In culture-historical archaeology, inference by cross-cultural analogies appears to have been much of a common-sense methodology in the early history of archaeology and anthropology. The Swedish archaeologist Sven Nilsson is one of the mid-century scholars who compared prehistoric, material culture (stone axes etc.) to contemporary South-American cultures to explain features in the northern Scandinavian archaeological record (Nilsson 1866:27f.).

> ...It should also be clear that the only method to appropriate *secure* and *total* knowledge about all these tools, about the way they were shafted and used, as about the tasks on which they were utilised etc., is to investigate whether such stone tools are still in use by contemporary, wild peoples, and to examine how they work with them. If we find precisely similar tools of these peoples, both of form and matter, then we can safely infer a similar utilisation; we will further not be mistaken, if we on the same grounds assume a similar life-style and knowledge of these contemporary wild peoples and those who several millennia ago ceased to exist in our northern Scandinavia (Nilsson 1866:27-8, my translation).

The increasing knowledge of other "primitive" cultures derived from the early colonists' and travellers' accounts made it tempting to compare prehistoric artefacts with the tools used by the colonised peoples. Processual archaeologists, such as Derek Freeman, have criticised the Victorian type of "primitivism", as expressed by, e.g., Lubbock (1865:336f.) and Sven Nilsson (1866:27f.). Freeman argues that contemporary Bushmen or Aborigines are not "social fossils", who can be compared with their supposed prehistoric counterparts (Freeman 1968:263). Also Binford is opposed to the use of ethnographic parallels to simply "interpret" archaeological data pointing out that increased ethnological knowledge does not by itself tell us more about prehistory (Binford 1967; 1968:268).

Binford more or less rejects analogies; he argues that ethnographical data should only be used for model-building. These "tested" and approved, ethnographically based models, then mysteriously transforms to be "facts", beyond the analogical state. This approach which Binford termed Middle-Range Theory (MRT) was, despite the crude and evolutional view of social practice, an ambitious enterprise that from a less stereo-typed stand-point could be interesting to develop (cf. Cornell & Fahlander 2002; Forslund, in this volume). Binford's aim was to establish links between day-to-day practice and how the

archaeological record was formed. The problem is that Binford stretched the generality of such links a bit too far.

The post-processual or "contextual" archaeologists of the 1980s never really explored the possibilities of MRT, but carried on seeking to distinguish "proper" analogies from "improper" ones. For instance, Hodder stresses the importance of context and suggests that questions of *relevance*, *generality* and *goodness of fit* may strengthen analogies (Hodder 1982:22). Alison Wylie (1982; 1985; 1988) and Richard Gould (1980) hold similar positions, proposing a multiple, comparative approach, although Wylie points out that the difference between simple and relevance-based analogies is in fact marginal (Wylie 1988:144). The main difference between post-processual ethnoarchaeology and processual MRT is more of a shift in perspective – from *how* things were done to *why* they were done. This is a major step away from simple observations of cause and effect(s) to complex areas of ideology, cosmology or ontology. Hodder and other postprocessual archaeologists suggest that some analogies are more valid or probable than others. But what are the premises for such distinctions? Is there really any significance about closeness in time and space or level of technology? Are comparisons of ideology between past and present socialities equal to piecemeal analogies of artefacts?

Analogies: same, same, but different?

Hodder notes that almost every archaeologist would assume that a circle of post-holes (i.e. regular patterns of coloured soil) indicates a building, unconscious of the original ethnographic reference to the houses of modern Africans or American Indians (Hodder 1982:11). As Hodder, among others, further points out, these basic piecemeal analogies, or general analogies, are not always as dubious as one might think; there are additional methods of strengthening the assumptions (Hodder 1982:11; Orme 1981:21f.; Charlton 1981:133). For instance, stone artefacts can be analysed for microwear traces to determine usage (Semenov 1964), and imitative experiments can be conducted in order to check the

practicability of the assumed application (Ascher 1961; Costin 2000). The problems appear when we move from such low-level types of analogies to cross-cultural comparisons involving questions of economies, social and cultural organisation, mythical beliefs or ideologies.

There have been many attempts to ratify the use of additional data to help interpret the fragmented archaeological record. In the early 60s Robert Ascher stressed "*the new analogy*", which simply means that analogies should only be made between peoples who "use similar environment in a similar way" (Ascher 1961:319). In his article, Ascher tries to find certain restrictions and seeks to identify parameters to control the use of analogies. He emphasises the importance of additional parallels to avoid the "Bongo-bongo phenomena', i.e., the probability that you will encounter at least one group in the ethnographical record who behave in a certain way. He also made a distinction of a special category of "*historical analogies*". Ascher regards the historical analogy as more reliable, assuming a continuously unbroken tradition in certain areas from prehistory to the present. Richard Gould (1980:32) echoes this distinction in the 1980s under the labels *continuous* and *discontinuous* analogies, and some contributors to the BAR volume seem to subscribe to such distinctions (e.g. Robrahn-Gonzaléz 2000; cf. Lyman & O´Brien 2001).

But can we really assume an unbroken chain of traditions, as Asher and Gould argue? For how long periods of time are such proposed historical or continuous analogies meaningful? Jaan Vansina (1965) has argued that oral traditions in general are valid for at least 200 years, in some special cases even down to 1000 years. Of course, some social practices might have continued invariably for a long period of time, but their purpose and meaning were more likely to have been transformed, misunderstood or changed (cf. Derrida 1974). This kind of reasoning is perhaps an effect of the lack of temporality in anthropological work that Gosden points to. Nonetheless, the historical approach is essentially patronising, viewing indigenous cultures as "cold" and stagnant without social progress or flexibility. From a

historical point of view, cultural continuity over a longer period of time-space would in itself be an anomaly (Leach 1989:45). There are no, and have probably never existed any, "cold" or "traditional" societies out of time, which unchanged and un-affected are ticking along like clockworks.

Similar questions also arise regarding the distinction of the *relational analogy*. Hodder distinguishes *formal* analogies from *relational* as two opposites on a continuum of variation. The first category concerns analogies between two objects that share a number of properties. The relational analogy, "…seeks to determine some natural or cultural link between the different aspects in the analogy" (Hodder 1982:16). But can we really determine what is relational and what is not? Of course, such questions always depend on the social context. In this case, it is easier to understand the structure-functional point of view. If one is willing to accept the postulates of cultural systems divided into subsystems and the typology of social formations such as the band-tribe-chiefdom-state scheme, then relational analogies may be meaningful. But from a less formalist and non-evolutionary, post-processual perspective, such "relational" aspects have little relevance.

For instance, are the social practices of the Inuits less relevant to the European Palaeolithic than those of African Bushmen? Is the climate or milieu most important, or is it group size, political organisation or level of technology? It is hard to see that there can be any general aspects of social behaviour that have supremacy over others; such distinctions have to be specific for each unique case. Despite the critique, the idea of the relational or "proper" type of analogy is persistent in post-processual archaeology (e.g., Ravn 1993; Ember & Ember 1995).

The third strand in the debate concerns the question of whether or not multiple, that is, additionally coinciding analogies strengthen interpretations. This may seem like a sensible approach, but it is nevertheless meaningless from an episte-mological point of view. In the BAR volume, Bernbeck advocates, in line with Hodder and others, the use of additional analogies

(Bernbeck 2000:143). Her arguments lack logic, as she strongly opposes the use of analogies when interpreting gender and gender roles, to which we can actually find a mass of coinciding references from the ethnographical record. This unequal state of affairs between the sexes is perhaps more a result of the power of contemporary, patriarchal ideology during the last two or three millennia, rather than a "natural" order (Fahlander 2001). Nonetheless, it is an illustrative example that shows how little multiple references strengthen interpretations of other social formations.

Analogy: the relationship between two things which are similar in many, though not in all, respects.

Analogical Logic: the assumption that, if two things are similar in one or two respects, they will be similar in other respects.

(New Webster's Dictionary 1992)

It is thus important to recognise that cross-cultural comparisons are not equivalent to the formal logic of analogies. An formal analogy is based on the assumption that, if two things are similar in one or two respects, they will probably be similar in other respects. An analogy between two such seemingly similar things has no epistemological value by itself; it is based on probability theory and the number of properties that are common to both objects. In a strictly logical sense, the significance of the analogy may be calculated in order to approximate the probability of similarity between the unknown properties of one of the objects. Analogical inference in this sense is not so different from traditional induction. Far more complex are comparisons of social practices and ideologies. In those areas, probabilities are impossible to calculate. Cross-cultural comparisons of social practices are both inductive and deductive, as they also involve a choice between ranges of different comparative, objects. They are

different from other archaeological methods, such as typology, which can be correlated with, e.g., carbon dating. Indeed, it seems that analogies concerning human behaviour have little conclusive value besides the rhetorical argument.

The revisionist critique

Another central feature in the discussion of relevant and non-relevant comparative data is the arguments of the so-called revisionist or critical anthropologists. It is commonly known that there are numerous pitfalls in interpreting ethnographic/anthropological "data" (e.g. Aunger 1995 and discussion; Leach 1989; Gould 1980:36; Friedman 1994). I shall not extend that particular debate here, but I would like to point to some matters of importance for the question of cross-cultural comparisons.

The first is the question of how the ethnographic record has been compiled and organised by ethnographers and anthropologists. Gosden discusses these problems to some extent and delivers several examples (1999:41, 103-8). One concerns the circumstances in which Malinowski worked on the Trobriand Islands in 1916. It was not a pristine social formation that he studied. He started out in a government compound, which included a prison, a hospital, twelve white residents, a large pearl industry and a large plantation of coconut trees. Malinowski also "overlooked" the fact that there had been Christian missionaries and traders present for at least twenty years before his arrival (cf. Leach 1989). In contemporary anthropology, the colonial influence and contaminating effect on politics, social practices and cultural boundaries all over the world are nowadays quite well recognised (e.g. Vansina 1989:244; Billington 1991:68-73; Stahl 1993:247-9; Gellner 1995; Tierney 2000). Leach, when discussing the impact of outside influence, states that "traditional culture is simply not available for inspection and has never been" (1989:39). He stresses that "outside influence" has been neglected in the history of anthropology and hence has created a distorted picture of "traditional" cultures.

The case of Malinowski might be argued to be a very special and unique example, highlighted by his posthumously published diaries, but there are nonetheless similar objections to most of the prominent anthropologists of the 20th century. For instance, Radcliffe-Brown gathered most of his genealogical data for his study of 130 aboriginal tribes from a remote hospital for venereal diseases (Layton 1997:69). It is hardly surprising that his material showed structural regularities, as the material had already been catalogued in such a manner by the colonial administration. Levi-Strauss noted a similar example. During the 1930s, about thirty, separate, aboriginal groups were gathered together by the Australian authorities in a camp where they were mixed with other groups and exposed to missionaries and soldiers. The camp included separate sleeping-houses for boys and girls, a school, a hospital, a prison, etc. (Lévi-Strauss 1963:67-114).

Gosden also discusses the circumstances of the Bushmen in Africa, who have for centuries been forced into the army and have moved in and out of pastoral, agricultural and foraging ways of life (1999:102f.). Similar objections can be raised to another frequently used analogy of the Big-man system of Papua New Guinea. According to Gosden, that particular social organisation is a result of the breakdown of the shell-money system caused by colonial interference (cf. Friedman 1994). The result of the colonial bias is a quite ordered, ethnographical record, which probably has little relevance to archaeological interpretations.

Besides these examples of the modern bias, it is also important to stress the impact of other earlier "contamination" by, e.g., medieval Muslim societies, the Chinese Empire, the Bantu system in Africa and many other influencing large socialities (Friedman 1994; Fahlander 2001). In addition, we ought to recognise that there certainly was quite intense interaction in prehistory as well to an extent which is unknown to us. Such processes are hard to establish, but the spread of raw materials and artefacts does indicate that different areas were in direct or indirect contact with each other, though not necessarily in the form of population movements (cf. Clark 1994; Kristiansen 1998).
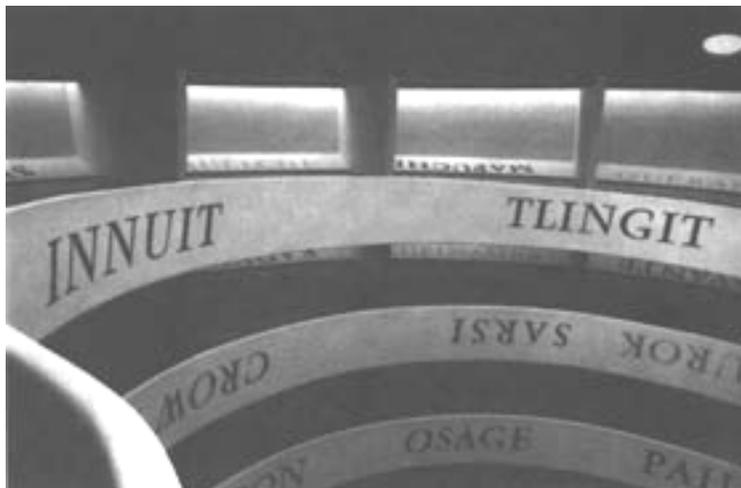
Fig. 1. Lothar Baumgarten, *American Invention* (detail), Guggenheim, New York.

Hybridisation and creolisation

In *cultural studies*, globalisation is a common theme, suggesting that all cultures are mosaics or hybrids (e.g., Hannerz 1987; Bhabha 1994). The globalisation of popular culture, the expansion of multinational companies, the formation of unions like the EU, the massive adaptation to the market economy and, of course, the pan-national Internet are mentioned as important factors in such a process. Nonetheless, this prospect is perhaps less likely to involve all kinds of social practices, strategies and behaviour. Instead of hybridisation it may be more relevant to speak about a homogenisation of certain powerful structurating practices in the long term. It is not the world-wide spread of Coca-Cola as a beverage that is important in this process, but the implicit ideology behind it that matters. The process is perhaps more about producing and reproducing clusters of *structurating practices*

and *structurating positivities* (Fahlander 2003:40-47). It seems obvious that the social practices in many small-scale, "traditional" societies have been to different extents transformed/influenced by "outside" interference, as well as, their particul local aspects. Such changes/transformations are always tied to ideological trans-formations (i.e. changes in structurating positivities). A number of social practices may seem to be unchanged but their social significance may very well have been altered or fragmented (cf. Kristiansen 1998).

Following that argument, there is little value in comparing, say, burial patterns of "separate groups" (i.e. cultures) as in the well-known case of Binford's analysis of mortuary practices (1971). By compiling ethnographic data about forty non-state societies, Binford concluded that there are strong relations between the social identity (i.e. sex and status) of the deceased and the complexity of the "cultural unit". He also showed that social identity is likely to be displayed in corresponding discrepancies in burial practices.

> Variations among cultural units in frequencies of various forms of mortuary treatment vary in response to (a) the frequency of the character symbolised by the mortuary form in the relevant population and (b) the number and distribution of different characteristics symbolised in mortuary treatment as a function of the complexity and degree of differentiation characteristic of the relevant society (Binford 1971:25).

Binford implies that these patterns are universally human and suggests that we may expect to find the same pattern in past societies. However, what Binford actually achieved in his study was to establish that in *our time* sex and status are prominent categories in many low-tech marginalised societies. When looking at past societies, that information initially is meaningless; we cannot assume a continuous tradition of such properties. We may, however, regard Binford's results as a hypothesis and investigate whether archaeological data from a specific time and place

support it. We should not, however, be surprised if the data do not fit (cf. Fahlander 2003:87ff.).

Fig. 2. "...we visited an ancient burial ground to collect Polynesian skulls. We were, however, accompanied by a Polynesian "spy", whom we tricked to follow me while Liv filled a sack with skulls for anthropological research" (Heyerdahl 1974:117).

It may be argued that such an awkward, analogical inference belongs to a past phase of archaeological practice, but it is easy to find more recent examples. For instance, in *A Phenomenology of Landscape* (1994), Tilley transfers a symbolic cosmology and shamanism of contemporary hunter-gatherers to the Mesolithic and Neolithic inhabitants of prehistoric Britain. He devotes a whole chapter of his book to ethnographical descriptions as a conceptual background (1994, ch. 2). The transference of the cosmology and mythical beliefs of contemporary hunter-gatherers

to prehistoric Britain are assertions which in no way strengthen Tilley's argument (symptomatically, anthropological studies suggest that the shaman activity of African hunter-gatherers increased during the earliest period of contact with the Western colonialists). It may have been more interesting if Tilley also included the works of, e.g. Lynch (1960), Davis 1990 and Soja (1996; 2000) on movement, perception and visibility in present day city of Los Angeles. Such modern studies are no less relevant to archaeological analysis; in fact, it may actually be helpful in getting rid of some of the mysticism and primitiveness that prehistoric people far too often are charged with.

One conclusion that can be drawn from the revisionist and post-colonial critique is that there are no genuine anthropological objects to employ as analogies. What we find in the anthropological record is a result of several hundred years of marginalisation/expansion processes and modernist ideas of how small-scale, social groups normally behave. The popular view of pre- and protohistoric, socialities as isolated, uniform, ethnic groups, "doing their thing" in their local environment, is indeed a hollow one (cf. Lévi-Strauss 1999:14f). Gosden and, I presume, most anthropologists of today are aware that no "cultures" exist as isolated islands, but how to deal with this situation and its implications for anthropology and archaeology is to Gosden an "ongoing question" (Gosden 1999:181). It is hard to identify any solution to this "problem", but one promising way is to study structurating practices as repetitive actions that do not need to be "understood" in any temporal, social or geographical frame. Such a programme, a microarchaeology, is outlined in a number of texts (e.g. Cornell & Fahlander 2002a; 2002b; Fahlander 2003; Johannesen, this volume).

From such a perspective, anthropology will have serious difficulties in obtaining a status as an independent discipline in relation to other social sciences, such as sociology. This, of course, has implications for archaeology as well. The revisionist argument has radical consequences for most traditional core-concepts, or core-universals (Cunningham 2003), and objects of study. Kinship

ties, marriage regulations, incest taboos or the concept of the nuclear family are perhaps not relevant to prehistoric, social formations. Such structurating practices may be quite recent social phenomena.

Beyond analogies? - Metaphors, metonyms and difference

Texts concerning analogies in archaeology seldom concern any new developments or insights, but we find a controversial attempt by Cornelius Holtorf in the BAR volume. Holtorf subscribes to a relativist position, arguing that neither past nor present societies are understandable: "It is obvious that, after studying the same evidence, different conclusions could have been reached equally well" (Holtorf 2000:167). Such a perspective offers a comfortable escape from dealing with the real problem. However, I do not agree on this matter. In some respects, the past is a heterogeneous and multivocal "thing" which we can never fully understand or reconstruct, but some aspects are more solid than others. As Pearce, among others, has noted, the temporal and spatial positions of artefacts and other traces of human social practices exclude some interpretations and support others (Pearce 1994:130). Archaeology is not simply about speculation and construction, although some aspects are more difficult to grasp than others. As Cornelius Castoriadis puts it, we can say something about certain aspects of *Other* cultures, past or present, but some areas are more problematic. For instance, Castoriadis denies the possibility of understanding the "affective vector"; we can learn a great deal about, for instance, the mystery cults of the Romans, but we can never understand how a Roman individual *felt* on entering the Eleusinian mystery cult (Castoriadis 1995:107).

Holtorf suggests that metaphor and metonymy, advocated by Tilley (1998; 2004), are better alternatives than to use simple analogies of practice. There is, however, difficult to establish a significant distinction between the two approaches. Metaphorical reasoning is, according to Tilley, "...to work from the known to

the unknown, to make connections between things so as to understand them. A metaphoric logic is an analogic logic serving to map one domain in terms of another" (Tilley 2004:23).

It may perhaps be a better choice to use metaphors, especially corporeal or bodily based ones, but it is hardly an alternative. Metaphors and metonymies are as general and mundane as any general analogy. There can hardly be anything substantial about metaphors than the simple fact that humans tend to think through them. Some strands of psycho-analysis make claims about "general" states and drives, but a closer reading of Freud or Lacan shows that the illusion of homogeneity in human mentality is more of a theoretical construction than what can be identified in actual practice (Fahlander 2001:85ff; 2003:24-30). The concepts of metaphor or metonymy are interesting and may prove usable: They are more consistent with the shift of focus from analogies of practices (processual) to analogies of the social and psychological dimensions (post-processual), but they are not substitutes for analogies.

Models and fictions in anthropology and archaeology

Over the years, archaeology has built up much of its "knowledge" of prehistory with the aid of cross-cultural analogies. The close bond to anthropology has created a varied yet consistent notion of typical, small-scale, socialities. The obvious problem is that some types of social practices and socialities are bound to have existed but are no longer represented in the present (cf. Freeman 1968:266; Fahlander 2003). Some of these extinct social practices are, however, likely to be found in the archaeological record and may have very little in common with contemporary social practices. For instance, various scholars have emphasised the great variety of issues such as the concepts of the self, sex, and nature (e.g. Foucault 1989; Laqueur 1990; Merchant 1980; McGrane 1989; Fahlander 2003). What we are dealing with in both anthropology and prehistory are variations of social practices, which have to be attended to in their own socio-

historical context. The central question is thus not an epistemological one about whether or not to employ analogies, but rather one manner in which we use our imagination and creativity and how to value our sources of information (cf. Tilley 1996:337f.).

It may seem that on a general philosophical level, analogies (or metaphors) are always involved in any interpretation of the world. This, however, may not necessarily be the case on a methodological level. In the same sense that our thoughts never totally are confined to language or a symbolic system, our analysis can involve *creative* elements. It may be in the form of unexpected or unorthodox combinations of information, or in rare cases, even something *"new"*. This creative ability can be found in writers of popular fiction, especially in science-fiction, which, at the best, actually expand our horizon to also embrace a greater variety of possible social practices. Such creative element may not be frequent in deductive comparisons of artefacts and their usage, but are needed in questions of social practice, economy, social organisation, social identities, ideology or cosmology. Thus, despite the often-repeated mantra, we do not necessarily need to use analogies!

I do not suggest that cross-cultural parallels are entirely without relevance. On the contrary, a wide range of ethnographical and social "knowledge" is likely to expand our horizons and to provide a better platform for interpreting material remains. This is, however, not enough, as we must expect to encounter "unknown" social practices and social forms with no correspondence with what we already "know". It is surely problematic to depart from what we "know", but it is nonetheless important to try. It is similar to the ontological insecurity that the trained scientist and special agent Dana Scully of the X-files experiences in her frequent contacts with the unknown. Her struggle to make sense of what she is trained to believe is repeatedly questioned by what she experiences in the field. In a monologue she complains: "How can I reconcile what I see with what I know?" That is something that we as archaeologist ought to question ourselves everyday, whether we are excavating or reading texts.

"How can I reconcile what I see with what I know?"

Conclusions

In this paper I have addressed some general problems in the traditional use of anthropological and ethnographical data:

- There have been social practices in the past that no longer are represented in historical or contemporary societies.

- Anthropological and ethnographical observations are *constructions* with both subjective and ideological bias. One cannot analyse social practice solely relying in oral information.

- No culture (social formation) is isolated or pristine. Individuals and groups have always been more or less involved in various regional or global interactions.

- One cannot compare apples to oranges. Issues of social identities, ideologies or cosmologies are too complex and varied and are thus beyond analogical inference.

We cannot assume that the similarities between different traditional peoples of today are due to universally human ways of organising their worlds. It rather seems more likely that such general similarities are a result of several thousands of years of homogenisation processes and regional interaction. From that perspective, the ethnographical record cannot have any supremacy over, e.g., Western social practices in our analysis of past social practices. It dissolves the disciplinary borders between archaeology, anthropology and sociology. In fact, it actually dissolves the border between social science and popular fiction. I have argued here and elsewhere (Fahlander 2001) that science fiction is an alternative as good as any, especially as it often deals with "unknown" social practices. To stick to the constructed ethnographical record or the illusion of the ethnic group leaning on ethnoarchaeological studies will only preserve a dull view of prehistory, not to mention its androcentric and Western, patronising implications. Gosden considers ethnoarchaeology as close to "immoral" (1999:9). The question is whether or not anthropology in the traditional sense is just as patronising.

The archaeological evidence does not speak for itself, either through metaphors or analogies. It is only through a discussion about how social practices relate to materialities that archaeology can maintain an important position within the social sciences. The archaeological record often contains more information than we generally use, and to find new and improved ways of extracting social information from such materialities is a most prominent task for 21st-century archaeology. After all, the uncertainty of prehistory is what makes our discipline exciting and meaningful.

However, to achieve such goals, archaeology must become an independent discipline and be able to contribute information that is not retrievable elsewhere. This is unfortunately not the situation today; archaeological knowledge is rarely addressed in social theory other than in crude, evolutionary and/or generalising manners. However, if successful, archaeology may enhance sociology, anthropology and even philosophy by providing the

social sciences with "independent" analysis of past social practices, fictions based on *particular* material evidence, rather than from direct analogies with contemporary data. In retrospect, it thus seems as if the close bond between anthropology and archaeology was a somewhat incestuous relationship, rather than a fruitful collaboration between two brothers in arms. Anthropology may, as Gosden claims, need archaeology, but archaeology does not necessarily need anthropology. Archaeology needs creative fictions, more complex models and social theories of practice that include the social dimensions of the material world.

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