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To hell with ethnoarchaeology!

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Abstract

This paper is a charge against ethnoarchaeology. Deliberately provocative, it aims at highlighting the flaws and ideological pitfalls of a sub-discipline whose actual contribution to archaeology remains hardly decipherable. Beside a methodological deficiency and the frequent narrow-mindedness of the research agenda, ethnoarchaeology suffers from two major problems. First, it carries implicitly an old evolutionary – and racist – ideology that divides the world between modern Western societies (inappropriate for ethnoarchaeological research) and premodern exotic societies (well fitted for ethnoarchaeological research). Second, the quest for universal models of interpretation leads ethnoarchaeologists to play down historical contingency and cultural specificities, which not only deprive them of a good understanding of the ethnographical contexts, but also contribute to restricting ethnoarchaeological applications to a mere quest for similarities (or dissimilarities) in the material record. The problems are too profound to justify an umpteenth salvage of the sub-discipline. Rather, the proposition is to get rid of ethnoarchaeology once and for all, and join forces with other, more serious, disciplines.

Keywords

ethnoarchaeology; archaeological method and theory; ideology; history; material culture

It must have started in Juan-les-Pins, at the 1991 Ethnoarchéologie: justifications, problèmes, limites international conference. The meeting was a major event, bringing together several big names of the ethnoarchaeological scene who had, for some at least, been associated with its early development. At the time, I was freshly engaged in Ph.D. research on pottery techniques in southern Cameroon, and just back from a second field season that had brought more confusion than the big breakthrough I was dreaming of. And there I was, eager to meet these famous people whose exciting prose had led me to launch into ethnoarchaeological research, but eager also to get a better grasp of the potentials offered by this sub-discipline, still often presented as 'emerging' despite its relative ancientness.

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For two days, most lecturers vibrantly and brilliantly championed ethnoarchaeology. From a theoretical point of view, however, many resorted to concepts – 'logicisms', 'middle-range theory' – that did not really appeal to me. Facing them was a handful of equally impassioned colleagues, who questioned the approach with arguments that I found no more appealing: 'destructured societies', 'weak homologies between the past and the present', 'lack of prior theoretical modelization'. All in all, discussions focused on question of packaging, on theories and concepts rather than content and context. Unsurprisingly, the only voices to be heard were those of the scholars engaged in the scientific debate. As for the people whose cultural practices were the alleged focus of the ethnoarchaeological approach, they remained desperately mute.

A growing irritation could also be perceived among some archaeologists, tired of being sermonized by colleagues who had not only stopped getting their hands dirty in field excavations or the laboratory, but had also developed this slightly condescending attitude of those who *have been* in Africa, Asia or Amazonia, and whose pathology is nicely described by Matthias Debureaux (2005) in *De l'art d'ennuyer en racontant ses voyages* (On the art of boring people in recounting your travels).

I think that I started to lose faith in ethnoarchaeology – and in my own research – at Juan-les-Pins.

A discipline in distress

The preceding text does not announce some sort of scientific memoir. My aim is simply to underline the subjective character of what follows. This paper is indeed a case *against* ethnoarchaeology, an approach from which I intellectually divorced years ago. Field experiences, questionings, exchanges with friends and colleagues, as well as readings, have led to a growing uneasiness in me about the ethnoarchaeological approach. For a long time, it was no more than a diffuse feeling, yet it tends now to objectivize and this is what this paper is about. If my contribution is not intended to be consensual, it meets the general conclusions of some other colleagues who, since the end of the 1990s, have urged ethnoarchaeologists to pay more attention to the methodological and ideological dimensions of their work (e.g. MacEachern 1996; Lane 2005; Wright 2002). Reading them and considering the case of ethnoarchaeology more attentively, one may even wonder if it is still worth flogging the proverbial dead horse. For ethnoarchaeology is not very well. Let us consider the book *Ethnoarchaeology in action* published by Nicholas David and the late Carol Kramer in 2001. In celebrating the work accomplished by two or three generations of scholars, who not only contributed to shaping the discipline but also helped it reach some sort of maturity, it seems to announce a promising future. In closing the book, however, one cannot help thinking of those CD compilations from ageing rockers, typically released at the end of a career or when sacked by their label. The times have changed, the excitement has dropped, the magic doesn't work any more.

What is discernible, in the case of ethnoarchaeology, is essentially the flaws of an approach whose actual contribution remains hardly decipherable. Take

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the American journal *Ethnoarchaeology*, for example, which was launched in 2009. Its editors state that

> [o]ne need that *Ethnoarchaeology* addresses is that there is little that unifies or defines our sub-discipline, although there has been an exponential increase in ethnoarchaeological and experimental research in the past thirty years. With such growth we must explore what distinguishes these approaches as a sub-discipline, what methods connect practitioners, and what unique suite of research attributes we contribute to the better understanding of the human condition.

So, after half a century of research and publications, here is a 'sub-discipline' whose practitioners are still clueless about their specific research methods and endeavours. How less scientifically decipherable can that be?

Moreover, if the popularity of a scientific approach is correlated to its position in academic programmes, there is no denying that the ethnoarchaeology star has set. With the notable exception of some North American universities, ethnoarchaeology is very sporadically taught, generally in connection with a single individual or a specific research programme, and it is failing to gather any momentum – still.

Ethnoarchaeology indeed seems in distress. Two attitudes may, then, be adopted: the first may be to attempt an umpteenth salvage, in redefining its aims, its domains of applicability and its methods. This has notably been done by Valentine Roux (2007) or Jeremy Cunningham (2009), among others. But the second attitude is to give the wreck one last kick to sink it once and for all. As readers must already have guessed, it is a kick that I intend to give.

Analogies without borders

As all discussions about ethnoarchaeology remind us, the use of ethnographical analogies is as old as the archaeological discipline. It is also intrinsically linked to its development. Interpreting a chipped stone as a human tool used for cutting something is making an analogy. Exploiting information collected in 'real-life' contexts is in fact an imperative for archaeologists: after all, who among us has practical experience of hunting, fishing, tending fields, building houses or making things – all activities that lie at the heart of our daily investigations? As vital as they may be, however, the selection and use of analogies is far from evident and may lead to reasoning as flawed as those they are supposed to improve. To come back to the analogy of the stone knife, François Sigaut (1991) offered a good case in point in demonstrating that 'a knife is not used *for* cutting, but *in* cutting', and hence that most phylogenic classifications or functional interpretations of cutting tools were intrinsically wrong (see also Veyrat, Blanco and Trompette 2007). Saying that a chipped stone resembles a 'knife' and was used for 'cutting something' is thus as useless as saying that it is an 'artefact', for it eludes a much more pertinent question: how the object was used in practice.

Ethnoarchaeology developed as an effort to rationalize and systematize the use of analogies. In the positivist context of the 1960s, the idea was to turn archaeology into a proper 'science', along the lines of natural sciences. What was lacking until then was the equivalent of a laboratory

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in which to test hypotheses about the material record, and such a laboratory existed supposedly in the ethnographical context. The rise of non-Western archaeologies dates from the same period. In Africa, Oceania or South America, archaeologists were increasingly confronted with people whose ways of life seemed very remote from those of Western societies, and, most believed, closer to those of pre- or protohistoric people. Among archaeologists, the interest in 'exotic' people therefore grew and systematized during the 1960s (David and Kramer 2001, 14–31; Lane 2005; Sadr *et al.* 2006).

That the first tentative steps of ethnoarchaeology were associated with questionable results is not surprising. After all, failure is a crucial part of scientific reasoning and does not preclude the subsequent soundness or robustness of a discipline. What I want to pinpoint here in relation to these early scholarly ethnoarchaeological contributions is their guiding principle, for it reveals a disturbing conception of human history. Even more disturbingly, this conception has endured through decades, without much questioning.

What a wonderful world

In the introduction of a collective publication synthesizing research made during the 1970s and 1980s, William Longacre – one of the founding fathers of ethnoarchaeology, and a participant at the Juan-les-Pins conference – wrote that 'the most fundamental aspect of designing ethnoarchaeological research is the selection of a society to study. That selection must be guided by the nature of the problem or set of problems the archaeologist wishes to investigate. Obviously, the investigator must determine the most appropriate society with which to work' (Longacre 1991, 5).

But what will a Western archaeologist likely consider the *most appropriate society* for designing an ethnoarchaeological project? Above all, a group of people whose activities and material culture can easily be compared to that of archaeological contexts. It is self-evident, and it is why, for instance, I started to work with potters rather than car mechanics or computer engineers. But one will also seek societies whose *image* fits with common representations of past societies. It is here that the ethnoarchaeological project faces a terrible pitfall, namely the ideology that emerged in the age of Enlightenment (Latour 1993) and was considerably reinforced by colonial ethnography. In its evolutionary incarnation (the most explicit), such ideology views modern Western societies as the outcome of a historical process whose earlier stages are still experienced by exotic people. In the early 20th century, when evolutionism dominated the anthropological agenda, this conception of history proved especially appealing for archaeologists. Here is, for example, how a pioneer of what would later become 'ceramic ethnoarchaeology' envisioned the link between ethnography and archaeology:

> The antique vessels that we find buried in the ground, in tombs, in the ruins of lost cities, often keep a part of their secrets and bring about hypotheses that often prove dangerous. We thus need to know the value of such hypotheses, and this is where ethnography is liable to help archaeology.

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> Indeed, there are people who remain, up to now, in a primitive state and still untouched by our technology. It is among such people that we will have the opportunity to grasp many of the secrets concealed by antique ceramics, for it seems absolutely sure that all people have passed through the same stages as regards the progression of intelligence and realizations of the mind (Franchet 1911, 1–2, my translation).

When ethnoarchaeology emerged half a century later as an archaeological sub-discipline, such conceptions had been officially abandoned by social scientists. Decolonization had taken place or was under way, and the evolutionist paradigm had been relegated to a distant – albeit distressing – past. But its underlying ideology continued to haunt Western thought, perhaps because it constitutes one of the founding elements of our relationships with the rest of the world. Thus when the first ethnoarchaeologists sought appropriate contexts for testing hypotheses about pre- and protohistorical behaviour, they logically fixed their choice on areas where one still found supposedly 'premodern' peoples. Unsurprisingly, hunter-gatherers – viewed by many a scholar as living relics from the Stone Age – were the first to receive the visits of ethnoarchaeologists.

Their status as 'living fossil' remained largely unquestioned. Why bother anyway? Among French prehistorians, for example, 'Bushmen' and 'Hottentots' had been part of the conceptual agenda since the end of the 19th century (Sadr *et al*. 2006). And in 1971 the world was suddenly confronted with an even more primitive people – the 'gentle Tasaday' of the southern Philippines – who could not make fire, and who used stone tools and lived in caverns as our distant ancestors had (Nance 1975). In this case, the slowness with which the scientific community finally came to admit that these 'Stone Age cavemen' were, in all likelihood, the invention of a local billionaire and of a handful of more or less credulous journalists (Headland 1992) tells a lot about the pervasive belief in the existence of 'living fossils'.

On such a basis, everything became possible. The San of the Kalahari, for instance, became living references for archaeologists struggling with the interpretation of sites frequented by early humans of the Ice Age. It did not matter that these contemporary hunter-gatherers lived in open compounds in a hot and arid environment, or that they were citizens of a nation state and engaged in a capitalist economy (Lane 2006): regularities were observed and transferred, without much afterthought, to archaeology.

Yet, as underlined by Scott MacEachern (1996), the San of the Kalahari only constitute one among many other ethnographic possibilities as regards hunter-gatherers. In West Africa, for example, there exist endogamous subgroups of hunters associated with highly hierarchical societies. But none of them has ever been visited by an ethnoarchaeologist, probably because they do not correspond to our preconceived image of prehistoric people. As for South African 'Bushmen', one can hardly continue viewing them as 'living fossils' when considering their history. They correspond indeed to a series of blurred social entities, whose economy seems to have shifted between production and predation for centuries – perhaps more than a millennium – and whose members entertained long-term relationships with agriculturalists

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and pastoralists (Sadr *et al*. 2006). This picture is quite similar to that currently reached for Central African 'Pygmy' populations (e.g. Joiris 2003; Klieman 2003).

Such 'Stone Age ethnoarchaeology' has proven incapable of improving archaeological reasoning. But it contributed to strengthening archaeologists' preconceptions about the past and the world in general. Of course, researches did not remain confined to hunter-gatherers. From the 1980s onward, the focus shifted increasingly to 'preindustrial' agriculturalists and pastoralists (David and Kramer 2001, 22–31). What remained unchanged, however, was the overall willingness to play down historical contingency – Roux (2007) symptomatically describes ethnoarcheology as 'a non-historical science of reference' – and to mask cultural specificities; in other words, to purify the ethnographic context until it was possible to isolate some behavioural components, whose 'functioning laws' had then to be sought. Such laws must, of course, have a universal range of application; if not, how to ensure their transfer to archaeology? As explicitly summarized by Alain Gallay:

> either we admit that ethnological studies may be useful for archaeologists, which means that an observation made at a point *X* in space and time is equally valuable for a point *Y* … and in such case this approach is transcultural … or we admit that such transfer cannot be made, due to the endless originality of cultures. We must then confine ourselves to case studies; the confrontation between ethnology and archaeology is not possible any more, and the ethnoarchaeological approach must be abandoned (Gallay 1990, 293, my translation).

Yet the extent and quality of field research made by Gallay and members of his team led to the adoption of a third perspective – direct historical analogy (or the 'direct historical approach'; Stahl 1993) – where the transfer is limited to archaeological contexts attesting to a historical and cultural continuity with the present (Gallay *et al*. 1990; Gallay, Huysecom and Mayor 1998; Mayor 2010; Mayor *et al*. 2005). This theoretical back-pedalling is also observed when Nicholas David admits, with regard to his quest for a 'theory of style', that he 'no longer expect[s] to be able to generate any generally applicable predictive theory' (David 1998, 7). Rather than taking a long and fruitless detour through ethnoarchaeological modelling, why not immediately launch into historical work? While such work also imposes a careful assessment of how to bridge archaeology and ethnography, it may at last guard against the foolish quest for universal laws.

What is so wrong about methods?

In his satirical book *Bluff your way in archaeology*, Paul Bahn describes ethnoarchaeology as

> picking on some unsuspecting group of people (hunter-gatherers, simple villagers, sheep farmers, etc.) – preferably in the third world or Alaska. You then go and live among them for a while, taking note of how and when they make and use things, and how and when they break and discard them …

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> After figuring out what you think is going on with the use and discard of objects (you should never stay around long enough to master the language) you return to your desk and use these brief studies to make sweeping generalisations about what people in the past and in totally different environments must have done (Bahn 1989, 52–53).

We already know about the obsession with exotic societies, but Bahn also bangs on the nose when pointing out the superficiality of hasty enquiries, made with the help of interpreters. Indeed, sophistication, reflection and deepening are not ethnoarchaeology's forte when it comes to fieldwork. Look at ethnoarchaeological publications – starting with my own: no discussion about field practices, no comment about data collection or the nature of the data collected. In the best cases, field methods reveal themselves bit by bit throughout the text (Roux 1990 is a notable exception), consisting in a heterogeneous assemblage of census forms, lists of common-sense questions – more or less induced, more or less contextualized – quantitative surveys, and descriptions of technical activities. The latter constitute the most obvious contribution of ethnoarchaeological studies so far. Yet while the description and conceptualization of technical activities have been considerably debated (and improved) since the 1970s – especially in France (e.g. Balfet 1991; Bartholeyns, Gogoroff and Joulian 2010; Lemonnier 1992) – such aspects have, so far, received sparse attention from ethnoarchaeologists.

The book *Ethnoarchaeology in action* (David and Kramer 2001) is a good case in point. While it does include a chapter entitled 'Fieldwork and ethics', the text mainly illustrates a profound lack of interest in field methods. The practical elements discussed come essentially from David's field experience: as informative as they may be for neophytes, they remain trivial as regards the methods used by anthropologists or sociologists.

I see at last three reasons for such a methodological deficiency. The first pertains to the academic training of archaeologists who launch into ethnography: they get no (or only a few) courses devoted to field enquiries, and methodological issues are seldom discussed in the few anthropological classes that figure in their degree course. The second reason ensues from the ethnoarchaeological approach itself. Since research projects focus usually on a narrow problematic, or on hypotheses that must be fast-tested in the field, researchers are compelled to take shortcuts and to avoid burdening themselves with nuances, contradictions and all other sociological 'background noise'. The third reason is more insidious. It results from a pervasive conception of 'traditional' societies, within which social ruses, individual strategies and multiple levels of meaning – so typical of modern Western societies – are thought to be insignificant or even nonexistent. While we Westerners are trained to question any opinion poll made in front of a supermarket, it seems that individual testimonies may be accepted as gospel and extended to a whole society when collected in exotic contexts. There are, of course, exceptions to this depressing picture, but they concern, symptomatically, long-term research programmes whose questioning has broadened and matured through the years (e.g. Bowser 2000; Bowser and Patton 2008; David 1992; Kramer 1997; Pétrequin and Pétrequin 2006).

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Magical thought and pragmatism play hide and seek

There is another trap resulting from the ongoing ideological opposition between Western and non-Western societies. Briefly said, members of Western societies would be prominently reflexive and engaged in endless processes of singling out and of identity reinvention (Giddens 1991; Kauffman 2004). Western thought would also be characterized by rationality – in science as in the economy – with a tendency to materialism and a correlated loss of social ties and cultural values. By contrast, members of non-Western societies would be governed by tradition. Driven by an 'infra-conscious social memory' (Kauffman 2004, 109), they would perpetuate collective identities and have difficulty conceptualizing their actions outside the socio-religious structures from which they arose. Their rationality, in other words, would be 'less rational' than ours, but would offer more room for social ties and cultural values.

Unwittingly, studies devoted to non-Western technologies tend to reify this big ideological division. Keen to illustrate the social and symbolic importance of daily practice and to transcend materialist interpretations, many – including me (Gosselain 1999) – depict them in static and unbalanced terms, and put the emphasis on logics that seem to loom over agents and society. Making, exchanging and discarding things in 'traditional societies' would involve an ongoing, harmonious process of interweaving social relationships, world views and ways of doing. Some go as far as proclaiming that production techniques 'are social rather than material necessities' in such contexts (Pinçon 1999, 4). While Western artisans are endlessly seeking profit and competing with each other, their non-Western counterparts would thus spend their time praying and socializing.

The historian Marcia Wright (2002) has denounced this Manichean conception of technology. Reconstructing the life trajectory of a Tanzanian master smelter, she showed that the stability and importance of rituals in iron smelting might have been overemphasized by ethnoarchaeologists. Indeed, post-1950 reconstitutions were decontextualized performances, achieved mainly by individuals whose role in the activity had formerly been peripheral. They consequently placed processes and rituals at the 'heart' of the event, for such components of the technique were those that they could more easily single out and analyse 'scientifically'. The biography of the master smelter reveals, on the contrary, that symbolic prescriptions were easily downplayed in the normal course of activities, for instance when faced with an economic challenge such as an increase in regional demand for iron tools (e.g. the smelting took place within the village, it involved the participation of male and female individuals unrelated to the craft, and so on). Neither sticking to 'tradition' nor fully embracing colonial practice, this master smelter was simply adapting to changing circumstances, without compromising his craft, identity or social position.

The symbolic and cosmological orthodoxy depicted in local ethno-archaeological studies (Barndon 1996; Schmidt 1997), and the presumed historical continuity of technical practices, are thus belied by Wright's study. Clearly, religious preoccupations did not supersede economic ones, or hamper adaptations to changing circumstances.

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A history without history

While some aspects of Marcia Wright's study may be questioned, she is definitely right in pointing to the problems caused by too strict a focus on symbols and rituals in the study of non-Western technologies. Giving symbolic systems such a prominent position, and considering that these systems remained largely unchanged through time (e.g. Schmidt 1997), simply denies the historical dimension of technical practices and, by extension, the dynamics of change experienced by *all* societies. Taking another, more insidious, road, ethnoarchaeology ends up reinventing Hegel's philosophy of history.

This relegation of non-Western societies outside history is well known in anthropology, and has been notably denounced with regard to structuralism (but see de Heusch 1993). However, that diachrony specialists such as archaeologists could shoulder it without apparent afterthought is quite baffling. One explanation, which brings us back to the founding paradigms of ethnoarchaeology, is that *formal continuities* in assemblages procure a false impression of *historical continuity*.

Paul Lane (2005) developed this point in detail when discussing the role of 'ethnographical imaginations' in African archaeology. Taking, among other examples, the famous 'Central Cattle Pattern' (CCP) in southern Africa, he showed how Tom Huffman (2000) had attempted to reconstruct the social organization, ideology and world views of protohistorical and historical populations from the 6th to 20th centuries with the help of local ethnographical sources. The parallel between archaeological and ethnographical contexts was plain for Huffman, since many scholars consider that southern African extant populations share a common history that could go back in time for at least a millennium. Regional ethnographies revealed the existence of a settlement organization centred on cattle, with a series of structural oppositions: centre/periphery, men/women, cattle/grain, senior/junior, front/back, up/down (note, in passing, that such basic oppositions apply to a large number of peasant settlements). Having found a similar spatial layout in archaeological contexts, Huffman concluded that the social organization and world view of those who built and used these structures were similar to what had been documented among early 20th-century populations (Lane 2005, 31). Such reasoning is, of course, circular, since it rests on the belief that there exists a historical continuity between protohistorical, historical and ethnographic contexts. But in considering the problem carefully, Lane observed numerous regional variants in the CCP, to which archaeological structures are only loosely related. Carried away by his interest in the symbolic dimension of spatial organization, he argues, Huffman has overlooked any meaningful chronotypological variations.

Transcending these false impressions of historical stagnation demands that we go beyond mere similarities in the material record, and hence broaden the scope of analyses. This means taking *every* element of the context into consideration. Such an enlarged perspective is nothing less than an imperative. Indeed, many material-culture studies show how formal resemblances in objects, or even borrowing processes, seldom lead to a

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duplication of conditions of use or symbolic representation (e.g. Bonnot 2002; Thomas 1991; Zeebroek, Gosselain and Decroly 2008). Rather than seeking interpretation clues in ethnographic contexts, every effort should be aimed at developing a proper 'ethnography' of archaeological contexts, from which interpretations may then subsequently be built.

Let's get rid of it!

As announced, what I have done here is to present a charge against ethnoarchaeology. However, before resting my case, I would like to point out some of its benefits. This is not in order to rehabilitate myself but, quite the opposite, to underline once again how what can be gained from so-called 'ethnoarchaeological studies' actually derives from theoretical and conceptual issues generated *outside* this sub-discipline.

Let us first consider the numerous case studies devoted to production techniques and material culture. From the 1970s onward, ethnoarchaeology has contributed to the development of a huge body of data, one all the more important since enquiries were often made among ageing native specialists whose very existence, knowledge and technical practices had generally been skipped over by anthropologists. If carefully manipulated, such data are of irreplaceable value for comparative studies. Yet their quality is often inversely proportional to the theoretical aspirations of those who collected them: scholars who placed all their efforts in the development of interpretation models left us with hardly exploitable documents. A strict adherence to the ethnoarchaeological agenda would thus seem to be counterproductive in that regard.

The development of reference collections based on field measurements and samples (tools, materials, finished products, etc.) is another notable contribution of ethnoarchaeological studies, for such collections may contribute significantly to the archaeological reconstruction of technical processes. However, ethnoarchaeology overlaps here with another sub-discipline – experimental archaeology – whose practitioners never pretended to seek 'laws of human behaviour', but rather to reconstruct processes of site formation and artefact manufacture. Ethnoarchaeologists may admittedly have a slight advantage over experimental archaeologist, since they work with professionals, and thus are (theoretically) preserved from naive approximations. Yet the reference collections they have generated remain purely behavioural and factual; they cannot be exploited for other purposes – i.e. building social theories. Such kinds of ethnoarchaeological approach thus belong more appropriately to the field of 'experimental studies', which already witnesses fructuous collaborations with other 'laboratory disciplines' such as archaeometry or experimental psychology (e.g. Livingstone Smith 2001; Roux, Bril and Dietrich 1995).

Finally, a series of ethnoarchaeological studies has developed a 'direct historical approach', in which modern traditions are used to interpret historically related archaeological remains. While the sheer diversity of these works opposes any wholesale presentation, their general purpose places them in the scope of other historical approaches – history, archaeology, historical linguistics, art history and historical anthropology. As illustrated by

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Stahl (1993; 2013), researchers who engage in direct historical approaches will consequently benefit from looking *outside* their disciplinary niche and developing relationships with other historical sub-disciplines.

To sum up, ethnoarchaeology is never as interesting as when it departs – deliberately or not – from the theoretical and ideological agenda upon which it was built; when, in other words, it mutates into a proper science of techniques and material culture, and engages with other disciplines. Systematizing and sustaining such a mutation would necessitate a complete reformulation of the goals, methods and philosophy of ethnoarchaeology. The task is colossal. But is it worth the trouble? Why should archaeologists strive to reinvent scientific fields that already exist? Material-culture studies and cultural technology have been developing for as long as ethnoarchaeology. While not devoid of theoretical sideslips, both domains currently benefit from some robust methodological advances that bowl over many ethnoarchaeological contributions. Would it not be more productive to join forces, and to bring to material-culture and technological studies the rigour or the historical dimension that they sometimes lack? (See, for example, the criticisms made by Ingold (2007) about how the properties of materials were often ignored by material-culture studies or so-called 'materiality' theories).

What should then be done with ethnoarchaeology? As for me, the decision has already been taken. For others? Well, think carefully!

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