

limits of a discipline. Indeed it can be the case that sub-tribal boundaries *within* disciplines are more of an obstacle than those *between* disciplines (Jones 2004), and we may find at times that we have more in common with researchers in other disciplines than we do with those in our own.

### **Conclusion: some practical consequences of tribalism**

Despite my reservations about their paper, I should congratulate Garrow and Shove for venturing into the murky waters of interdisciplinary work. It just seems to me that they have misinterpreted the nature of the problem. In reality it is not any form of intellectual boundary that confronts us, but rather a system of social, tribal boundaries. As such the real problems are mostly practical. Most higher-education institutions retain a traditional departmental structure, albeit that one or two (e.g. Sussex University and the University of Linköping) have experimented with interdisciplinary teaching structures. These department-based structures necessarily mean that funding interdisciplinary work is always a problem since it will be judged peripheral or liminal. Similarly, interdisciplinary work can be considered of lower prestige than getting to grips with the 'core issues' of a given discipline. The academic struggle often seems to resemble natural selection, where the fiercest competition is between conspecifics, rather than between different species.

This pattern is naturally reflected in publication. Many if not most journals will tend to be discipline-specific, with those that accept interdisciplinary contributions being in the minority. Equally, anyone attempting to publish articles or books which break new ground outside of traditional academic territories may well fall foul of the conservatism of peer reviewers. Indeed, by its very nature, any interdisciplinary project will find it hard to identify appropriate peers.

Given these practical obstacles, one might be led to despair, yet I think that in the specific case of material-culture studies the very range of disciplines that are involved in the subject holds some basis for optimism (see e.g. Brown 2004; de Léon 2001; Gracyk 1996; Jones 2004). There must be some powerful reasons why material culture attracts such a diversity of scholarship. The real question is how we go about sharing this increasingly crowded piece of academic territory.

### **Acknowledgement**

I would like to thank Alan Costall for some suggestions regarding this commentary.

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### **Display matters** *Harvey Molotch*

Garrow and Shove use objects to reveal larger social and cultural patterns, in this case – most ambitiously – epistemological and methodological practices

of their respective disciplines. I think it works and I agree with their support for future interdisciplinary object-swapping.

Speaking as a sociologist, I believe there is an asymmetry in the two disciplines' use of objects that the commentaries elide. While Elizabeth's own sociological work does indeed concentrate on artefacts, she is an exception. She has allies and appropriately cites a number of the leaders. But the vast sociological research enterprise treats objects, from the built form of the contemporary city to small-scale elements like the toothbrush, as incidentals. In most sociological work material elements arise, almost parenthetically, in stories about the important matters like social status, demographic change or neighbourhood organization. As silly as it may seem to readers of this journal, the artefacts that could so enrich sociological understandings or lead to new ones remain largely unexamined. They are still, in Latour's term, the 'massing masses' (Latour 1992, 225).

The major exception in the heritage of sociology was the Frankfurt School and modes of analysis derived from it. But commentators from this tradition treat material goods as social bads, embedding artefacts into the larger theoretic concerns of neo-Marxist critique. Under the aegis of the inter-war intellectuals, many of whom emigrated to the US and UK, where their influence spread across the social sciences, consumption was part of the capitalist plot. Capitalism deploys consumerism to render workers quiescent. This helped explain the lack of social revolt anticipated by Marx as well as, in a related vein of analysis, psychological ennui and alienation among the modern populace. This overarching product disapproval long inhibited the kind of strategic use of goods exhibited in both Duncan and Elizabeth's approach to their respective objects.

Even without the Frankfurt dystopia, there are still, the experiment tells us, disciplinary lenses (and blinders) that shape inquiry. Sociologists of occupation have long documented the distinctive way people in particular trades apprehend their worlds (Hughes 1984). It is an elaboration, in social and practical terms, of the Whorfian pragmatics of cognition. The Eskimo sees variants of snow; the carpenter takes in (and inquires about) lumber and joins. Archaeologists and sociologists will indeed see objects differently and lose the trail of inquiry at different points or in different ways. I was struck by the archaeologist's attention to concrete detail: precise measurement and characterization ('eleven rows of bristles'; p. 122). I take this to result from the frequent need to make comparisons among similar but not necessarily identical objects, something that sociologists have seldom cause to do. Sociologists might do it with types of juvenile delinquents, but not with types of objects. When the sociologists do follow-up from the object, they look for contemporary settings where the object exists in practice. Without such access, Elizabeth and her visitors feel frustration. They lack the skills of the archaeologist more accustomed to imputing the social from the details of the physical.

On the other hand, the standardized good seems to stymie the archaeologist; it does not enlist the skill set of identifying, classifying and comparing. Too many toothbrushes are just the same, pre-coded by brand, model number and even batch (the likely source of the numerical marking noticed by Duncan, but sociologically irrelevant). This makes it an intellectual ready-made that

can be a basis for theorizing (as the Frankfurt people do), for observation in use or production, or for eliciting responses in survey-like methods. Elizabeth does fine with mass manufactured goods (as her excellent corpus of work testifies), even trying to suck the Habitat tea towel into her line of analysis.

Garrow and Shove (and now I return to last names to shift to their professional roles) merge their two disciplines for purposes of experiment, displaying a common bent rather than a distinguishing one. They share in a creative and somewhat risky empiricism, the mark of both disciplines at their best. In that spirit, I would like to add a dimension that seems relevant to both of their experimental settings: the displays in their two offices. All objects are apprehended within a particular micro-context of other elements. Elizabeth refers to the tea towel of the rock and Duncan to the desk upon which the toothbrush sits. I would make more of this.

Both the rock and the toothbrush are looted, in the sense of being taken away, for the purpose of an experiment, from their rightful places. The rock-cum-axe belongs in a museum or among the archaeologists; the toothbrush belongs on a shop shelf (and in a clamshell package), or a bathroom, or among product-oriented sociologists like Elizabeth. The precise conditions of their mis-placement affect how their strangeness operates.

The tea towel marks the rock as something other than a 'real' rock. Real rocks live outside or under special indoor conditions, like a little landscape at the base of house plants or somebody's Zen garden. The towel tacitly disrupts and signals 'none of the above'. This helps the rock generate some curiosity from Elizabeth's office visitors, even as they are thwarted in further questioning. Like the indigenous artefact moved into a museum vitrine, an object changes meaning with its relocation to a new setting and the kind of context its custodians provide (Kirshenblatt-Gimblett, 1998). That Elizabeth retained the conveyance cloth provided by Duncan affected the object's meaning even if it failed to deliver an axe head.

Now for the toothbrush on Duncan's desk. Where is *its* tea towel? It needs something – a museum mount, perhaps – to render it socially parallel to Elizabeth's rock. Why do people not ask about it who visit Duncan? I do not think it is just because it is so common an item. First, the little dial on the bottom makes it unusual, worthy of some good questions, as indeed Elizabeth's commentary helps to answer. People do remark on one another's consumer goods all the time, especially when they are distinctive in some way. But they avoid commenting on personal items, especially when polluted or polluting. A toothbrush on a desk is not just out of place; it is in a morally wrong place. Something on a desk can be touched, can make contact with dirt and germs. If that is the way Duncan wants to live, let him carry on but there is taboo against bringing attention to such suspect ways.

So I am saying that the experiment, inventive and rich as it was, had a design flaw. The differential response of the office visitors in the two 'treatments' may be due to more than disciplinary paradigm. Some of the variation may come from the physical specifics of exhibition. Materiality – the force that united the two scholars in the first place – may help explain the experimental results.

At a surface level, it looks like archaeologists do not ask, at least not about toothbrushes, and that sociologists do not look, at least not at rocks. But

as the commentaries elaborate, caution is in order on both fronts. Even if most sociologists really do not look much, those of Elizabethan wives are of a different sort. And archaeologists do ask a lot of questions about social and cultural context, at least when on more familiar product turf. More ambitiously perhaps than envisioned by the experimenters, the next steps are to fuse the skills and propensities to further elaborate how materiality operates *in situ* and how objects can teach about society.

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## **Artefacts between disciplines. Response to responses**

*Duncan Garrow and Elizabeth Shove*

As is often the way, a ‘finished’ article disguises aspects of its own production and gives an overly orderly account of theoretical and methodological moves which were not especially planned. In responding to our commentators and in coming clean about our own working methods we have some confessions to make. In making these admissions we<sup>1</sup> continue what has by now become a conversation about matters of interdisciplinarity and about our approach to the axe and the toothbrush.

The axe–toothbrush story developed from a casual exchange between two strangers at a conference dinner table – the conference, Connected Space, was designed to promote ‘interdisciplinarity’ but not in any especially organized way. That an article emerged from this accidental encounter is itself a sign of the serendipity of academic life and of the common and distinctive histories and traditions on which we draw. Despite being strangers, we both belonged to an academic community defined by common practices like going to conferences, writing articles and responding to comments. In recognizing this point, we also recognize that it is only because we are the academic equivalent of Eskimos that we are willing and able to make distinctions between the many different kinds of paradigmatic, theoretical and methodological snow. Without shared points of reference and a relatively common language it would have been impossible to proceed, or to develop a method of elaborating on our differences.

This is not in itself news, so what were we really trying to do in relation to existing debates and literature on the disciplinary structure of academic enquiry? The honest answer is, not much. Despite some of the rather grand claims we make in the opening section of our article (confession no. 1: consistent with the conventions of academic writing, we use references to ‘interdisciplinarity’ to contextualize, and perhaps overplay, the significance of our work), our ‘real’ point of entry, and in a way our real focus, was to take seriously Hanson’s (1981) argument that observation is theory-laden. With this in view, we wanted to reverse-engineer our different accounts of things in order to ‘see’ the tacit framing and theoretical loading of research in