

CHAPTER 1

Assembling Science at Çatalhöyük

Interdisciplinarity in Theory and Practice

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INTRODUCTION

Within archaeology, the term ‘assemblage’ has a long and central history, though it has perhaps not been theorized as much as other terms. The notion that artefacts are associated together in assemblages within contexts has always been the key that separates archaeology from antiquarianism. If the associations of traits in assemblages are recurring, archaeologists are able to identify cultures, time horizons, elite and non-elite graves, functional tool kits, and so on. The underlying idea is that an artefact found with other artefacts within an assemblage can be interpreted in terms of these other artefacts, and vice versa. Assemblage is thus a building block of archaeological method and theory that allows us to gauge the date, function, type, meaning of objects. But this building block is relational and contextual; relational because one find is interpreted in terms of others, and contextual because the specific set of associations can be related to stratigraphic and spatial information beyond the assemblage itself.

Without assemblages archaeologists would not be able to work out the environment of a site, its economy, or social organization, they would not be able to date many contexts or understand the relationships between sites. Without context and assemblage, there is little to archaeology beyond collecting objects. But there are problems in the definition and interpretation of assemblages (Binford, 1982; LaMotta & Schiffer, 1999; Bailey, 2007; Lucas, 2008). When does a cluster of artefacts become an assemblage? What is the relationship between palimpsest and assemblage? Do we find assemblages or do we actively construct or assemble them? And are clusters of artefacts intentional associations or unintentional relations produced by depositional or post-depositional processes? And if intentional, what types of intention (conscious or non-discursive etc.) are involved? And who made the association; for example, are the associated artefacts in a grave the assemblage of the deceased or of the living? So, in archaeology, the notion of assemblage raises questions about the processes of assembling. An assemblage is not self-evident.

It is perhaps unfortunate then that the term has been so little theorized in archaeology (see, however, the online Sheffield graduate journal of archaeology called ‘Assemblage’). In contemporary social theory, on the other hand, there is an active and important discussion of assemblage. This theoretical debate deals less with the associations of past artefacts in contexts and more with the production of knowledge—that is with the ways that statements are based on assembling bits of information from divergent sources. It is primarily in this sense that the term is used here, though clearly there is a connection between how archaeologists assemble arguments and how past social actors constructed assemblages. Taylor (1948) argued for a conjunctive approach and I have argued for a contextual approach (1986); in both cases interpretations are based on associations of objects in past assemblages and contexts. But how exactly are theoretical arguments based on these contextual associations? I have argued that archaeologists follow a hermeneutic approach (Hodder, 1999) while Wylie (1989) has argued for a tacking to and fro between different types of data in order to build arguments.

The twenty years of research conducted by the current project at Çatalhöyük allow investigations into how archaeologists assemble arguments by moving between different types of data. Can current social theories about assemblage contribute to an understanding of the archaeological process? Whether it is the work of Latour (2005) on ‘Re-assembling the Social’ or the ideas of Deleuze and Guattari (Deleuze, 2004) and their influence on DeLanda (2006) and Bennett (2009), does the social theoretical discussion of assemblage throw light on the Çatalhöyük research experience?

What are the inflections of meaning that are given to ‘assemblage’ in this social theoretical debate? According to DeLanda (2006), assemblages refer to heterogeneous entities that are not holistic. Assemblages come about historically and have both stabilizing and destabilizing components (that he calls territorialization and deterritorialization). The focus in DeLanda’s assemblage theory is not on essential

categories like city or government or person, but on their emergence in specific historical circumstances and on their maintenance. For Marcus & Saka (2006: 101), ‘assemblage ... permits the researcher to speak of emergence, heterogeneity, the decentred and the ephemeral in nonetheless ordered social life’. The components of assemblage described by Bennett (2005) are as follows. Assemblage is (1) an ad hoc grouping that comes about historically. (2) Its coherence co-exists with internal counter energies. (3) Assemblage is a web that is uneven and power is differentially distributed. (4) It is not governed by a central power. (5) Assemblage is heterogeneous, made up of different types of actants, human and non-human.

ASSEMBLING ÇATALHÖYÜK

To explore whether these notions of assemblage apply to the research conducted at Çatalhöyük, the project’s working practices need to be explained (Hodder, 2000). As in any large archaeological project, there are a lot of different specialisms. There are one hundred and sixty people currently working on the team—dividable into excavation teams and pods, and there are laboratories in which thirty-six specialisms work (listed in Figure 1). The team members in these different specialisms are brought into conjunction through working together on site, through the ‘priority tours’ where lab members choose priority units together with the excavation pods every second day, through use of a common data base, through writing together in themed volumes, through social events and venues on site, and in some cases through reading each other’s online diaries etc. Within these interactions there are lots of tensions. For example, a major

tension has been described elsewhere (Hamilton, 2000) between excavators and lab teams. And there are also fault lines between those specialists more based in the natural sciences and those more engaged in cultural data—I have described elsewhere the ways these different specialisms work (Hodder, 1999).

While I as Director make decisions about team membership, and have made major changes to the team on two occasions over the twenty years of the project, and while some will argue that I am a tyrannical and despotic director, the overall research structure is in my view quite flat. There are overall research questions—such as the overarching statement that the project aims to place the art and symbolism within its full environmental, economic, and social context. There has been an overall shift through time from the study of individual houses and depositional processes to the study of the settlement’s social geography. But I as Director play a small or remote part in many research groupings, and a wide range of specific questions have also been asked by different team members, often related to the different profiles and interests of funding bodies. Figure 2 shows the main research interests of different funding bodies that have supported the project over recent years. The research goals do not coincide. By working with these different funding bodies, team members have been pulled in different directions. So, for example, the Templeton Foundation that focuses on religion has drawn in Lynn Meskell and myself on symbolism, Carrie Nakamura on placed deposits, and Lori Hager on the interpretation of a particular burial. Funding from the Thiel Foundation and Imitatio focuses on the relationships between real and symbolic violence and has drawn in bioarchaeologist Chris Knusel regarding evidence of violence on human bodies, groundstone specialist Christina Tsoraki to explore the role of mace heads, and the chipped stone team regarding the function of bifacially flaked points and daggers. National Science Foundation funding was obtained by Kathy Twiss and Amy Bogaard for faunal and botanical studies relating to the question of economic integration and cultural survival at Çatalhöyük. Another group has written about the issue of burning in B52 and whether the fire was caused intentionally or was an accident—this issue has brought in Kathy Twiss and Nerissa Russell from faunal the laboratory, Amy Bogaard and Mike Charles from the botanical laboratory, members of the excavation team including Shahina Farid, Tristan Carter from the chipped stone lab, Nurcan Yalman from pottery and Mira Stevanović from architecture. There are many other examples documented in our themed volumes and in this new volume, sometimes related to funding opportunities, but often just resulting from shared fascination with sets of data that people come to notice fit together or that create interpretive puzzles or problems. The

Laboratory specialisms	
GIS	Architecture
Geochemistry	Bricks
Micromorphology	Fire forensics
Coprolites	Ceramics
Malacology	Lipids
Archaeobotany	Figurines
Charcoal	Stamp seals
Phytolith	Clay balls
Starch	Geometrics
Fauna	Worked bone
Microfauna	Metallurgy
Isotopes	Wall paintings
Dental microwear	Ornaments
Fish	Groundstone
Bioarchaeology – human remains	Chipped stone
Burial associations	Conservation
Heavy residue	3D reconstruction
Clay sourcing	Database

Figure 1. The main groupings of scientific specialists working on the material excavated from Çatalhöyük.

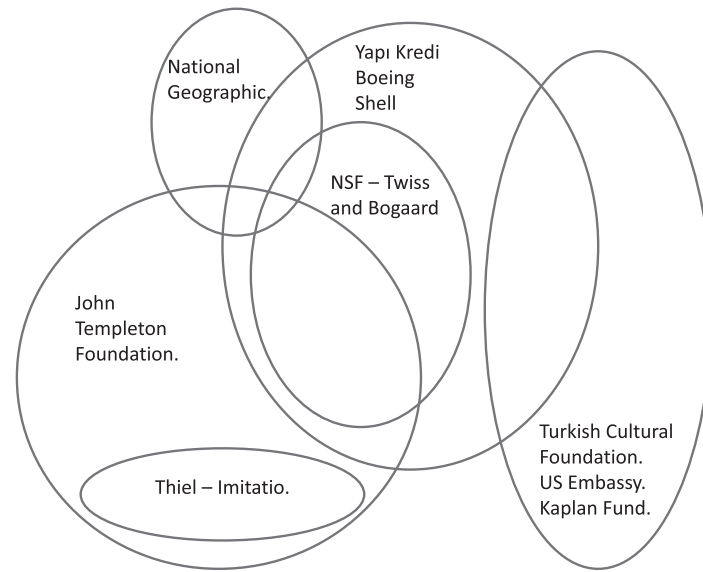


Figure 2. Overlaps between the research interests of the different funders at Çatalhöyük.

network that put together the ‘house foundation’ paper for this volume (chapter 8) is shown in Figure 3. A fuller account of these networks and a more adequate description of their working are provided by Mickel and Meeks in chapter 3.

It sometimes seems that if up to four to six types of data can be assembled by these groups in such a way that they align and give the same answer, the interpretation appears robust and persuasive. These groups with more fits are more likely to persuade other groups in the team and beyond. A good example is the evidence for increased mobility in the upper levels of the East Mound, as discussed in this volume by Sadvari et al. (chapter 12). The evidence for increased mobility is based on at least seven strands of evidence—the cross-sectional geometry of human femurs, *Phragmites* encroachment near the site (indicating people had to

travel farther from the site), pottery production that increasingly used non-local clays, sheep isotope data suggesting wider use of the environment including C4 plants, obsidian data indicating the use of sources in eastern Anatolia, beads and groundstone items produced from a wider range of distant sources. It seems that strong arguments can be made by boot-strapping different types of data so as to assemble a coherent and persuasive argument. But it should be noted that each one of these types of data could be interpreted differently. For example, the use of more distant pottery, groundstone, and obsidian sources may have nothing to do with increased travel across the landscape but could result from exchange. Each individual strand of evidence is interpreted in relation to the other strands, even if they are quite weak, such as the only marginally statistically significant results on the cross-sectional

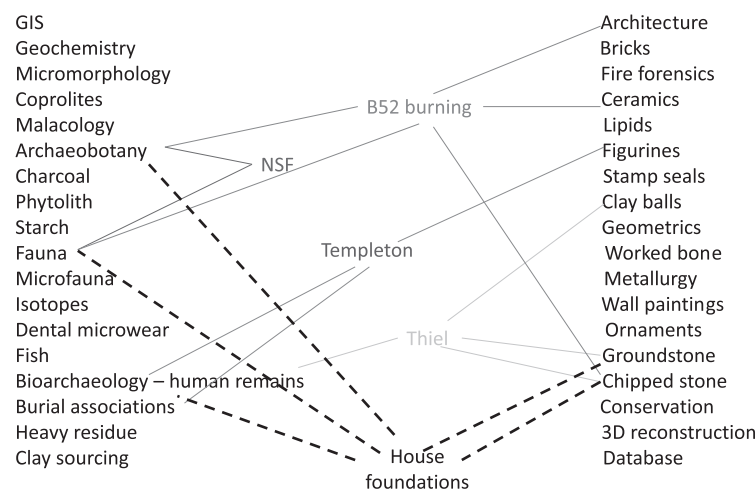


Figure 3. Specialist groups and their research networks.

geometry of human femurs. The idea of boot-strapping or assembling seems appropriate. A theory is produced in the pulling together of different types of data as things are made to cohere. Assembling is an active process that is relational. Everything depends on everything else. In this case, if cross-sectional geometry had shown that human femurs showed less mobility over time, the artefact sourcing data could be re-interpreted in terms of exchange rather than movement.

Sometimes the coming together and assembling into a coherent argument does not work for long—and the project has been going long enough to see the rise and demise of certain theories. Earlier reconstructions of the environment around the site by Neil Roberts and Arlene Rosen had envisaged sufficiently wet conditions that agricultural fields would have been located 12–13 km to the south on drier terraces (Roberts & Rosen, 2009). This reconstruction was based on sedimentological and dating studies of cores taken around the site by Neil Roberts and his team, and on studies of phytoliths by Arlene Rosen that suggested that crops had grown in a dryland environment. New more intensive coring work (by Chris Doherty and Mike Charles), however, has suggested that Çatalhöyük was situated in an undulating and diverse environment, in a marl hollow rather than on a local rise in topography (Charles et al., 2014). A fragmented mosaic is envisaged with higher hummocks interspersed with connecting water channels. Within this diverse environment both wetland and dryland resources were exploited and at least some fields could have been near the site. This new hypothesis is based on strontium isotope studies of plants found at the site, on arable weed taxa found in the archaeobotanical assemblage, on studies of seeds in sheep dung, on faunal remains composition, on oxygen isotope and dental microwear studies of sheep and on studies of larger samples of phytoliths (by Philippa Ryan). Thus at least eight strands of data seem to come together to make a strong new argument. But it is also undoubtedly the case that the team has come to accept this new hypothesis as making more sense in relation to wider expectations. There was a worry that it just did not make sense to have fields far away from the site, and there is strong within-group peer support for a more usual scenario that also fits with previous publications by current team members. It should be noted that Neil Roberts argues that at least some of the new identifications and reinterpretations made by the current team are mistaken and that aspects at least of the old model should be retained. In the end it seems that even the identification of a ‘back swamp clay’ is an interpretation that can be contested and re-interpreted in relation to other data.

There are many other examples of ideas that have emerged informally among team members. For

example, early on we started using the very unhelpful and ill-defined term ‘dirty floors’ to describe a type of floor we saw in the southern parts of main rooms in houses. This was initially just a short-hand that circulated in the group to describe a difference between clean and dirty that we noticed. But it became hardened and has even now entered the literature with elaborate definitions and numerous analyses and studies that quantify and demonstrate the difference (Hodder & Cessford, 2004). The notion that there are different types of midden emerged in the same way. The idea of history houses suddenly emerged in a Templeton seminar in the seminar room on site (Hodder & Pels, 2010) and has grown to dominate our research even though the category remains elusive and unclear. In all these examples we see ideas emerging within various forms of network—whether ad hoc and informal or funded and ‘official’; the ideas either grow or die in the networks. The networks often have social components, based on peer groups that like working together or see strategic advantage in working together, but they also derive their coherence from different and multiple strands of data that seem to align.

A recent example of a piece of data that did not initially seem to fit is the work of Marin Pilloud based on using teeth measurements as proxies for genetic distance between the bodies buried together within the same house (Pilloud & Larsen, 2011). We had all rather taken it for granted that those buried in a house or history house were from the same genetically related ‘family’ in some sense, however large that group might be. But Pilloud showed that those buried within a house were no more linked genetically than any two individuals in the population as a whole. She and Larsen have thus talked of practical rather than biological kin making up those buried beneath a house. This idea was immediately seized upon by those such as Bloch (2010) and other members of a group of Templeton funded scholars as proving that Çatalhöyük was indeed a ‘house society’. For the rest of us on the team, there has been a more skeptical response, but team members can be observed trying to find ways of aligning their data with these new results. People are asking whether their specialist data can be re-interpreted in terms of the proxy-genetic information. They ask ‘if this is true, then what follows in terms of ‘my’ data or ideas? Can I assemble a fit here?’

So in the end the theories that endure are those that fit within the group or some sub-group within the team (see chapter 3), fit the data, and fit within wider theorizing. But the process is always an active one as individual strands of data are re-evaluated and re-interpreted in relation to other strands.

As project participants come to Çatalhöyük, they seek to interpret the site from the standpoint of their own previous experience and theories. A similar

process applies to documentary and film makers, artists, and tourists. Mellaart's interpretations were influenced by the lens of the archaeology of dynastic Egypt in which he had been trained; I have interpreted it through the lens of prehistoric Europe and my own ethnoarchaeology in East Africa; Ruth Tringham and Mirjana Stevanović (Tringham & Stevanović, 2012) brought the idea of the intentional burning of Neolithic houses from the Balkans. Chris Knüsel is a new member of the human remains team who wants to question the absence of violence that has come to be accepted at Çatalhöyük, at least partly because he has previously worked on this topic (Armit et al., 2006), and recently Barbara Mills has interpreted the site in terms of her knowledge of American southwest Puebloan societies—to great effect (Mills, 2014).

But as people work at the site over time, they adjust their perspectives derived from external sources in relation to the contextual data. But they do this in complex, overlapping alliances that involve other specialisms and people and various forms of data and technique. While there is a continual process of hypothesis making, the main way that ideas are generated and accepted is through various types of networks of researchers and data. These informal, formal, ad hoc, and strategic networks and alliances actively assemble data and try things out. They see if some new idea or piece of data can be used to re-interpret their own specialist information. They seek out new corings, new isotope data, new measurements that might add to or undermine preliminary ideas based on other data or outside theories. It is a continual bustling and jostling.

CONCLUSION

Within philosophy and social theory, the term assemblage is often used, as a result of the work of authors such as Deleuze and Guattari, DeLanda and Bennett to refer to the contingent ways in which juxtapositions of usually separated elements lead to the emergence of new knowledge. At Çatalhöyük all of Bennett's components of assemblage are present. Collaboration between usually separated specialisms has produced contingent alliances and co-workings that easily transform. The arguments that emerge do not come about solely from the top-down testing of hypotheses and expectations worked out before-hand; rather the arguments emerge through the process of interlacing and braiding across and between domains within evanescent networks of various types. These assembling operations can lead to dissonance as the different types of data are shown to be misaligned, or they can lead to strong and robust arguments as four or more different types of data are assembled that fit together

and as a community of scholars comes to take them on board and see them as useful within wider networks. But it is important to add to wider theoretical debates about assemblage that the assembling process is an active one that involves seeking new data and re-interpreting data in relation to other data. The process is entirely relational and contextual, returning us to the original archaeological definition of assemblage. It is also an active and intentional process of assembling—we are not just finding fits in multiple strands of data but also making and assembling them into new assemblages.

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