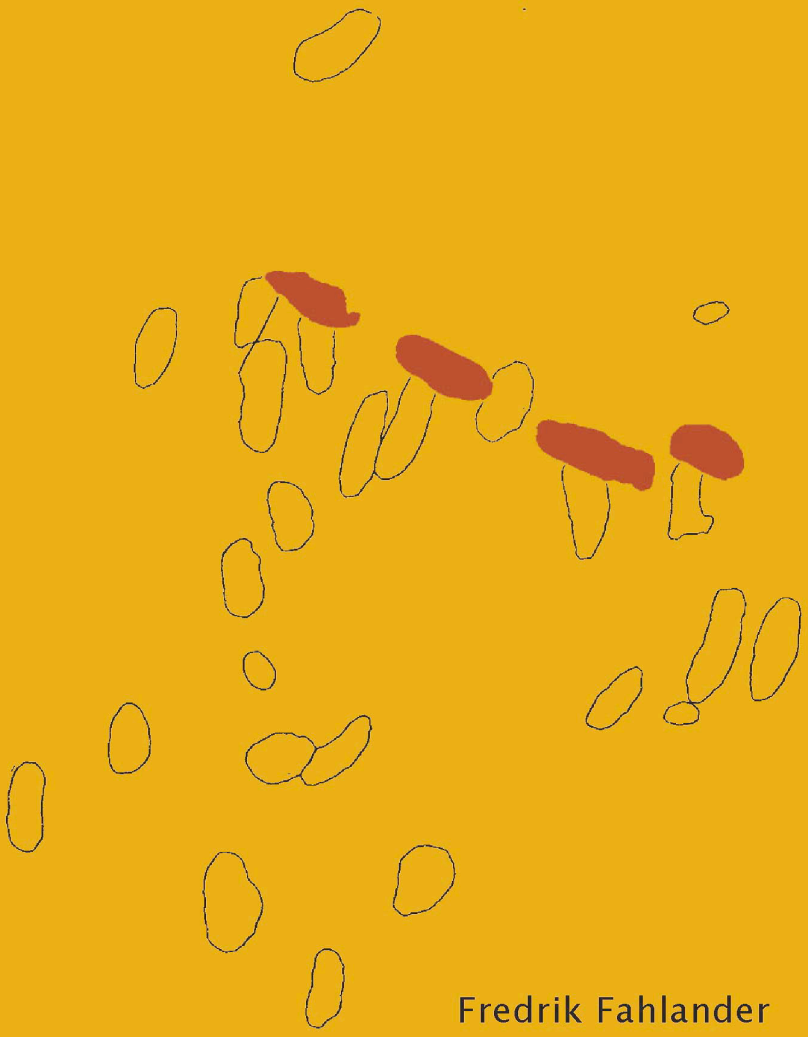
The Materiality of Serial Practice

A Microarchaeology of Burial



Fredrik Fahlander

Errata:

p iii. The heading '*Archaeological approaches to* Materialities' on p. 56 is missing in the list of contents.

p 63 The correct reference is: (Horden & Purcell 2000:80)

p 74. There are, of course, a number of exceptions to Kroeber's cultural relativism, because burial customs [can be] both consistent over time and closely linked to religion and cosmology.

p 89. Both images of Fig 3. needs to be rotated ca 20° CW.

p 187 The complete reference is:

Houby-Nielsen, Sanne 2000. Child burials in ancient Athens, *Children and material culture,* Ed: Sofaer Deverenski, Joanna, London: Routledge, pp.151-66.

p 189 The complete references are:

Kirk, Trevor 1991. Structure, Agency, and Power Relations, *Processual and postprocessual Archaeologies. Multiple Ways of Knowing the Past,* Ed: R. W. Preucel, Carbondale: Center for Archaeological Investigations, Southern Illinois Univ. at Carbondale, pp.108-125.

Lacan, Jacques 1977a[1966]. *Écrits. A selection,* Trans: Alan Sheridan, London: Tavistock Publications.

Lacan, Jacques 1988b. *The seminar of Jacques Lacan. Book II,* Ed: Jaques-Alain Millner, London: Tavistock Publications.

p 191 The complete reference is:

Lillehammer, Grete 2000. The world of Children, *Children and material culture,* Ed: Sofaer Deverenski, Joanna, London: Routledge, pp.17-26.

p 192 The complete reference is:

Meskell, Lynn 1999. *Archaeologies of Social Life. Age, Sex, Class et cetera in Ancient Egypt,* Oxford: Blackwell.

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Introduction
The small, the local and the insignificant

The finger-tip people invaded Yorkshire...
O.G.S. Crawford 1927

In 1998, a local farmer decided to construct a simple dirt road to the top of the Tambouria hill. It was constructed according to local practice, simply by letting a bulldozer cut through the rocks and soil, zigzagging up the slopes and making a rough, but flat enough, surface for pickup trucks or jeeps to pass. It is a fast but brutal method, with little concern for the natural and cultural landscape, that leaves a long, winding, open scar on the hillside. In the scarp of the road in the lowermost area of Tambouria, slabs of stone are now visible, along with smaller concentrations of bones and non-diagnostic potsherds. With or without knowledge of its past use, the road was laid out straight through an ancient cemetery.

Some decades earlier, one or two ancient cist graves had been recorded at the site. They were typical cist graves, constructed of rough stones forming a rectangular cavity and sealed by one or two larger slabs of stone. Such constructions are well known in the area and elsewhere with no particular reference in time or space. The graves were plundered many years ago and any bones or artefacts have long gone. In their current state, they have not been regarded as significant and have thus received little attention from the local heritage management. Now those burials have gone, destroyed by the new, 'constructed' road. Instead, however, a number of previously unknown graves are visible in the scarp or as depressions in the road. In closer examination, traces of their original constructions could be identified. In one case, a small (20x20 cm) hole in the scarp

made it possible to look right through into an almost undisturbed cist grave, including a complete human skeleton. Despite efforts to seal the grave, it was found ruined by animals on being revisited a year later. In one sense, the destruction of the ancient graves is a tragic case of ignorance by the part of the landowner which also reveals gaps in the local cultural heritage management. Some information about burial practices of an ancient social formation is lost forever. From another point of view, the careless construction of the road presents a case of 'free' information that could be retrieved only by expensive excavation.

The destroyed graves at Tambouria illustrate a type of situation that archaeologists often come across. It may be small concentrations of lithics, tiles or pottery, an area of scattered cooking pits or regular formations of post-holes, which are small pieces of information that are often categorized, described and fitted into the general picture of the history of the area. It very seldom produces new, grand narratives of prehistory, but the partly destroyed and seemingly insignificant and mundane objects like the graves of Tambouria could, however, make a difference, if studied in detail in their local setting. Even such scattered information might under favourable conditions form the basis for a new hypothesis that might provide a more diversified view of past social practices. The lack of artefacts in the graves may prevent any traditional analysis of social organisation or the social personae of the buried, but may yet be interesting archaeological evidence, for example, as points of reference in an analysis of the structuration of space.

Boundaries

The graves in question are located on the lower, western slopes of Tambouria; one of many hills in the Asea valley, a high plateau in Arcadia in the midst of the Peloponnesian peninsula of Greece. The geographical location of this particular case is not necessarily important, but a brief description of the area will be in place. The Asea valley is a fertile plateau, c. 650-660 m.a.s.l. and surrounded by mountains reaching 1252 and 1086 m.a.s.l. respectively. Topographically, the valley is a relatively sealed-off area, only accessible through three passes. It has, for all we know, never been a centre of innovation or played any major part in the history of Greece. It has been populated since the Palaeolithic by small groups of hunter-gatherers, who exploited the area using the local chert and other lithics for tools, along with non–local flint and obsidian that was obtained from outside the valley. During the Bronze Age, houses were constructed on top of the Paleokastro, a flat-surfaced hillock situated in the centre of the valley. The acropolis was later the centre

of the city-state of Asea, known from historical sources, which developed into a *polis* in the Classical and Hellenistic periods (Forsén & Forsén 1997, but see Drakopolous 1997). The Asea valley suffered a typical decline in population during the early imperial period and was sparsely populated during Roman to medieval times (cf. Alcock 1993). In modern history, the area has suffered much from emigration and urbanisation, which has left the valley sparsely populated and relatively little exploited. At present, a small number of farmers occupy the area, scattered in small villages, and occasionally visited by expatriate-Arcadians on weekends and holidays.

In the mid 1990s, the valley was surveyed by the Asea Valley Survey Project, an intensive, multi-period survey, which provided a relatively detailed picture of past activities in the area (e.g. the extent and locations of the *polis*, small hamlets, Roman *villa rusticae*, cult places, temples and graves, etc.). The special topography of the valley seems at first to form natural boundaries for a study area, but, of course, it has never, regardless of the restricted accessibility, been a cut-off and isolated 'social unit'. The written sources tell us about its inhabitants' involvement with other *poleis* in wars etc. and the material culture found in the valley reveals evidence of trade and other frequent contacts with people outside the valley. Thus, even under these seemingly favourable conditions, topography seems to be a poor factor for limiting social analysis (cf. Butzer 1982; Hodder & Orton 1976, Cornell & Fahlander 2002b).

The particular circumstances of the Asea valley raise a series of questions on how to find any proper spatial boundaries for social studies. There is a general trend in social theory today to move away from the ideas of homogeneous cultures and ethnic groups, pointing instead to the heterogeneity and complexity of social life (e.g. critical anthropology, post-colonial theory, queer theory, etc.). Archaeology has been slow to explore this approach, perhaps because of the fragmented and 'mute' nature of the archaeological record. How can we possibly reconstruct the practices carried out at a certain time and place if we dismiss the idea of nice and neat clusters and spatial entities? What about the small cemetery on the slopes of Tambouria? Why was this particular location 'chosen' for burial? What factors (material, practical, economical, aesthetic, social, etc.) were involved in that decision? Can we presume that cosmology or religious apprehensions in relation to practical (functional) considerations were the principal guiding factors? Should we settle by trying to associate the graves with the closest contemporary settlement or should we search for relations that are more intricate? A crucial question is how we can relate the particular information to the general picture without violating the local variability of the empirical information.

Fig. 1. The Asea valley, situated in the heart of Arcadia in the midst of the Peloponnesian peninsula of mainland Greece.

The structuration of space

A typical aspect of the Arcadian countryside is the multitude of terraces. Even the smallest patch of soil on top of the hills is normally terraced, a clear sign of the intensive use of arable land for cultivation or grazing. It is difficult to date these constructions; some are surely ancient, while others are later, e.g. Byzantine, medieval or perhaps relatively recent constructions (Foxhall 1996:44). The question of who owns which piece of land is both fluid and straight forward. Most farmers and landowners claim to have quite good ideas of which parts belong to them. On the other hand, a recently launched government project aimed at formalising the validity of these claims has had problems, as few legal documents are available. When I was working in the valley, there was one incident that struck me as odd and interesting. In a certain part of the valley, we frequently met an old lady riding a donkey and bringing a herd of sheep for grazing. This is not an odd sight in the rural Greece of today. The peculiar thing about this particular lady was that she herded her sheep quite far away from the village in which she lived.

This daily trip was undertaken, despite the fact that there are plenty of uncultivated areas suitable for grazing closer by. The reasons why she made this extra effort are many, and it is not necessary to account for them in detail. Yet the incident was striking. As archaeologists, we walked the fields and slopes recording concentrations of artefacts (sites) which ended up as dots on a map. It became apparent to us that our assumptions of hamlets and villas as central nodes of a circular sphere of activity would not always be an accurate model for the various ways of past land-use and other activities. The uses of individual and collective 'sub-areas' are not simply anomalies but are likely to be more frequent than we normally acknowledge. An attempt to describe the daily patterns and relations between people, their land, their places of residence, etc. would certainly prove interesting; perhaps illustrated in access diagrams or in time-space diagrams á la Hägerstrand (see Fig. 3, Chapter 2). Locations, paths and daily movements can also be discussed by relating hard-data circumstances (material topography) to cognitive maps (e.g. Lynch 1960; Soja 1996).

Such analysis would probably reveal much seemingly irrational behaviour far from the point of view of rational-choice theory. Here, we should not forget the aesthetic or affectual vectors involved. Some paths and places are certainly experienced as better or worse with different material or social qualities. The idea of the small village or hamlet where the farmer kept the animals in stables, cultivating the neighbouring land, is only an assumption that covers only one of many, more or less simple ways of utilising and movements in a microecology. Two-dimensional maps and three-dimensional GIS data can thus sometimes be misleading in analysing social spatiality, and various aspects are perhaps better grasped by the personal (bodily) experience. The structural relations between the inhabitants and their milieu (daily routes, paths, working and dwelling areas) are probably better seen as an ongoing structurating process. It is structural yet individual, temporal yet recurrent. It has a past, a present, and a future. Obviously, such considerations must occupy an important place in our analysis of material culture. For instance, where did they actually live and work, those who buried their dead on the slopes of Tambouria? Can we really assume that they lived somewhere close by or are other aspects (material and social) involved? These are surely matters worth investigating further.

Data and theory formation

So why do I want to share these personal experiences and observations made during fieldwork? I do this because they highlight a number of important issues. This thesis is mainly about social the-

ory and method, which may seem abstract to some readers. But the text is not simply about elaborating social theory. The experience of the Asea Valley Survey initiated a process of thought, quite heavily influenced by the special geomorphic and historical conditions of the area. The formation of such knowledge is often continuously adjusted, elaborated, complex or simplified. This text is only one example of such a process. Like any process of learning and experience, it is socially and embodied constituted, ongoing and never finished. Texts like the present are only temporary halts, points of reference to which forthcoming works may be related. The important issue at stake here is that the formation of any 'scientific' work cannot simply be restricted to the reference of texts or empirical data. It is also a social process involving less scientific issues like personal history, fiction novels, movies and everything that, in this case, comes out of a middle-class European, male experience. It thus seems difficult to draw any distinct line of demarcation between empirical studies and theory-building; it is rather a mutual process. There may be a difference in learning from a text than from actual experience, and indeed there are pros and cons to both. I shall not dwell further on the issue of theory-building here, but, as this work is in some respects abstract and theoretically advanced, I want to emphasise the role of fieldwork and data. The theoretical approach advocated here is thus not simply a result of elaborating social theory; on the contrary, as I have tried to show, the field experience of the local material circumstances of the Asea valley seems to have initiated ways of thinking about the particular data. The basic questions that are considered roughly correspond to the themes I have introduced here:

(a) An interest in exploring the possibilities in the small and the ordinary, the 'subaltern' objects of the past.
(b) To point to social heterogeneity and find means to do archaeology without confining the analysis to general cultural traits or regional traditions.
(c) To discuss the social structuration of space.
(d) To discuss graves as sources of social information
(e) To draw attention to the obvious links between data and theory-building.

These basic questions are, with the exception of the last, what the reader will find discussed in the remaining text. These objectives may perhaps seem dispersed at first, but throughout the text it will be apparent that they are in fact closely related. I do not, however,

claim to cover all the options and angles of these fields. This said, I shall give a brief outline of the remaining text.

Terminology and the outline of the text

The structure of the thesis is generally organised in three blocks; besides the introduction and a summary, the first block is a quite extensive theoretical chapter (Chapter 2), while the later two (Chapters 3 and 4) introduce some empirical data as examples of operations. The theoretical block discusses social practice in general, but I have chosen to limit its 'application' to the social analysis of burials as a common theme. The second block concerns the social analysis of a small, Neolithic, burial ground, while the third block discusses the structural relations of the places of graves in their local microecology. The main chapters of the text are subdivided into *general* and *particular* parts. This is simply a loose orientation of the contents, in which 'general' refers to discussions valid for more or less any social study, past or contemporary, whereas 'particular' concerns aspects related to archaeological analysis. This is one way of dealing with complex social theory and the problem of linear writing, as some matters are difficult to discuss without dealing with others in advance and vice versa. Consequently, the first part of Chapter 2 concerns general social theory, while the second, particular, part concerns theoretical and methodological aspects that are particular to archaeological circumstances. Matters of social heterogeneity, boundaries, structuration of space and relation to the material world are highlighted. As the title suggests, I advocate a *microarchaeological* approach, which consists of a bundle of theories, concepts and operations. Microarchaeology is not simply about detailed studies on a micro-scale. The particular is emphasised, but also relations to more general 'structures' on the meso- and macro-levels. It is suggested that *executed* social practice is an appropriate object of study, as it mediates individual experience and extra-individual facets (general structures), as well as the relations between the particular and the general.

The outlined microarchaeological approach provides a general perspective, nomenclature and tools with which to analyse social practice. As a major part of the text is devoted to quite complex social theory, it may be appropriate to dwell for a moment on some theories and concepts that may not be familiar to all archaeologists. One quite well-known concept is the process of s*tructuration.* It refers to the basic process of sociality outlined by Giddens (1984), who argues that individual action and general structures are mutually constituted. Social structures (or discourses) are thus both a medium and an outcome of social practice. The central figure in this process,

the individual, is a complex character and cannot simply be referred to as a homogeneous mass of 'knowledgeable' agents. The different approaches of the social sciences have produced a varying and sometimes confusing nomenclature of this intricate character. Frequently used terms are, e.g., actor, member, agent, person, subject, etc., all carrying different connotations, deliberate or arbitrary, of what an individual is and its capabilities in relation to materialities, social structures or discourse (cf. Fahlander 2001:68ff).

I have chosen to employ mainly the term *social subject* here to denote its dual constitution as both a material (corporeal) body and a flexible social constitution. A subject might simply be equal to an individual as a thing (a body) or signify the socially constituted mind. Related terms here are *subjection* and *subjectivation.* By subjection, I refer to the various ways in which a subject can be subjected with reference *to* something, i.e. a despot or social structure /power/materialities. The term subjectivation *(assujettissement)* is more ambiguous and differently defined. I use the term to denote less conscious, social categorisations and distinctions, like the way many of us semi-unconsciously distinguish between a person in a suit and another in working clothes, as well as between non-subjective distinctions between other individual characteristics etc (cf. Zizek 1989:174; Foucault 1990:91; Braidotti 1991:48). There may also be a need to clarify the concept of the *actant.* The concept was originally a linguistic term (Cornell & Fahlander 2002a:62) but is perhaps better known from the work of Bruno Latour (e.g. 1991). Latour argues that material objects often functions as social actants similar to a human agent; they can potentially play an important part in social events and chains of actions. The term is also employed here to denote the socialness of the *corporeality* (i.e. physical constitution) of living or dead human bodies.

From Sartre, I have taken a very useful notion of *serial action.* Sartre invented this notion in his attempt to grasp the core of a dispersed and fluid, working class (1991[1960]). It suits the micro-archaeological approach like a hand in a glove, as such a serial collective is defined by individual actions and relations to similar materialities rather than by predefined social categories. It is thus very useful in trying to make sense of plural and multivocal pasts. To make Sartre's concept of serial action even more useful, I have added the notions of *serial categories* and *serial routes.* Serial categories are defined from a *number* of serial collectives that some individuals are regularly participating in. The concept of serial routes is simply a way of adding a time-space dimension to the concept of serial categories. Different individuals tend, for various reasons, to move regularly in similar paths in time and space (going to work, leaving work, stopping at a store, going home, herding

sheep, etc.). The intricate and complex relations between the individual and social structures (ideology) are approached in depth from a psycho-social viewpoint. Important concepts here are Jaques Lacan's notion of the Symbolic, the Imaginary and the Real as well as Slavoj Zizek's (1989) unorthodox use of the term *ideology.* Except for the last, these concepts are not normally adhered to in archaeology, but they are very enlightening in grasping the complex relations between individual experiences/agencies and seemingly extra-individual, social structures (ideology). The focus is thus on the social implications of Lacanian theory rather than on the clinical aspects. In my 'awry' reading of Lacan, the social dimension is seen as the remnants of our socialisation processes as *corporeal* and *social* individuals; it is a matter of both cause and effects. The process is structurating that generates general symbolic references and ideologies with many individual 'deviances' from an imaginary 'normality'. It is not a closed, anti-humanist perspective; on the contrary, the resulting social conditions are likely to vary under different social and material conditions. Lacanian theory thus implies that past experiences and practices, individual or collective, may be very different from what we know and thus impossible to understand in a hermeneutic sense. The very same processes also imply that social heterogeneity is a more likely result of collectiveness than homogeneous, collective experiences and representations. I shall return to these issues in greater detail in chapter 2.

The microarchaeological approach also brings about a few neologisms. Central are the concepts of *structurating practice* and *structurating positivities.* The first term concerns monitored and regular social practices that may have a potential of being structurating. It is generally akin to concepts like tradition or ritual, but, contrary to those, the term 'structurating practice' represents only unconsciously or consciously *executed* practice, that is, not concerning the assumptions and representations that often lie behind traditions and rituals. The second term, 'structurating positivities', denotes more general, often cross-cultural or intersocietal, ways of thinking and acting. It thus concerns a higher level of regularity in social practice that often cross-cuts social boundaries and which is generally opaque to the individuals who by their structurating practices sustain or alter them. These terms may seem abstract at this point, but I hope that their usefulness will be evident throughout the text. The microarchaeological approach also includes two metaphorical concepts, *fibres* and *threads,* which may help to clarify the relations between structurating practices and structurating positivities. These are, however, abstract metaphors which should not be taken literally but may prove useful in more general discussions.

The ways of operationalisinge social theory on past practice are vast. I touch upon a number of aspects in the first block, but the following 'case-studies' are, for obvious reasons, limited in scope. As the title suggests, the focus is on graves as a source of social information. I discuss graves from two general viewpoints: in the second block, I turn to account their potential as sources for the social analysis of subjectivation and social practice, while the next concerns their potential roles in the structuration of space. The general part of the third chapter is devoted to the archaeological study of burials. I recapitulate some general themes in 'burial archaeology' or 'the archaeology of death' and outline the general aspects of the microarchaeological approach. The particular part concerns a case-study of the Neolithic burial ground of Ajvide, Gotland, Sweden. The example is more or less randomly chosen, that is, not because of its great potential, but as an example of a burial ground that is moderately sized yet complex enough for elaborated analysis. The study starts from a view of graves as being traces of social practices rather than as manifesting religious ideas of death and the afterlife or individual social identities. The burial is viewed as a manifested 'statement' *(énoncé)* of *social practice,* which means that we do not need to relate the properties and interments of the grave to any particular individual. Instead, I discuss associations between corporeal attributes (stature, sex, traces of wear, etc.) and elements of the grave type and interments, in order to identify changes in structurating practice and social subjectivation processes. It would be a great advantage if we could bypass the awkward question of individual identity and status in burial analysis and instead focus on identifying subjectivation parameters and social categorisation processes. Here we shall find that Sartre's concept of series has many advantages in discussing such correlations and categories.

In Chapter 4, I return to the burials of the Asea valley. The general part concerns the social structuration of space, that is, social practice in relation to topography and the material conditions of action. Graves are also in this chapter the main objects, but here the focus is set on their location in the 'natural' and 'cultural' landscape (e.g. microecology). The particular part of the chapter seeks to elaborate and exemplify the general discussion, drawing on data acquired from surveys of the Asea valley and the Tegea valley of Peloponnese, Greece, in which I participated during the period 1995-2001. My personal knowledge of the local conditions of the area makes these locales a natural choice. In dealing with the social structuration of space, I employ the term *signifiant* for material elements, natural or cultural, which have a potential function as 'nodes' in the process of structuration. The name originates from French linguistics and is used to emphasise the arbitrary significations of such

objects. Material elements functioning as signifiants are assumed to be loaded with social content (a hermeneutic would use the term 'meaning'), which may impute agencies and interfere in the social structuration of space. An example of a signifiant can be a conspicuous element of the landscape that may evoke images and ideas of metaphysical powers or mythical origin. In this third block, I argue that the choice of location for burials can be utilised in two respects. The first is the social information that can be extracted, which may be informative about local structurating practices, as well as the social organisation of space in a specific microecology. The second aspect of the study concerns methodology. In the Mediterranean area, domestic sites are relatively easy to locate, since concentrations of tiles and sherds are commonly visible on the surface, while subterranean burials demand more thorough efforts for their discovery. If some structurating parameters involving the placing of the dead can be identified, the known positions of settlements can be used in combination with the properties of the local environment to predict possible locations for burials. The data of Asea and Tegea are generally fragmented and limited in scope, apart from their position in time and space, but they will nonetheless reveal something about what happened to the preliminary observations after theoretical considerations.

These three 'blocks' form the general outline of the thesis: the elaboration of social theory and method in general and two particular examples of how it can be 'applied' to the analytical field of burials. I am confident that this first attempt will foster an interest in the great potential of microarchaeological studies.

2
Microarchaeology
Serial practice and materialities

If a lion could talk, we could not understand him.
Wittgenstein 1953:223e

In Spike Jonzes' film *Being John Malkowich* (1999), the leading character Craig Schwartz (played by John Cusack) manages to get inside the head of the famous actor John Malkowich. Cusack plays an unemployed puppeteer who gets work as a filing clerk in an office (on floor seven and a half), where he stumbles across a portal into John Malkovich's head. When he enters the portal, he can for a short period of time see and feel what Malkowich experiences before he gets thrown out. The film is, suffice to say, quite surreal. The ability to get into someone else's head, if only for a moment, may seem intriguing but is, of course, not feasible. Still, this is what many archaeologists seek to do when they interpret material evidence. Hodder (1987:7; 1992:17) claims never to have read an archaeological text in which the interpretation did not to some extent depart from the experiences of 'them'. "In my view, the idea that archaeologists can get away without reconstructing ideas in the heads of prehistoric people is pure false consciousness and self-delusion" (1992:18). Of course, Hodder is correct in a basic sense. On the general level, we relate social agency to the material situation in which social practice are conducted and we often do this from a general human point of departure (i.e. our personal experiences as human beings). This is, however, not the same as attempting to *understand* social practice or localizing its *meanings*. In Jonzes' film Schwartz

actually experienced what Malkowich saw and felt through his eyes and body. Still, his mind was his own, in which he processed and related Malkowich's sensory input. If he really *was* John Malkowich, he would not have experienced it as different and thus not remembered it. The same must, of course, also be true for archaeologists, with or without secret portals into the past.

Morris (2000:24) has discussed the questionable development in archaeology from 'being fact-grubbers to mind-readers'. He stresses that archaeological theory is curiously inert in comparison with other historical sciences. Indeed, post-processual archaeology inherited much of processual and culture-historical archaeology and has in some respects failed to develop more elaborated, social models. One example is the failure to deconstruct the traditional frame of reference, culture and culture areas. A majority of archaeologists would argue that archaeological cultures, like the Bell-beakers, are nothing but heuristic tools that do not necessarily correspond to 'real' ethnic, social groups (cf. Malmer 1975:48). Still, the focus on hermeneutics as the prime methodology requires a common frame of reference to make the concept of meaning meaningful. It is certainly true for most social formations that there are to some extent cognitive kernels, *representations* (at least on an imaginary level), which make social cooperation possible. As Barth (2002:31) puts it: "… let us expect *some* functional imperatives, *some* normative pressures, *some* deep structural patterns, *some* effects on the relations of production on life chances, and *some* shared cultural themes in ranges of local institutions."

The rate of social homogeneity and intersubjectivism should, however, not be exaggerated. On the contrary, much contemporary social theory rejects such simplistic viewpoints, pointing to the heterogeneous, contradicting, plural and multivocal aspects of social life (Braidotti 1991; Moore 1994). A reason why the idea of social units is still so persistent in social theory may be found in the specific circumstances of European history. The Spanish sociologist Lamo de Espinosa (2002) has recently questioned the ideal image of homogeneous nation-states by relating their composition in a global, perspective. He concludes that, of 160 states throughout the world only 28 are homogeneous (in the respect that 90% of the population within the national borders share ethnic identity). European nation-states, however, differ from nations of other continents, since they include only 4.6 languages per state and the average number of speakers per language is 4.4 millions (world-wide, the average number of speakers of a language is 700.000 and the average number of languages within nations is 30). It is thus not surprising that western European discourse tends to exaggerate homogeneity and cultural understanding in social studies.

In recent years, post-processual or interpretative archaeologists have acknowledged the open and fluid character of social formations, stressing buzz-words such as plurality, heterogeneity and multivocality. The problem for mainstream, post-processual archaeology is that social multiplicity can hardly be analysed by hermeneutic interpretation/understanding of meaning. The hermeneutic approach has thus been criticised for its inconsistency in assuming a general understanding within social groups, while 'mouthing varieties of liberal pluralism' (Spivak 1999:9; cf. Gero 2000; Berggren 2000). If past social formations are truly open and multivocal and populated by heterogeneous agents, subjectivated in different categories, there can hardly be one common horizon from which we can understand agency. The difficulty in analysing social action from a pluralist perspective is not a problem restricted only to archaeology but to all social sciences. We find attempts to deal with such problems prominently in post-structural feminism, queer theory and post-colonial theory (e.g., Braidotti 1991; Spivak 1999; Rosenberg 2002), and also in strands of sociology and anthropology (e.g. Moore 1994; Barth 2002). There is, however, at present no single approach that we can simply adapt and apply to archaeological analysis. The special circumstance of archaeological analysis, that is, analysing material traces of action without given frames or comparable models, differs here from other fields of social science. The microarchaeological approach advocated here is an attempt to work with archaeological evidence from such a heterogeneous perspective, taking the complexity of social action seriously.

Microanalysis of practice

The microarchaeological project has previously been discussed in a series of papers (Fahlander 2001; Cornell & Fahlander 2002a; 2002b; see also Cornell 2000). Some repetition of the arguments and general discussion will be necessary, but I shall also take the opportunity to explore some issues further. Microarchaeology started out from disappointment with the discussions in archaeology and the social sciences at their apparent 'failure' to construct an operative theory of social agency and practice. As previously hinted, microarchaeology is not simply a theoretical construct but is developed in relation to fieldwork and empirical data. Microarchaeology is thus both a theoretical project and a general operative approach to sociohistorical phenomena (like material culture). It would not be very meaningful to discuss social theory on the general level without explicit considerations of how to apply it to empirical data. Sociohistorical phenomena, like material traces of action, are not randomly constituted; they are formed by social practice (although

'distorted' in various ways over time). Here we have a link between social action and the archaeological record. The problem is to make social analysis from such a fragmented record of behaviour. The theoretical basis of microarchaeology is neither processual nor post-processual in character, but seeks to combine strands of thought, methodology and practice independently of their origin. The most notable sources of inspiration are Sartre's theory of serial collectivity (1991), Foucault's 'archaeology' (1972), Lacan's psychoanalysis (1988a; 1988b) and the structuration theory of Giddens (1984). The microarchaeological approach has in some respects a number of things in common with other 'small-scale' approaches of social theory, such as microsociology (e.g. Goffman 1967, 1971, 1974; Garfinkel 1967), microhistory (e.g. Levi 1991; 1998) and microecology (Horden & Purcell 2000). The prefix *micro* does not, however, simply refer to a limited scope of analysis. A local perspective is often necessary to grasp social variability, the queer and strange. Analyses of large areas and time spans tend to mask such important information and create a too general image of social variability. This small-scale focus is not to be confused by individual studies of separate events. The point of departure is the relation between chains of actions and repetitive events. The analysis of single and repetitive actions is thus analysed in terms of relations between the *particular* and the *general*. It is argued that the individual actions and the particular events generally have some relation to more general structures.

Microarchaeology is a *social archaeology* in the sense of a focus on the *social* character of action. Social action is always related to the Other; it cannot be reduced to one's own free choice. The main subject of analysis is neither the mind of the single individual nor social structures but *executed social practice.* Such practice is not necessarily preceded and initiated by thought, language or knowledge; the practices of daily life are rather 'automatic' and semiconsciously executed in a given material context. It is generally a structurating process, as formulated by Giddens (1979; 1984) and Sartre (1991), among others, that is, a mediation between the structural and material conditions of action and individual experience and motivation. The focus on executed action thus allows us to override static notions of structural constraints versus individual experience. Both aspects are present in varying degrees. Social practice, the performed activities of a particular situation, is, in a sense, *a mediation* between these poles (cf. Giddens 1979:4). Social practice is in different respects a result of the properties of the particular situation, but these cannot be seen as necessarily unique, as they also include traditions, institutionalised power relations and other aspects of the 'outside' world. By focusing on executed action rather than on in-

tentions and experiences, microarchaeological analysis does not need to define static notions of a primary social context, like culture, society or ethnic groups. This is an important aspect of microarchaeology. Social practice is not analysed in relation to any preconceived general representations, which means that analogies with contemporary information are kept to a minimum. Microarchaeology is thus not a hermeneutic enterprise.

An archaeology without social frames of reference may at first seem awkward and strange; the identification of social or ethnic groups has been a prominent goal of archaeology and considered a necessary tool for discussing innovation, diffusion, meaning and social organisation. However, as previously argued, it is difficult to discuss multivocality and heterogeneity, on the one hand, and to define homogeneous, social or territorial groups, on the other. The constitution and initiation of social practice cannot be derived from concepts of collective representations, social or cultural systems. That would be too much of an oversimplification of the social complexity. The identification and problematic definitions of such groups or collectives are not necessary in a microarchaeological approach. Instead, microarchaeological analysis relies on the social information embedded in sociohistorical phenomena (materialities and other traces of action), rather than on analogies with known social practices or societal structures (contemporary or historical). The microarchaeological approach is by no means a finished project, but, by way of introduction, some main themes will be discussed in detail here. Others are considered in Chapter 3 and 4 in relation to the case studies. In the following Chapter, I shall begin by discussing the general aspects of social practice, while the latter part will concern questions particular to archaeology.

Agency and social subjects

The history of social science is marked by disputes and controversies regarding many of the key concepts: subject–object, macro–micro, materialism–idealism, agency–structure, etc. The conflict seems to focus on whether general structures are a fluctuating result of individual strategies in interaction with others or whether praxis is constituted by pre-set structures. Is the whole more than its parts? These are classical sociological questions, traditionally contributed to the social projects of Weber/Simmel and Durkheim /Mauss respectively. In the social sciences, we find an almost endless stream of text-books on agency, describing the pros and cons of various approaches. No single method or theory seems to be without drawbacks; either it focuses too much on individual experience or too much on the discursive forces of society. It seems impossible

to simultaneously analyse small-scale, individual experience and cognition without losing sight of large-scale, structural patterns or vice versa. This is not simply a scientific matter; the social sciences have always been subject to ideological bias on this matter, mixing political agendas with generalisations and ethics. Individuals have been perceived as autonomous entities, cultural/discursive dopes or chiefly as social animals driven by their biological constitution. The analogy of the jigsaw puzzle is frequently used to illuminate this problem. It is argued that detailed analysis of one or a few pieces does not account for anything, but, when put together in their proper places they form a complete image. However, this particular example builds upon a static and simplified view of human agency and social homogeneity. Individuals are not equally able and empowered and the sum of social relations is an ongoing process with many contradictory facets.

These aspects of *processes* and *diversity* make it no better to speak of bottom-up and top-down views; i.e. respectively to analyse society from the perspective of its members or to study the impact of the society on the individuals. Both views seem equally valid, as they are different approaches to the same phenomena (cf. Wallerstein 1990:65). It is perhaps natural to focus on subject-side matters in face-to-face situations, and similarly on general trends while, e.g., comparing societies. However, many sociologists and anthropologists (e.g. Malinowski 1939:962; Callon & Latour 1981; Giddens 1984:139; Ritzer 1992:74) reject the micro-macro dualism as both artificial and misleading. There are, as Ritzer (1992:74) points out, no clear boundaries between macro and micro (or meso) levels. It rather seems that this particular problem is posed from an erroneous perspective, disparaging the multileveled complexity of social practices. In recent years, we have seen a number of attempts to 'bridge' the classic dichotomy, for instance, Norbert Elias' (1998) figuration theory, Habermas' (1972) discussion on the colonisation of the life world, Sartre's (1991) concept of serial collectivity, Bourdieu's concept of habitus (1990) and Roy Bhaskar's (1979) transformational model of social activity. Perhaps the most illustrative notion of the process is to be found in Anthony Giddens' theory of structuration (1979; 1984).

The process of structuration

Giddens stresses the double nature of structure – that individuals both produce and reproduce social structures by their actions, constrained or enabled by structural properties (Giddens 1984:162). In this view, social structure is both the medium *and* the outcome of social action (Giddens 1979:5, 69, 218; cf. 1984:25ff). Social structures

in the Giddenian sense consist of recursively organised sets of *rules* (e.g. habits and routines) and *resources* (material and ideological), which are organised in social formations as *institutions* (Giddens 1984:28-34). In *The Constitution of Society*, he outlines the structurative process as follows:

> The basic domain of study of the social sciences, according to the theory of structuration, is neither the experience of the individual actor, nor the existence of any form of societal totality, but social practices ordered across space and time. Human social activities, like some self-reproducing items in nature, are recursive. That is to say, they are not brought into being by social actors but continually recreated by them via the very means whereby they express themselves *as* actors. In and through their activities, agents reproduce the conditions that make these activities possible (Giddens 1984:2).

Giddens seeks to incorporate the phenomenological aspects of motivation by emphasising the reflexive monitoring of the agents as a continuing process across time and space. The agents routinely reflect on the causalities of their own and others' actions. In this process, Giddens stresses the knowledgeability and reflexivity of agents in opposition to, e.g., behavioural schemes of intentions and responses. Giddens' path to integrate structure and action may be seen as individual–oriented, accentuating the equal capacities of individuals to change and manipulate their worlds. Nonetheless, one must view some of Giddens' statements as rhetoric characteristic of the time rather than a theory of autonomous subjects (cf. Meskell 1999:25). Structuration theory, at least as outlined in *The Constitution of Society* (1984), favours the regulative aspects of social practice over individual autonomy. Considering that Giddens' actors are subject to rules, routines and limited access to resources, and bounded by unacknowledged conditions and unintended consequences of their action (Giddens 1984:5, 12f, 294; cf. Thompson 1984:151f, Thrift 1985:619), there is little room for individuals and non-institutional groups to radically transform their social system in a conscious manner.

It is safe to say that Giddens' structuration theory has had a great impact on the formation of post-processual archaeology. Many archaeologists have stressed structuration theory as a favourable perspective for social analysis (e.g., Hodder 1982:208; Shanks & Tilley 1987a; Donley-Reid 1990; Thomas 1989:101; Kirk 1991; Hodder 1992:85; Barrett 1994; 1998; 2001). This attention is understandable, since structuration theory is rhetorically persuasive, but to transfer a social theory developed for the contemporary western world to the more uncertain past is, however, not without complications (cf.

Burnham & Kingsbury 1979; Cornell & Fahlander 2000). From an archaeological viewpoint, Giddens' understanding of pre-industrial societies leaves a great deal to be desired. In this, he is as negligent as many other sociologists. Giddens' knowledgeable agents are mainly well educated and have a fair amount of knowledge of the structural elements of their societies - almost as good as 'any social scientist' (1984:xvii). This ability and knowledge cannot be assumed to be valid for all phases of prehistory. We should rather expect that the knowledgeability and self-consciousness of agents will vary a lot, according to their specific sociohistorical context. Although the notion of structuration is fairly well able to account for the general process of how social formations persist, change, or cease to exist, we still need to dig deeper in that process. In this chapter, I shall focus on two processes in particular: the constitution of *social subjects* and *ideologies.*

Corporeality and social subjection/subjectivation

Individuals appear in many forms in social analysis. Perhaps the most common form is the anonymous subject, tacitly comprehended as a grown, mature male, whose properties and potential vary according to a sociohistorical 'normality'. The scale varies from primitive, instinct-governed savages, through Spencer's 'economic man' to the concepts of the (post)modern, self-reflexive individual. To be sure, prehistoric individuals, as well as non-Western, pre-industrial, indigenous ones, are often situated somewhere near the beginning of that scale. These are, of course, prejudices, with little or no support in theory or data. Many archaeologists have also pointed out the inconsistencies in viewing social subjects as homogeneous, equivalent and socially *able* (e.g. Berggren 2000; Gero 2000). This confusion may be one explanation of the controversies regarding the issue of social agency. The social subject is in a basic sense a biological being. The biological 'facts' are, however, not very helpful to the social scientist; their impact on social action is likely to vary according to the level of technology and the sociohistorical context. The biological imperatives are far too general to be of any significance beyond the mundane. The needs may be the same (e.g. food, sleep, sexual satisfaction, etc.), but the ways in which these needs are fulfilled or expressed are known to vary culturally (Berger & Luckmann 1966:210). However, to belittle the biological aspects is not the same as to emphasise mind over matter; social subjects are *embodied beings* with body and mind in conjunction. The literature on the body and the corporeality of the body has rapidly become vast in recent decades (e.g. Davis 1997; Welton 1998; Burkitt 1999) and embraces various strands of thought.[1] Some projects depart from the

phenomenology of Merleau-Ponty, stressing that all sensory input is transmitted through the physical body and hence is affected by its constitution (e.g. Bigwood 1991). Others follow strands within post-structuralism, such as Foucault's theories of the disciplined body, the body as imprisoned by the soul, exposed to (and exposing) subjectivation and power (e.g., Foucault 1980; Butler 1997). To make things clear, we need for a moment to make an *analytical* distinction between the individuals as *corporeal* subjects and individuals as *social* subjects. The corporeal dimension can be discussed in two ways. One facet is that our corporeality is often related to the way we are able to act in the world. For instance, Joanna Brück (1998:28) has argued that pregnant women or disabled individuals may (but do not necessarily) have different ways of negotiating with monuments like the Dorset cursus.[2] In a similar way, the general abilities and means of children at different ages will by and large affect their agencies in other ways than adults (e.g., Sofaer Derevenski 2000). To acknowledge such variability in agency and ability must be regarded as crucial for interpreting material traces of action. Especially the wide category of children is important, since they have always been present and thus 'responsible' for some of the archaeological record (cf. Grimm 2000).

The other facet of corporeal characteristics is their potential as imperatives for social subjectivation and categorisation; the body as an *actant*. The corporeal subject is a material node subjected to social and ideological processes. Some aspects of corporeality are likely to function as active social signifiers, arranging and subjecting individuals into social categories or groups. For instance, phenotypic aspects of individual corporeality such as sex, age, skin colour, etc. are (today) conventional bases for the construction of social categories and identity (Moore 1994:13; Fahlander 2001:78ff). Foucault's work on the exclusion of the mad, together with his work on the genealogy of homosexuality, are interesting historical examples of such subjection and subjectivation processes (e.g. 1981; 1989). The corporeal view also points to the fact that individual subjects are not alike, with identical properties and (dis)advantages. It dismisses the idea of the 'sameness' of a person as bodies go through corporeal alterations over time, through childhood, maturity, old age and death (Turner 1996:30). We may also consider less attentive, physical differences in weight and length as potentially important social factors (but not necessarily only in the sense of physical strength). Paul Higate (1998:191f) has made some interesting notes on body size and status in hierarchical organisations such as the US military. His examples indicate that characteristics such as body length often confuse and interact in otherwise formal and strict hierarchical situations. We may add several other, less striking, phenotypic

characteristics (hair colour, nose and ear shape) that may or may not be socially significant in a given case. Of course, many corporeal traits may be purposely hidden or rearranged for many purposes and there are also corporeal aspects which are invisible 'inside the body'. Such internal variations may nevertheless be exposed by their effects or lack of effects (e.g., menopause, genetic abnormalities, deafness, etc.). To grasp a little of the social complexity, we would thus profit by attending to a wider array of corporeal aspects than the usual sex, age and race. The archaeological application of such a corporeal approach is not evident. Some corporeal characteristics, but not all, may be distinguishable in the archaeological record (e.g. burials) and some of these, but not all, might have been socially significant in the given social formation. I shall return to the particular archaeological aspects of corporeality in Chapter 3. So far, we may settle with the recognition of social subjects as multifaceted and changeable actants with varying means of agency and exposed to different subjectivation processes.

Subaltern practice?

It is commonly argued in post-structuralist theory that the social subject is fleeting; a displaced, multifaceted thing which acts according to a disparate number of subject positions rather than from a consistently unified ego (e.g. Zizek 1989:174). The numbers of available positions in a given social situation are not endless, but are delimited by, e.g., position in time and place, power relations, access to resources and social subjectivation processes. From this point of view, the social subject is caught in a web that forces or lures us to occupy a number of available positions (an anthropologist would perhaps have preferred the terms *roles* or *social relationships*). It is thus an authoritarian, although flexible, systems perspective that has difficulties in accounting for individual creative agency. To claim that the subject is equivalent to discourse is as narrow-minded as claiming that the ego is autonomous and self-defined. We may rather say that discourse 'leaks in and out' of the self and thus constitutes a structurating process (Alcorn 1994:19, 27, 40). Structural perspectives on agency also suffer from a too strict apprehension implying a binary relation between what is excluded and what is not within a given frame (i.e. discourse). Such definite discourse or paradigmatic perspectives have problems in defining how rules of inclusion/exclusion are established or how social change is possible in such a static system. I will attempt to point out the possibilities of employing discourse theory without such rigid presumptions: a framework that also allows non-discursive action (cf. Fahlander 2001:17f; Cornell 2001). The key point is that social

action is seldom so strictly organised; it only seems to be so. An example that may serve as illustration can be found in Spivak's well-known example of the *subaltern* (1988; cf. Cornell & Fahlander 2002a). Spivak argues that the subaltern is always doomed to be censored or misunderstood. Her example is well picked. In 1926, in colonial India, a teenage girl involved in the resistance movement failed to carry out an order to kill. Instead, she chose to take her own life, perhaps as a public protest. Before she went through with her mission, she awaited her period to make sure that no one would claim that she had killed herself for other reasons. Nonetheless, despite her precaution, the official and accepted story is that she killed herself because of an unwanted pregnancy (despite the fact that her menstruation was known at the time). Spivak's point here is that a subaltern cannot speak; it can act, but it will not be recognised. The Indian girl could have taken as many precautions as she wished, but as a subaltern (as woman in a patriarchal ideology and as a colonial subject) her actions would never be recognised as intended (Spivak 1988:307f). Spivak is quite rigid on this matter; a subaltern is always silent; if a voice is recognised, it has ceased to be subaltern. It is a matter of power relations between colonial interests versus the interests of the subjected.

We may, however, develop Spivak's arguments in a slightly less strict, 'Foucauldian' direction and suggest that the girl's 'statement' *(énoncé)*, a political suicide, was not recognised for a number of reasons. One interpretation of this particular situation is that her statement was *misunderstood* or was simply *unintelligible* to others. That a girl could commit suicide because of an unwanted pregnancy was tragic but perhaps understandable and was known to have happened. But the idea that a girl would kill herself for political reasons made no sense to people at that particular time and place. Thus her statement needed to be rephrased to something that made sense. Such interpretation makes the official version more likely to be accepted, as it matched the conceptions of a wider 'audience' than the regime. It thus seems possible to act in contradiction to the 'rules' of a discourse but that such 'statements' are seldom recognised as such. They are rearranged and manipulated to fit a common idea of normality and matter of sense. A similar process can also be found in the unconscious *repressions* of knowledge; we sometimes 'know' but we do not let it matter (cf. Alcorn 1994:33). The example of Spivak may be questionable in many respects regarding background, authenticity and sequence of events, but examples of contra-discursive agency are for obvious reasons hard to detect. We have little means of knowing how often such actions are carried out, but perhaps this is done more frequently than we normally acknowledge? One thing is certain; such a margin for

agency is actually a condition for making social change possible (without external causes). If we continue to explore the hypothesis of 'translation' instead of simple ruler-subject relations, we find that the rules of discourse (what is repressed/understandable or not) are constituted on a structurating basis between agency and structure. The rules of discourse are thus not superstructures, mysteriously laid out by an invisible hand, but rather *residues* of the constitutional processes of the individual. This possibility finds support in Lacanian psychoanalytic theory of the constitution of the subject, especially in the work of Slavoj Zizek.

Mind the gap!

Zizek has in a number of texts approached the subject of ideology from various sides (1989; 1991; 1992; 1999). The bases for his arguments are to be found in awry readings of Hegel and Marx 'translated' through the psychoanalytic theory of Lacan. It is not necessary to be familiar with the works of the first two to follow Zizek's arguments, but a general knowledge of Lacanian theory will be required. Lacan's work is, suffice it to say, quite complex and has changed direction during his career (Zizek 1989:73, 133). The body of theory has also taken different directions between generations of scholars as well as between schools of thought (Laclau 1989:ixf). The following discussion is not so much an orthodox reading of his texts, like the ones we find in text-books and readers, but rather a critical reading focusing on its implications for social theory (cf. Zizek 1991:viii; Alcorn 1994:19-25).[3] It is probably a good idea for the reader who is not acquainted with Lacan to put aside any presumptions about psychoanalytic theory based upon ego-psychology or popular psychology; Lacan's 'return to Freud' is a return to the unconscious (the *social* subject) in opposition to theories that put the Ego in the centre of the individual psyche (Lacan 1972; 1995; cf. 1988b:10, 44).

Lacan's psychoanalytical framework is centred on a triad of abstract concepts; the symbolic, the imaginary and the Real.[4] The *symbolic* is a network of references, a play of significants if you like, that makes things and relations intelligible to us. It can be equivalent to an abstract translating matrix that makes us able to speak about a 'table' knowing that the general idea of what such furniture usually looks like and is used for will be understood by others. The symbolic precedes us as subjects and is thus something we need to relate to in our constitutional process (Lacan 1988b:20, 29, 116; 1996:56, 109). It is the general frame of reference, but not necessarily the final frontier of thought and practice; the symbolic is not static but is affected by social practice and changes in the material world.

It is not a totality but is rather reproduced in a structurating manner. The symbolic machinery is not an external entirety; it is an outcome of the gap between the Ego and the Other in the ongoing process of social constitution.[5] The symbolic is sometimes expressed in terms of the *symbolic order* or the *big Other*. The concepts are often equal to each other (Zizek 1989:72; 1996:125; 2001:109), but the latter is better described as the always present Other, the third eye. The big Other is present in both practice and inner dialogue, similar to Freud's concepts of the Id and the super ego; that is, the conscious and moral or a sense of norm and normality, the common practice, how things are done (cf. Sjöholm 1996:9). [6]

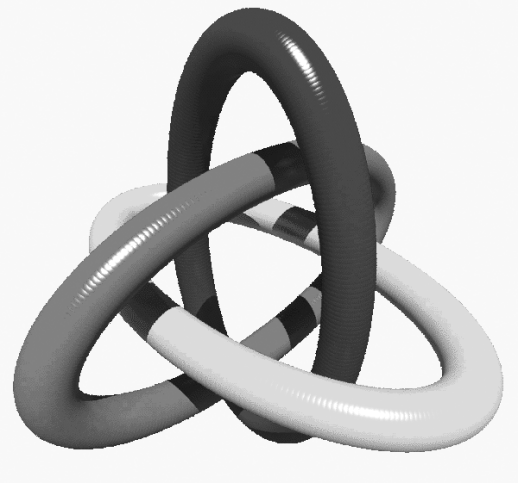

Fig 1. The Borromean knot. A 'simple' illustration of the interrelations of the dimensions (rings) of the Imaginary, the Real and the Symbolic.

The problem is that the internalisation of the symbolic never wholly succeeds; there is always a remnant, a spot of traumatic irrationality and meaninglessness left unsymbolised (Lacan 1996:56; Zizek 1989:43; Zizek 1992:239). We are never able to see 'it all', as the symbolic universe is never complete. The remnants, *symptoms,* are part of the Real that imply a nagging insecurity within the subjects that the world is not complete and fully understandable. The Real is not to be confused with reality. *Reality* "is simply the 'field of symbolically structured representations', the outcome of symbolic 'gentrification' of the Real" (Zizek 1992:239). The Real is rather what

escapes (refuses) symbolisation. It can be traumatic and foreclosed matters or simply be something non-discursive and unthinkable (Lacan 1977b:279f; Zizek 1989:69, 169ff; Miller 1996:25; Feldstein *et al* 1996:39-237). Pieces of the Real can sometimes be dimly visible as 'gaps' in the symbolic, but the paradox is that we cannot speak about them before they are symbolised and have thus ceased to be Real. This is one reason why the Real is frightening and traumatic; it reveals the gap between how things 'really are' and how we actually 'see them'; it exposes the lack of intersubjectivity and collective understanding of the symbolic (cf. Bauman 1992:8; Miller 1996:26; Zizek 1999:14). It is thus not surprising that Zizek employs horror stories of popular culture as illuminating examples to expose elements of the Real (Zizek 1992; cf. 1996:17).

The *imaginary* dimension is the phenomenological, subjective (but not necessary false) experience of the world (perceived or imagined). One facet of the imaginary is found in the relations between the symbolic and the Real; we need an intermediate 'layer' that masks an unsymbolised Real core. The imaginary thus functions like 'plaster' that fills the cracks and empty spaces of the symbolic order (Zizek 1989:171). In our constitutional process of becoming subjects, we need to internalise a ready-made (but not complete), symbolic order; we are limited to choosing something 'already chosen'. 'The subject who thinks he can avoid this paradox and really have a free choice is a *psychotic* subject...' (cf. Lacan 1977a:185; Zizek 1989:166; Thomas 1989:102). Nonetheless, what precedes us is not static but a *changeable* and *incomplete* translating matrix (symbolic order). It is left for us to create our own set of imaginaries and by our actions sustain or alter that matrix (Lacan 1977a:81; cf. Alcorn 1994:32, 37).

To summarise the arguments: We are born into a world that is to some extent already symbolised, that is, made intelligible and coherent, which help us to learn to engage in society. What we experience as 'reality' is thus what is already symbolised (Zizek 1994:21). The symbolic order is, however, not complete; it is full of gaps and contradictions. These irregularities, i.e. *symptoms,* are artefacts of the processes of social constitution and symbolisation. This implies that different individuals in different ways and with varying degree of success internalise parts of the *constructed* symbolic and *modify* its structure by imaginary, *ad hoc* tapestry. The gaps and lack of consequence in the symbolic, along with personal internalisation and imaginary tapestry, suggest that we may expect a certain level of multivocality and plurality in world-view *within* culture areas or societies (i.e. symbolic orders). It is a play of repeated misunderstandings; we believe that we know what we are doing and we believe that we know what we believe. The paradox is that the sym-

bolic seems to work surprisingly well, despite its lack of completeness. One answer to that paradox is found in the dual facets of the imaginary. It is partly personal, we all have different apprehensions of the world and ourselves, but it also has a more general collective dimension, *ideology* (Zizek 1989:33).

The sublime ways of ideologies

Zizek chooses to use a wide concept of *ideology* in his analysis of contemporary society and popular culture. Zizek's definition of the concept of ideology is, unlike many others, not simply about false consciousness, but what appears as *social reality*, whose existence is based on the subject's lack of knowledge of its Real essence.

> ...ideology must be disengaged from the 'representationalist' problematic: *ideology has nothing to do with 'illusion',* with a mistaken, distorted representation of its social content. To put it succinctly: a political standpoint can be quite accurate ('true') as to its objective content, yet thoroughly ideological; and, vice versa, the idea that a political standpoint gives of its social content can prove totally wrong, yet there is absolutely nothing 'ideological' about it (Zizek 1994:7, *italics* in original).

This definition of ideology goes beyond notions of differently empowered individuals who are in various ways able to see through the illusion as a means of achieving power. An ideology would not be an ideology any longer if we discovered its Real kernel (Zizek 1989:21). Ideologies in Zizek's terms are the collective, imaginary tapestries that obscure the ugly, the cracks and meaninglessness, which is often too traumatic to be reproduced without subtle alterations/sublimations (1989:28). Ideologies are thus empty gestures of the Symbolic that make us accept a given, meaningless reality as our own social construct (1989:230). One example of how this can be manifested in social practice is the burial ritual. We cannot escape the fact that we are mortal beings, but, according to Zizek, we try to give that irrational, natural process a meaning by burial rituals. By repeating such symbolic practices, we seek to 'transform' something traumatic, over which we have no control, pretending that we have a free choice (1989:219). The ideological tapestries are, however, not only present at the structural level but affect our day-to-day, mundane practices and inner dialogue as well (Zizek 1996:104).

The interesting aspect of Zizek's social readings of Lacanian theory is that they illustrate the complex relations between social heterogeneity and social homogeneity, as well as the past and the

present. Ideology is both disguised for us but is at the same time our own construction; a kind of artefact from the social constitutional processes. It is thus very much a human creation, which also to some extent departs from our specific biology and psychological constitution as subjects (Fahlander 2001:97f). The ready-made (yet not complete) symbolic order surely accounts for much of the homogeneity in thought and practice of a given time-space. The uncertain relations between what is symbolised and elements of the Real imply a need for an imaginary dimension to make concepts like meaning meaningful and matter of sense sensible. This reasoning thus undermines generalising concepts like 'cultural meaning', intersubjectivity, collective representations and cultural horizons as to a great extent *imaginary illusions.* It is important here to note that Zizek's concept of ideology is not a complete and static set of 'translations'. Any ideology always consists of symptoms that make a closed totality impossible. Elements of the Real sometimes 'pop up' and disturb reality as we experience it. This incompleteness and inconsistency are important, as such gaps and anomalies imply social change as a logical consequence rather than just a possibility. Nonetheless, there is a problem with Zizek's definition of ideology in that it tends to represent virtually everything. Such a wide notion of the concept reveals something of its complex nature but suffers the risk of being self-explanatory and toothless in concrete analysis. It is more likely that we should expect several parallel, conflicting or contradictory *ideologies* (which is also hinted by Zizek in his discussion of ideological struggle). Zizek's general use of the term is, however, sufficient for this general discussion.

With the notion of ideology as an imaginary residue of social constitution processes, we can return to the subaltern and Foucauldian discourse theory. The rules of exclusion and inclusion in Foucault's discourses and episteme are very similar to the work of ideology. The *episteme* is a general network of codes, a system of reference for thinking and theorising, that generates ideas, sciences, and so on in the long term (Braidotti 1991:50; Ritzer 1997:43). The concept of *discursive formations,* are more like regularities /irregularities among discursive 'objects, types of statement, concepts, or thematic choices' (Foucault 1972:38, 107). Foucault has been criticized for not being able to explain how discursive formations or epistemes suddenly change and why they seem to be so fundamentally different. Zizek offers a model that may help to illustrate how such changes in thought and practice are possible. He suggests that ideological struggle circles around certain *nodal points* (or master signifiers) that totalise and fix the meaning of 'floating signifiers' and proto-ideological elements' (1989:87). In other words; discursive rules set the constitution of the symbolic order, that is, governing

the interiorisation of the Real in the symbolic, as well as defining the elements of subjectivation and their social significance. Zizek suggests that, when an ideology is significantly altered (i.e. changing nodal points) the understandings and logic are fundamentally changed as well. This implies that individual statements or practices of different ideologies are not commensurable. A 'statement' from Foucault's work on the birth of the clinic (1994) may serve as an illustration of such lack of understanding of 'meaning' or 'sense'. The following medical account of the treatment given by the physician Pomme is a most peculiar story which, at least to me, seems strange and almost incomprehensible (cf. Sunesson 1992:ii).

> Towards the middle of the eighteenth century, Pomme treated and cured a hysteric by making her take 'baths, ten or twelve hours a day, for ten whole months'. At the end of this treatment for the dessication of the nervous system and the heat that sustained it, Pomme saw 'membranous tissues like pieces of damp parchment ... peel away with some slight discomfort, and these were passed daily with the urine; the right ureter also peeled away and came out whole in the same way'. The same thing occurred with the intestines, which at another stage, 'peeled off their internal tunics, which we saw emerge from the rectum. The oesophagus, the arterial trachea, and the tongue also peeled in due course; and the patient had rejected different pieces either by vomiting or by expectoration (Foucault 1994:ix-x).

Foucault admits that the example is somewhat extreme, but not unique in the history of medical science. Similar strange accounts are also to be found in *Madness and Civilisation* (Foucault 1989). Of course, such ideologies and discourses are never totally different from others; there are limits for possible thought and practice. The 'bricks' that make up social formations are not essentially different; we share a general biology and a limited variety of material conditions. In that respect, not all historical or archaeological 'statements' are necessarily totally obscure to us. As we shall find out further on, some 'nodal points' seem more persistent than others, to use Zizek's metaphor. The question is how we should analyse thought or practice constituted within (or between) different ideologies/discourses. It would not be sufficient to reconstruct a symbolic scheme (e.g., Hodder 1982:185) or a symbolic order. We need also to take heed of the imaginary dimension on the individual and 'collective' levels. These remarks rule out an understanding of what they really meant to the 'authors' (which they would not 'know'), but, as the example of Spivak on the subaltern demonstrates, it is still possible to analyse relations between executed practices in a given context of other

known actions. As archaeologists or historians, we have unique possibilities of making the subaltern heard in retrospect.

The main point of this rather abstract excursion is to show how the intricate relations between the dimensions of the Real, symbolic and imaginary *displace* the question of individual versus society/structure/ideology/discourse. They rather suggest that the relations are neither dichotomous nor a continuum, but a conceptual illusion. The general symbolisation of social worlds (and thus the material) is rather the result of the constitutional processes of the human biology and psyche. To get a grip on social practice, we cannot exclude one aspect for another or fall back on simplified, dichotomous models. Having said this, I will step down from the abstract level and continue to discuss the 'logics' of everyday social practice on a more basic plane.

The logics of practice

Social action or agency are terms that embrace a variety of different types of action. Action can be intended or unintended, have more or less predictable results, be consciously, semi-consciously or unconsciously constituted. It does not necessarily have a given result or predictable further consequences and the range of unintended consequences of action may be more or less evident to different individuals. Many of the things that we do daily are routine actions, actions that seem so obvious that we do not need to reflect on them (unless something unexpected happens). When people interact in regular, day-to-day business, we generally find that it seems structurated to a certain degree. The 'rules' of everyday behaviour may be explicitly outspoken or be based on 'silent agreements' or 'contracts'. The everyday discourse is regulated by a mix of unconscious or (semi)conscious ways of doing things, yet it is not randomly constituted. Many tasks and daily routines are carried out by different individuals in various ways, which are partly the result of pragmatic considerations and 'agreements': if it works, there is little need to change routines. But, of course, division of labour is not simply something that is always negotiable. It is not necessarily formulated on a give-and-take basis but may be unrighteous and ineffective. The division of labour and the ways of carrying out daily routines are all to varying degrees related to ideologies. What seems 'proper' at a given time and place may depend on many aspects: material conditions, traditions. etc., but 'commonsense' is frequently equal to ideology.According to Zizek, the subject's understanding of itself, its surroundings and the affects and effects of agency is blurred and often opaque and misunderstood by the agent and by others (Zizek 1989:31). For a concrete example of the phenomena of 'they don't

know what they do but they do it anyway', we may turn to William Rathje's garbage project (1974, 1981; Rathje & Cullen 1992). The garbage project combined interviews and careful recording of the refuse from a number of households in Arizona, USA. One of the important outcomes of this study was the discrepancy between the quantity and types of commodities that the members of the household claimed to have consumed and what was actually found in their garbage (Rathje & Cullen 1992:66f). In other words; people cannot always accurately account for what they do or believe they do. Not surprisingly, we find a gap between personal experience and actual behaviour. This gap seems to be wider when it comes to behaviour related to ethics, what is proper and what is not. For instance, most households in Rathje's study consistently reported that they consumed a smaller quantity of alcoholic beverages than what was found in their refuse (Rathje & Cullen 1992:71). This was *despite* the fact that they were fully aware that they were being subjected to investigation.

Rathje's garbage project illustrates the fact that social action is not simply unconscious or conscious; it may be a question of neither individual motives nor structural forces. We may add an interjacent, semi-conscious level, so-called tacit knowledge, knowledge of the body, like those actions that we perform but which we cannot explain exactly how we perform them (Giddens 1984:9, 49, 374f; cf. Fahlander 2001:91ff). As embodied individuals, we all possess differently situated knowledge related to body characteristics and the particular material and social conditions. The semi-conscious level of agency may be dimly recognised, we may be able to account for these actions if they are pointed to, but otherwise we simply perform them without much consideration. Here we find many routine actions that we perform on a daily or regular basis which are a major part of our activity (Goffman 1967; Giddens 1984, cf. Tilley 1999:16; but see Conein 1998). Social action can be seen as a duality, a recursive relation between individual motives and the influence of the Other, the normal way of doing things. To be able to analyse the messy business of intentions, unintended effects, and structural implications that constitutes day-to-day agency, we need to find ways of dealing with it in an analytic manner. A necessary first step is to discuss the importance of materialities as regards agency in a time-space perspective.

Serial action and serial categories

Sartre has produced some interesting thoughts on how individuals often act 'together as solitudes'. Especially his concept of *series* is helpful in analysing the collectiveness of individual action. Sartre

has frequently, but perhaps somewhat unfairly, been labelled an idealist or subjectivist, advocating an individual-oriented perspective. According to Sartre, we are neither subjected to biological drives nor necessarily determined by our biographies; we have a possibility of being aware of our existence in the world. *Existentially conscious* individuals may, to a certain extent, be able to choose alternative trajectories or at least depart from the 'discourse of the Other' (the normal way of doing things). Despite the passive constitution of the Ego through experiences and consciousness of states, caught in 'fields of practical inertness', it is nonetheless exposed to its own products (Sartre 1988:292; 1991:46). These 'products' and the temporal (relative) properties of the 'I' (i.e. that it can be conscious of itself as one, or several, historical I's) provide the basis for the individual to stand beside some discursive rules (Sartre 1991:24; cf. Börjesson 1986:160). In this perspective, individuals may, by their semi-independent actions, actually change their social worlds. Needless to say, Sartre's conception of the existential individual is similar to Giddens' knowledgeable agents.

The 'later' Sartre has, however, developed these ideas into a more complex form. In his magnum opus, *Critique of Dialectical Reason,* (1991[1960]) he seeks to merge existential philosophy with Marxist theory. He aims to sketch an image of a social totality, the structural elements, without excluding individual intentions and perspectives. The multiplicity of sociality is not simply described as constituted by atomised and autonomous subjects, but as a totalising process, whereas the social multiplicity is interiorised. This process is not that unlike Giddens' notion of the duality of structure or Lacan's concepts of the Imaginary and the Symbolic. Sartre argues that a larger social collective (a practico-inert ensemble) "makes of everyone *both* a polyvalent isolation (with millions of facets) *an*d an *integrated* member" (Sartre 1991:257). A most interesting notion in Sartre's work that elegantly illustrates this process is that of *serial action* (1991:256-69). Sartre argues that many socially constituted collectives are better seen as *series* (the inert effect of separate activities) than as social categories or groups. To be a member of a *group*, in Sartre's sense, one has to enter consciously, as in a fraternity, and join with a common cause (Sartre uses the example of the French Revolution and the Resistance to the German occupation, 1940-44). Most of what are normally considered social groups or categories do not qualify for this definition. Instead, many social collectives are better seen as momentary series, constituted by the common circumstances of situated individuals (1991:258). As a simple but illustrative example, Sartre discusses a group of people waiting for a bus. These individuals may be of different sexes, have different body postures, belonging to different fraternities etc. and are not inte-

grated as a group in the strict sense but are united by their intention of riding in the same bus. The individuals in the queue would probably neither recognise themselves as a group, nor do they need to share a common social milieu (habitus) or individual, discriminating attributes; this particular series is defined by a 'fluid homogeneity'.

Fig. 2. A bus queue in Paris in the 1960's. A typical series of differently embodied individuals, with different destinations and motives. Unknown photographer.

Sartre's example may seem naive and his distinctions less meaningful, but this perspective may prove to be very helpful in analysing the social plurality. As an example, Marion Young has employed the serial perspective in feminist theory. Feminist theory is troubled in defining a homogeneous category of women as a subjected group (Butler 1990a:324f). The individual situation of women of different social contexts is varied and many do not share the same goals or problems (Braidotti 1991:158; Moore 1994:10f). Their differing experiences make them more of a heterogeneous collective or, in Braidotti's terms; a collective singularity (Braidotti 1991:132). Young's solution to the problem is to see 'women' and the notion of 'gender' as series rather than as social groups or categories. This particular type of subjected series is not defined by biological sex or 'femininity', but rather by the unequal situations that many women face and by the practices that they perform in a patriarchal social

order. It would, for instance, not be surprising to find many women in the queue waiting for the bus while their husbands dispose the family car. In this particular case, we find a direct relationship between a superstructure (gender ideology) with individual agency and materialities. Agency is a key-word here; the series is not necessarily defined by the intentions of the subjects, but by their actions. The people in the queue have acted in different ways to situate themselves at that particular point in time and space. Marion Young's argument is thus partly sustained by Butler's definition of gender as 'stylized repetition of acts' (1990b:140; cf. Bordieu 1990:69f on 'hexis'). Sartre's concept of series is thus very fit to describe social categories as the result of both individual agency and motives, as far as social subjection/subjectivation processes and other structural force are concerned. To make Sartre's concept of serial action even more useful, we may add a notion of *serial categories.* Such categories are defined from a *number* of repetitive serial actions, instead of single, momentary series, as in the bus-queue example.

Most individuals participate in a number of serial collectives during their day-to-day activities. After the bus queue, some individuals may join another type of series at their workplace and so forth. All the serial memberships that a person is involved with do not necessarily constitute a part of social identity. The type of profession can mean very much to some, while others base their identity on other grounds, but very few define themselves as 'public transporters'. From this, it follows that, from the perspective of executed action, we may speak of individual, momentary, *serial situations* in favour of subject positions or status-roles, and *serial categories,* instead of social categories. Thus, Sartre's concept seems effective in discussing agency and social processes as it is in describing individual acts from a structural point of view.

Materialities and serial action

An important point that Sartre makes (which is found only implicitly in Giddens) is that agency is not simply a social phenomenon. Agency is constituted (enhanced or prohibited) by the material conditions of action. Social relations can rarely be an affair only between social subjects; materialities are always involved in one way or another (Gosden 1994:77; Latour 1998). Series of individuals are not only acting according to intentions and place in space; they are formed in relation to the *material world.* In the example of the bus queue, the bus, or the presence of a communal transporting system, is the central node around which this particular series is located. The bus is, however, not simply a material 'symbol', but a cog in the complex machinery that makes up the typical life of the city (Lynch

1960). The material constitution of the bus, its properties (speed, number of seats, etc.), is not unimportant. The series depends on these factors. To this, we can add other sociohistorical phenomena, such as the way in which buildings and streets are formed in relation to the need of public transportation etc. This is a vital aspect that is only marginally implicit in, for instance, the structuration theory of Giddens (Fahlander 2001:61). That objects and materialities are involved in the social process is nowadays well recognised as comprising a growing field of research. The 'socialness of things' has been stressed in various strands of social theory, including archaeology (e.g. Latour 1991; 1992; Riggins 1994; Gottdeiner 1995; Andrén 1997; Komter 1998; Schiffer 1999; Preda 1999; Graves-Brown 2000; Cornell & Fahlander 2002a). These attempts at incorporating things, artefacts and aspects of the environment, as social *actants* in the social process, range from symbolic to hard-data analysis.

The potential of objects and materialities to be social is not a simple matter of being active or not; as actants, they may work in various ways. Materialities may simply be 'good to think through' as Lévi-Strauss claimed (1966), or function as metaphors or vehicles for the mind (cf. Tilley 1999). Materialities may have an almost determining effect on people. One can be constrained or triggered by objects, consciously or unconsciously. They may be produced or appropriated with specific intentions and yet influence future actions in an unpredictable way. Indeed, some objects are indispensable for a typical way of social life. The built-up environment is as much an active generator of social behaviour as it is constituted by it. One example is houses and buildings. Houses are living, and to some extent active participants in society, like any human (Bailey 1990:28). They are 'structurating structures' (Donley-Reid 1990:349; Parker Pearson & Richards 1994; cf. Bourdieu 1977:90; Cornell & Fahlander 2002a:123ff). They are, like buses, nodes for repetitive action, owing to their inertness and resistance to change. Like the social subject, buildings are seldom static but have varying biographies. Buildings may be erected for one purpose but re-used for other purposes; for instance, a house at a certain stage of decay may be converted from being a dwelling to be used as a storage room or stable.

An illustrative example of this kind is to be found in the archaeology of Michel Foucault. The 'things' and mute monuments in Foucault's archaeology are mainly texts, but he does discuss materialities, especially buildings, such as hospitals, prisons, factories and schools. For instance, in *Madness and Civilisation* (1989), he argues that the very existence of separate and isolated *leprosariums* in early 16th-century Europe played an active part in the process of distinguishing the mad as a social category. The mere material existence of leprosariums perhaps stimulated, if not evoked, that process by their

very presence. In *Discipline and Punish* (1979), Foucault addresses the structure of prisons, schools and factories as a material manifestation of the modern discourse of the increasing surveillance and discipline of bodies. In a similar sense, material objects interfere in the structuration process. Paul Treherne provides an archaeological example of a similar mutual process between social process and materialities. He argues that the masculine warrior ideal of the Bronze Age would not have been possible without the possibilities and properties of the new material (Treherne 1995). A similar example is to be found in the hero-cult of the Geometric period. Ian Morris, among others, argues that the Greeks of the Iron Age probably had no accurate apprehension of the prehistoric predecessors who built monumental graves. But by being there, the graves were charged with social significance and came to play an important role in the formation of the early city-state (Morris 1988:756). There are many other implications of this subject, which unfortunately go beyond the scope of this text. I shall, however, return to some archaeological implications of materialities and social action further on in the particular section of this chapter.

Temporal and spatial aspects of materialities and serial action

Most people are involved in more or less complex patterns of 'serial routes' in their daily, weekly or annual routines. Such temporary leaps in and out different serial collectives share many similarities with the time-space geography of Hägerstrand (1970; 1985). Social individuals tend to follow recurring time-space paths in their day-to-day lives (Fig. 3).[7] For some of the people waiting at the bus stop it is a singular trip that they are about to make, but many of them will probably be in the same queue at the same time the next day and so forth, perhaps for transportation to their workplaces. Hägerstrand and other time-geographers point to the regularities in movement (as solitudes or as series) when they carry out their everyday tasks (projects).

Serial routes include various forms of environmental and social constraints, like the need for regular sleep and food, the type of available transportation and social restrictions, like laws or norms (cf. the old lady's route in Chapter 1). These constraints are not generally valid for all individuals and have temporal implications. For instance, a city park may be a popular place for most people during the daytime but is not safe for women at night. Time-space diagrams and access-diagrams may prove helpful in the archaeological analysis of social action in a given locale. Contrary to ordinary layout plans, access analysis also involves the schematic representation of movement and control, visibility, and identification of private and

public spheres, from the viewpoint of different users of a building, rather than the actual spatial arrangement of rooms in a building (e.g. Fairclough 1992:351). In this sense, we may find a relation between the imposed forms of buildings and the general aspects of the given social environment.

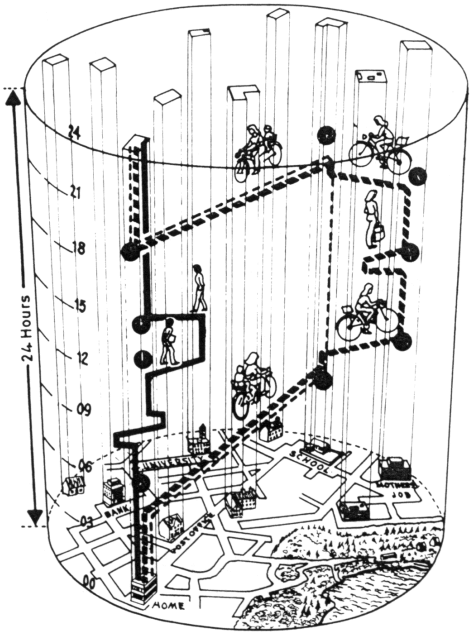

Fig. 3. Time-space diagram of a serial route in a small community (from Rose 1993:21).

William Whyte's (1954) study of how the 'word of mouth' works is an illustrating example of how a seemingly random, spatial pattern of materialities can be better elucidated from a microperspective of serial action than from general behaviourist theory. His example is well picked; a neighbourhood consisting of similar, 12.000-dollar houses, inhabited by fairly homogeneous, white-collar couples in the ages of twenty-five and forty who earn between 5000 to 7000 dollars per year. The white X's on some of the roofs in Fig. 4 indicate houses with air-conditioning installed, a recent commodity that was not as common then as it is today. Despite the homogeneity of the neighbourhood and its inhabitants, we find that the conditioners are unevenly spread and clustered. Whytes' explanation of this pattern

is that the word of mouth, the social contact between the families, is primarily spread between next-door neighbours and across the backyards but not across the streets. The reasons for this clustering depend on the given material conditions of the area, the outline of the houses and the neighbourhood, but also on the social practices of the members of the households. When Whyte interviewed some of the couples, he found that one reason for the stronger backyard connection was that social contact between the families was initiated by their children, who play with each other primarily in the backyards of their houses.

Fig. 4. The picture shows a residential area of Philadelphia during the 1950s. The houses that have air-conditioners installed are marked with a white X (from Whyte 1954).

There are, of course, webs of complex relations that are involved in this specific case, but it is interesting that the roads (designed for communication) actually inhibit social communication. In archaeology, we often find similar, seemingly random, patterns and clusterings which to some extent may be better understood from a microperspective. A traditional analysis of a similar pattern on an archaeological site (with the X's indicating houses with finds of a special ware) would probably seek connections between all X-houses in the whole area. These non-related households are likely to be bluntly interpreted as signifying a status or ethnic category, that

is, simply re-assigning a material pattern to a social category. Instead, we may see the houses with air-conditioners as clusters of series which can be analysed in various ways. In this particular case, it is evident that the most plausible explanation of the constitution of these series is found by employing horizontal stratigraphy, but we could also analyse the pattern from economic, social, etc. relations. Of course, it is not only cultural objects like buses, houses and commodities that structurate serial agency. The material implications in social life do not discriminate natural from culturally modified materialities. For instance, I will exemplify in the case study of the Asea valley (Chapter 4) how natural features of the landscape may act as *signifiants,* that is, nodes of structuration, imputing agencies and interfer in the social structuration of space. The term signifiant is intentionally chosen to point out its arbitrary 'content'. It is not necessary to understand *why* it works as a structurating node; the important thing is that it can potentially fill such a function.

To sum up: Sartre's notion of series highlights the collective character of social life. Indeed, it seems that structurating practice is often regulated and routinely performed and often structurated in time-space paths in relation to the material world. Individuals may in many situations act and think as solitaries, but they nevertheless reproduce patterns according to general, social and material conditions. It is interesting to note that the serial perspective does not necessarily depart from what the agents may think of the materialities that surround them. Of course, individuals apprehend materials in various ways; some they find ugly, others are practical, some we like, some we don't, and in everyday life we do not think about most of them in particular. For instance, it is evident that some people do not like buses and seek other ways of transportation; these we probably found in other series concentrating on other materialities like cars or bicycles. Sartre mentions, in addition to serial praxis, the existence of serial feelings and serial thoughts: hence, "a *series is a mode of being for individuals both in relation to one another and in relation to their common being* and this mode of being transform all their structures" (1991:266). Here Sartre expresses a similar attitude to being in relation to the Other as that found in Lacan's psychoanalytic theory. The key words in Sartre's perspective are the slow-flowing inertness of social practices, with materialities functioning as nodes in the structuration process. We also find interesting connections between individual action and the more general aspects of social structures. What is more important is that Sartre's serial concept is a theory of social action or, more specifically, *executed action.* In order to make these notions operative, we need to invent tools for

discussing social action in general and in particular. Here, we find that the concepts of series, serial routes and serial categories can be complemented by introducing the concepts of *structurating practice* and *structurating positivities.*

Structurating practice and structurating positivities

Given the discussed conditions of action, individual or collective actions are to a great extent regular and routinely performed. The temporal regulations, intentions and experiences of individual action may differ, but social action tends to follow patterns in time and space in relation to the material world. This is, however, not to say that social action is uniform, without exceptions and particularities. What we can learn from Sartre, Giddens, Lacan and Foucault is that social actions are interconnected in various ways; culture, discourse or 'the common way of doing things', are interiorised and appropriated in varying degrees in the social subjects, their actions generally follow fluid serial patterns often spatially and temporally concentrated around nodes in the material world. Giddens' notion of the duality of structure does not account for how the structuration process works at the particular level (Giddens 1993:6-7). For instance, Arthur Stinchcombe has remarked that agents are often more rational in some situations than in others (1986:5f). Similarly, Mouzelis (1989) emphasises that agents act in relation to other agents that are differentially structurally situated and empowered. Margaret Archer (1982) notes the obvious fact that there are social situations in which structure is a greater determining force and, similarly, there are areas with less structural impact.

This ambiguity in structuration theory must, however, be considered an advantage. There can hardly be a general theory that specifies the relation between all social factors and materialities. A focus on the micro-level of practice does not necessarily imply that we lose sight of the more general, structurating elements that precede and interact and are thus possibly reconstructed by the outcome. Garfinkel (1967), Hägerstrand (1970:9), Goffman (1971:11; 1974) and Collins (1981) have all argued that many important aspects are to be found outside the time and space of the social situation and that these 'outside' relations are often hidden or unnoticed by the agents involved because they are taken for granted. Whyte's example of houses with air-conditioners can also be employed to illustrate these relations. It shows some of the ways in which local individual action at a given time and in a given space can be related to the more general structures (ideology) of the American society of the 1950's. For instance, the houses were built for, and used by, typical, western, nuclear families, normally with

the wife working at home, responsible for the care of the children. In another setting, let us say one in which nannies, instead of the housewife, look after the children, the backyard contacts would probably be less frequent between the individuals who have interest in and power to affect the equipment of the house. Social action is thus not simply a matter of individual intentions and motives but is always related to a larger frame of reference. If the assumption is correct, that social practice is constituted by both general and local, structurating elements, it would imply that individual action involves a bit of both. This relation between the general and the particular makes it possible to move from detailed studies of particular events to the general aspects of social life. We may thus view the performed activities of a particular situation as *mediating* the particular and the general. Social practice is in different respects a result of the properties of the particular situation, but these cannot be seen as necessarily unique, as they also include traditions, institutionalised power relations and other aspects of the 'outside' world.

The fact that the subjects involved in a situation or event may have different opinions about what happened, why it happened in that way and its future consequences is not very important here. By focusing on executed action rather than subjective experience of action, we do not need to restrict our analysis within any particular, social or regional frame of reference. In general, studies of social action are commonly related to certain frames of practice, such as societies, regions, nation states, ethnic groups or cultures. This is because such entities are believed to encompass a general homogeneity regarding norms, laws and cosmology, in which symbolic universe all social action finds its inspiration and thus its meaning. Now, structurating effects are seldom confined within such narrow boundaries. For instance, Giddens argues that "All societies are both social systems, and at the same time, constituted by the intersection of multiple social systems" (Giddens 1984:164). The notion of the cross-societal nature of structural elements implies that ideological and political structures are not confined by a single social formation but may also be shared and interconnected with other formations in regional or global networks (cf. Wallerstein 1974-89; Ekholm 1981; Friedman 1994).

As a means of analysing social practice as multivocal and 'unbound' by social and spatial barriers, we may find the concepts of structurating practices and structurating positivities operational. Acknowledging the routine character of most social practice and the serial perspective, we may distinguish *structurating practices* from the seeming multiplicity of agencies. Structurating practices are the regular and serially constituted actions. They are not to be under-

stood as just particular, regulated, recurrent activities of an institutionalised or ritualised kind. There are structurating aspects in most social practice, although their frequencies vary. In contrast to concepts like tradition, ritual and culture, such practices are often in different respects opaque to the individuals who execute and monitor them. This is, however, not the same as discursive practice; structurating practice is not necessarily related to a particular discourse. Structurating practices are those recurring routines that we perform with some regularity, day-by-day, weekly, monthly or yearly. Such practices may be related to daily necessities, like eating and sleeping regularly, or related to aspects of subsistence. Structurating practices may also be of a more 'cultural' character, that is, relating to games and festivities, aesthetic values, ritual activities, etc. It is probably true that there are structural elements of almost anything we do, but such wide definition would not be operational or helpful. Giddens defines the structurating process as 'knowledgeable monitoring of action'. The awkward term 'knowledgeability' constitutes a problem of its own, but we may generally stick with Giddens' notion of 'the collective factor'. Structurating practices are thus normally restricted to actions that are social in the sense that they are *observable* (but not necessarily understood) by others. There are exceptions though; singular acts have a *potential* for being structurating if they have observable *results* or are materially manifested. Structurating practices are, like serial practice, mainly performed in relation to materialities. This key-point implies a good chance for us to abduct action through its persistent traces in the material world.[8] One field that includes most of these aspects is structurating practices related to the disposal of the dead. A recent example of a single event that, unintended, seems to have generated a structurating practice is the burial of the late Swedish Prime Minister Olof Palme. Before his official burial, white coffins were a rather rare choice of Swedish citizens and, if such model was chosen, it was generally for women. But after Palme's broadcast funeral, in which he was seen to be buried in such a coffin, that model became increasingly popular for both sexes.[9]

The second concept, *structurating positivities,* is less basic; it refers to general patterns of action spanning over longer periods of time and space. Structurating positivities are basically clusters or bundles of more or less resultant, structurating practices. From an archaeological point of view, structurating positivities are normally traceable as fragmentary elements of structurating practices and hence in the formation of the material evidence. The concept of structurating positivities differs from Giddens' concept of structurating principles and Lévi-Straussian structures in that they do not have any given or essentialist content. They are not superorganic

structures but are rather produced, reproduced and changed by the serial and structurating action of individuals. Like Giddens' structures, structurating positivities may be more or less evident among some of the individuals that (re)produce them but may also be opaque to others. The second part of the term, positivity, points to their general, inert, substantial and persistent nature. Structurating positivities are not 'things' in the Durkheimian sense; a structurating positivity is not defined, nor does it define, anything in particular (cf. Cornell & Fahlander 2002a:66). Despite their virtual character, structurating positivities can be very powerful homogenising factors. They are not confined within, nor do they define the typical ways of a certain social collective but are likely to transcend social, temporal and spatial boundaries. Such positivities do not work in solitude; there are often inconsistent, structurating practices present that contradict and challenge the 'normal way of doing things' within a certain social frame.

We may recall Zizek's discussion on ideology; the concept of structurating positivities is very similar to Zizek's 'nodal points'. Some structurating positivities are very persistent, but when they dissolve or change, it generally has serious implications for social thought and practice. I have already touched upon one general positivity which may serve as an illustration: the patriarchal order. The subjectivation of women in the contemporary world has many practical implications for both sexes, for instance, the plausibility that we will find many women in the bus queue. We can add a number of similar social situations and types of agencies that are found to be feminine and 'natural', according to the general structurating positivities of patriarchy and heterosexual normativity. Here we also find a connection with individual physiognomy and the social importance of corporeal properties apparent in the bipolar division between men and women. The concepts of structurating practice and structurating positivities do not require that the subjects must be regarded decentred in favour of structural determinism or of a superorganic, invisible hand governing social action. The serial perspective of performed action makes it possible for us to analyse the reproduction of dominant discourses and categories (structurating positivities), as well as the conflicting and opposing, structurated practices which make social change possible.

Fibres and threads

The relation of structurating practices and positives does not comprise a solid system. They do not account for a full picture of social life, nor do they correspond neatly with each other. On the contrary, there is likely to be contradiction and fission between different

structurating elements and individual action. One way of describing their relations would be as a rhizome network (e.g., Deleuze & Guattari 1988:3ff). Structurating positivities find their material bases in various ways, for instance, in the structuration of space, and the outline of buildings. To illustrate how these may be related over time and space we may turn to Wittgenstein's discussion of family resemblances, a conception very much like Sartre's concept of serial action. Wittgenstein discusses "the various resemblances between members of a family: build, features, colour of eyes, gait, temperament, etc. etc." The family in Wittgenstein's sense is polythetically defined; there is no direct relation or a given element that we find in all members of the family. The family resemblances rather overlap and criss-cross each other, like the way a thread is spun by twisting fibre on fibre. "And the strength of the thread does not reside in the fact that some one fibre runs through its whole length, but in the overlapping of many fibres." The only thing running through the whole thread is "the continuous overlapping of those fibres" (Wittgenstein 1953:§67).

Fig. 5. Fibres (structurating practices) of different lengths (over time and space) that correlate and make up threads (structurating positivities) that, if intervening, can even come to constitute a rope.

Wittgenstein's metaphor of fibres and threads gives an illustrative example of how structurating practices and positivities are related. The fibres correspond roughly to structurating practices and can be woven into each other, forming threads (structurating positivities). The keypoint in Wittgenstein's metaphor is that the threads are made up out of fibres of different lengths. The fibres are momentarily woven together but do not remain so forever. If some fibres suddenly cease to correspond, the thread may either dissolve or take another form by comprising other fibres. The metaphor of Wittgenstein can be extended further by letting threads make up ropes on a higher level of temporal and spatial generality (regional/global, structurating positivities). The metaphor of the thread is, however, not to be taken literally. The relations between structurating practices and structurating positivities do not form a closed, coherent system; it is perhaps more relevant to speak of clusters of fibres, more in the shape of 'dust balls' or molecules than a straight, consistent thread. Structurating positivities are thus not some determining, never-changing, structural force in the traditional sense. They are composed out of clusters of structurating practices and are less likely to persist if one or more practices changes.

The microarchaeological concepts of structurating practices and structurating positivities and the metaphors of fibres and threads provide a general way of discussing social action. They are not particularly new inventions; similar notions are to be found elsewhere, for instance, the structurating properties and structurating principles of Giddens' structuration theory.[10] These are, however, only seemingly similar in nomenclature; Giddens' framework is a bit too stiff and typical for modern societies. Examples of structurating principles for tribal societies are 'operating along axes of tradition and kinship'; writing is another principle related to class-divided, state formations etc. (Giddens 1984:180ff). This kind of framework is not very different from, for example, Service's band-tribe-chiefdom-state scheme or Fried's (1967) classification of egalitarian, ranked, and stratified societies. They are too general and start from contemporary (or historically) known elements which may not suit all prehistoric situations.

A better parallel to the conceptual pair of structurating practice and structurating positivities is to be found in the archaeology of Foucault. Foucault makes similar connections between his microanalysis of power and macro instances and emphasises the material manifestations and conditions for action in terms of buildings and the human body (Foucault 1979:136; Braidotti 1991:88). There are also similarities between his concepts of discourse and episteme and the microarchaeological terminology. In one sense, discourse in Foucaultian terms comprises a group of 'interrelated' statements or

the normal way of doing things. It is used on a broad basis from small to large scale, including collectives of sub-discourses such as the 'medical discourse' (Foucault 1972:163ff; Braidotti 1991:38). If we change the terms 'statements' and 'utterances' to executed actions, we find many similarities with structurating practices or 'fibres'. Another central concept, *discursive formations*, i.e. regularities/irregularities among discursive "objects, types of statement, concepts, or thematic choices" (Foucault 1972:38, 107) are not very far from the concept of structurating positivities or 'threads' (but perhaps closer to Lacan's notion of the symbolic order). In a similar sense, the metaphor of 'ropes' may roughly correspond to *epistemes*. The main differences lie, however, in the circumscribed character of Foucault's concepts, which tend to constitute totalities, closed systems of thought and practice. Foucault implicitly acknowledges the existence of parallel discourses and thus the possibility of standing outside discursive formations (e.g. 1980:219f), but his theory still leans more closely to structuralism than microarchaeology does.

The main problem in Foucault's archaeology concerns how discourses are formatted, maintained and changed; a question that Foucault stubbornly avoids. He does not wish to explain, or even speculate about, how discourses are constituted or changed (1981:139f; 1972). Quite the contrary, the microarchaeological framework allows several different ways in which social practices, and even the more inert, social positivities, can change rather than being reproduced. This is because of the fluidity of the concepts of fibres and threads; they are interwoven and of different lengths and effects. The ways in which social changes are possible is perhaps obvious, it will suffice to mention a few examples here. For instance, we may consider small unnoticeable changes in structurating practice, perhaps caused by misunderstandings or inability to do things the 'right' way, that may cumulatively lead to sudden change. In a similar way, changes in material circumstances can also potentially initiate changes in practice. Examples could be changes in ecology or demography. A third scenario departs from changes in ways of handling materialities (e.g., technical innovation, building techniques, house and settlement layout), intentionally or with tacit bodily knowledge. Social change may also occur because structurally conscious subjects that occupy powerful subject positions may want it. This is perhaps a less likely possibility than we normally want to assert, but, as Lenin once put it: "In some respects, a revolution is a miracle". Indeed, revolutions are rare, almost impossible, but sometimes miracles still occur (Zizek 2001:85). Furthermore, we may consider cases in which large-scale changes in nodal points/structurating positivities/threads generate different ways of thinking about certain practices and materialities. Finally, structu-

rating practices may change because of unintentional effects from other structurating practices. There are certainly more possibilities, but these are sufficient for the time being.

It is important to emphasise here that the main goal of the microarchaeological approach is *not* to identify threads and fibres in particular. On the contrary, it is rather the structurating practices that actually reproduce a certain thread. This is not to claim that the microarchaeological approach provides an impeccable tool for analysing social practice. For instance, we may find a 'gap' between structural practice and structural positivities. There is a great difference in complexity between, let us say, a way of making pots and practices involved in the disposal of the dead. But there cannot be a set of analytic tools that comprise all human agencies on the same level of detail. The archaeological toolbox has to be general to provide openness in dealing with any given case.

Summary of the general arguments

So far, the general discussion on the constitution of the social subject and social practice has centred on general issues, more or less valid for both prehistoric and contemporary social studies. I have outlined a basis for a microarchaeological approach as the study of *executed practice.* I have agued, via Giddens, Sartre and Foucault, that day-to-day, social practice is mainly of a routine character and such mundane practices have structurative relations to the material world. The outline of a farm or a house is thus both an indication of the way of doing things, while at the same time such contructions have a potential of initiate or steer action in new, unforeseen directions (cf. Bailey 1990; Bradley 2002). It may appear as a paradox that social life is repetitive and regulated, considering the flexible character of the human psyche and the diversity of personal experience. But via the psychoanalytic theory of Lacan, we find one answer to that paradox in the constitution of ideology as a *residue* of social constitution processes under certain material conditions. The imaginary dimension of the subject's experiences of the world (including the Real) and the problem of shifts in 'nodal points' imply that the individual experience is a doubtful point of departure for social analysis. The same multiplicity and problems of intersubjectivity make it hard to sustain unitary concepts of, e.g., social systems, cultures, societies or thought collectives. Social action is not confined within such virtual entities (although we may find 'bundles of fibres' within given space-time slices). To make progress in social studies we can change focus from what people might have experience of to what people do, that is, *executed action.* Social action is not as varied as the multivocal apprehensions about them. Here we find

that the dichotomist division of top-down and bottom-up views of social life or the individual-structure can be bypassed by acknowledging executed action as *mediating* these extremes. Social action may be conscious and intentional but also semi-unconscious routines. In most cases, actions have unintended and unforeseen implications. Social action is thus both constituted from an individual perspective but must also take into account constraints and possibilities of structural forces, as well as material conditions. Although the microarchaeological approach focuses on slow-changing and inertness elements of social practice, I am not, however, suggesting that ideologies or structurating positivities are found above the heads of the individuals who (re)create them. As pointed out, there are many different ways in which social change may occur, but it is 'normally' an inert process in the time-scale of generations.
In the remaining part of this chapter, I shall narrow the general approach and focus on the particular possibilities and constraints of archaeology. Many factors differentiate archaeology from other social sciences. For instance, anthropology and sociology generally make use of written records, behavioural studies and living informants, while archaeology primarily studies material traces of social practice. Archaeological interpretations are also complex, since, due to temporal depth, we must expect to confront social formations of unknown organisation and structure. Here we cannot rely on the ethnographical record as a reference, because it is more of a result of historical processes that are intimately linked with other, including western, contemporary, social formations (cf. Fahlander 2001:31-41; Fahlander ms). The following discussion suggests that archaeology needs creativity and imagination in theory and model-building to cope with the 'unintelligible' nature of past social formations.

Beyond nodal points: Analysing abstruse pasts

Archaeological analysis is in many ways unlike contemporary social studies. One difference is found in the lack of living informants and written texts, something which, it is often argued, constitutes a lack. However, bearing studies like Rathje's garbage archaeology in mind, we may as well reckon archaeology as being *liberated* from such information. It is often tempting to emphasise written or oral information, as it appears to us in a direct way, implying that understanding is possible. But, as Rathje's study points out, people are seldom capable of fully or correctly accounting for their doings. Studies of material traces of action may not be able to encompass all actions, but they still inform us of how some of them actually were executed. Giddens has argued that contemporary social studies imply a double hermeneutics, that is, that the analyst analyses a

responding object which hence is being affected by the analysis (Giddens 1984:284f, 374). Shanks and Tilley have argued that anthropological studies of the 'culturally other' involve extra levels of hermeneutic tasks. One difference is the influence and bias of the analyst's own 'pre-interpreted universe' in studying something different (Shanks & Tilley 1987b:108). For instance, that one usually puts questions which seem relevant to one's own reality but which may be irrelevant in social formations with a different symbolic order.

Shanks and Tilley also add a fourth level of hermeneutics in the specific study of the past, due to the distance in time. It is perhaps a bit silly to stress that remoteness of time and space require additional levels of hermeneutics, but there are nevertheless differences in interpreting a prehistoric social formation as opposed to any contemporary one.[11] In our images of the past, we often apply an evolutionary perspective of 'us' at a pre-industrialised stage – the study of 'us' as something other. The chief difference between contemporary studies and prehistoric analysis rather lies in the lack of relevant references. The past may not be completely unknown to us, but it may possess a social variability greater than we can imagine. After all, some bundles of social actions and social organisations once forming a social world are bound to have become extinct and no longer present (cf. Freeman 1968:266). The present social variability and similarities are very much the result of long-term, regional and even global processes (Fahlander 2001:37ff; Fahlander ms). Such a perspective has many theoretical and methodological implications. Which are the relevant references and sources of inspiration? Here we will find that the ethnographic record has never been sufficient to encompass all the possible variability of the conditions of action. In our attempts to analyse the material traces of social action, we need to be creative and expand our horizons to fill the gap (cf. Castoriadis 1995:107). For instance, we can draw inspiration from popular fiction or any other creative thinking in constructing social fictions on 'sociohistorical phenomena'. This is not to suggest unlimited speculation; *any* model of fiction must be related to observable, sociohistorical phenomena (i.e. material traces of action).

Us and them: emic vs. etic perspectives

The social sciences, and anthropology in particular, face the awkward question of *emic*, the perspective of the object, or *etic*, the concepts, abstractions, etc. of the researcher (see Gellner 1985:145; Harris 1990:48ff; Sestieri 1992:10f). These concepts are simply an abstract division to make it easier to discuss the general differences

between the different positions of the acting subjects and the objectivating analysts. Here we need to deal with the fact that 'we', the analysts, are as heterogeneous and changeable as 'they' whom we seek to analyse (cf. Moore 1994:132). Of course, the subject positions within emic or etic perspectives are no more homogeneous than the other, but these divisions can be helpful in a general discussion. The main critique of the post-processual movement was the etic perspective of processual archaeology. They were mainly concerned with *how* things were done in the past, not *why* (Parker-Pearson 1999). But is it possible to understand the whys of a people whom we 'know' only from a fragmented material record? In interpretative frameworks, the focus is naturally set on emic definitions of the social totality. The hermeneutic approach needs a common frame of reference to understand the sociohistorical meaning of artefacts and agencies. In a similar vein to Zizek, Cornelius Castoriadis in a series of essays has pointed to the problems of 'translating' other people's ontologies, past or present. His examples are often taken from the ancient Mediterranean world, a period which we believe that we know pretty well from written texts and archaeological studies. Yet Castoriadis argues that we are able to reconstruct the *intentional vector*, the energy or drive of a social formation, only by its activities and value hierarchy. We may also be able to study the vector of *representation* through the context of the observable 'sociohistorical phenomena'. But Castoriadis is not very hopeful on the possibility of understanding the 'affective vector'. He argues that 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* while entering the Eleusinian mystery cult (Castoriadis 1995:107). Similar arguments have been put forward by Paul Ricoeur (1976). He poses the question of what indeed can be understood and appropriated:

> Not the intention of the author, which is supposed to be hidden behind the text; not the historical situation common to the author and his original readers; not the expectations or feelings of these original readers; not even their understanding of themselves as historical and cultural phenomena. What has to be appropriated is the meaning of the text itself, conceived in a dynamic way as the direction of thought opened up by the text. In other words, what has to be appropriated is nothing other than the power of disclosing a world that constitutes the reference of the text. In this way we are as far as possible from the Romanticist ideal of coinciding with a foreign psyche. If we may be said to coincide with anything, it is not the inner life of another ego, but the disclosure of a possible way of looking at things, which is the genuine referential power of the text (Ricoeur 1976:92).

The critical hermeneutics of Ricoeur, along with the arguments of Wittgenstein and Castoriadis, suggest that the interpretative methodology and the search for meaning is, so to say, at best an ambitious enterprise. It is obvious that the problem of emic versus etic perspectives is not a question that can be solved theoretically, or methodologically, albeit that excluding the emic dimension as out of reach does not mean that we are left with a structural, top-down perspective; we are still able to analyse individual social action. In fact, the concepts of structurating practices and positivities would be very empty if we neglected the social agents that (re)produce them. A possible way of 'thickening' and 're-humanising' their abstract and analytical character may be to create first-person narratives loosely based on the information at hand. This is a difficult task, in which few archaeologists are trained. The attempts made in this tradition are still not very convincing. It seems that archaeological narratives either go 'over the edge of credibility' or get too hampered by archaeological evidence that probably was never very significant at the actual event (e.g. Spector 1993; Chapman 2000a:9-11).

Examples from other fields are, however, more promising, for instance, the account by Isabelle Ebernhard of a rural Muslim burial (1990:202-5) or Karin Salomonsson's (1998) ethnographic study of working-class women in Gothenburg during the 1950s. Salomonsson 'thickens' the dry and objective, police protocols by adding small paragraphs of narratives written from an emic, first-person viewpoint while Ebernhard simply provides a poetic, yet non-sentimental, observation of an event. Such creative extensions of fragmented information are not necessarily the same as writing novels. Writing archaeological narratives is thus not simply about speculation or fantasizing about the past (Tilly 1981:234f; Tilley 1993:13ff). In either case, we are nonetheless still faced with the problem of finding and analysing traces of action from fragmented 'observable sociohistorical phenomena'. I will spare the reader any personal attempts in this novel practice, but I am confident that a microarchaeological point of view and the concepts of structurating practice and structurated positivities constitute a promising basis for such elaborate narratives.

Models, fictions and analytical fields

Models are essential and unavoidable for any study of the past but are, despite that, seldom explicitly discussed. Yehuda Elkana (1981:20) has stressed that the scientific method is often a second-hand construction. Our analysis often begins with a problem, which imputes the construction of a theory that explains or accounts for

the given pattern. It is also a fluctuating process. This notion is certainly valid in many areas of scientific work, but we need to consider that models, especially social models, very much dictate the process of theory-building. Models always run the risk of being transformed into the actual object of analysis. The material evidence is squashed and manipulated to fit the types of the model. Social models encompass a great variety of general, implicit or explicit notions of human action, from the mythological savage to the 'economic man' or social types like hunter-gatherers and settled agriculturalists. Also concepts like male-female oppositions are commonly assumed as if they were all-time, valid types of social categories. A typical and illustrative model is the band-chiefdom-state schema of social anthropology. The model suggests that each stage of social evolution can be described by typical attributes like kinship system, presence of aristocracy or egalitarian organisation. This particular model has been subjected to much criticism for simplifying the variability of social organisation, as well as for the way in which it was compiled (e.g., Leach 1989; Friedman 1994:6), but the types are nevertheless still employed (e.g., Earle 1987; 1991). It is evident that any preconceptions of the general organisation of, let us say, a chiefdom steer and restrict the analysis of the actual sociohistorical phenomena. This is not something restricted to processual or evolutionary approaches; we find a similar use of models in post-processual and interpretative archaeologies. For instance, Hodder seems to have a clear image of the typical behaviour of 'small-scale, lineage-based societies' (1984:61, 64; cf. Reybrouck 2000) and Tilley devotes a whole chapter to describing the more or less irrelevant 'typical' notions of space and landscape of small-scale societies (1994, Chapter 2).

Models are thus necessary for social analysis and are often implicit in our choice of *analytical fields*. We always face the question of which aspect(s) we want or can analyse of the material traces of action. In a general sense, we find two main, sometimes competing, dimensions: the social and the cultural. The division between the two dimensions of social practice can be traced back to the tradition of American ethnologists and anthropologists, who focused on cultural properties, while Anglo-Saxon anthropologists were generally concerned with the social aspects (Billington 1991:4; Jenks 1993:29). The actual contents and meaning of the two fields vary between and within the sociological, anthropological, ethnographical and archaeological disciplines. The concept of culture is widely appropriated and is almost beyond any definition. It normally connotes 'that complex whole' (Tylor 1871) or just the normal way of behaviour of a people (e.g., Goodenough 1961). In some strands of thought, culture has special aesthetic values, for instance, separating

the fine arts from popular culture (Adorno 1970; Bourdieu 1977). The social dimension emphasises the organisation and hierarchical relations of a people. *Social organisation* denotes the internal hierarchies of a people or group, while *social structure* is a more abstract, superorganic and general notion. Gellner provides an illustrative example of how the different concepts of culture and social structure are generally understood:

> The idea was that a tribal society has a certain structure or organisation, each part of which imposed such pressures and sanctions on the individuals within it as to ensure that they behaved in a way that sustained that structure, and so on forever, or at any rate for quite a long time. Structure was important, a matter of serious concern for men (inside the society or among investigators). Culture on the other hand, was relatively ephemeral, accidental, epiphenomal and altogether suitable for women (inside the society or among investigators). Structure was, for instance, whom one could marry; culture was what the bride wore (Gellner 1985:135f).

This quotation from Gellner illuminates the implicit preconceptions of a social or a cultural perspective. The social structure of a society seems somewhat synonymous with the activities of the male members, while cultural properties were something embracing the 'less important' activities of women (cf. Billington 1991:120; Dick-Bissonnette 1998). However, despite the voluminous debate in the first half of the century, the differences between the cultural and social anthropologies were in fact quite small. They used approximately the same methods (field studies) and theory (functionalism, e.g., Lévi-Strauss 1963:355). The distinction between the cultural and the social is, however, still found in archaeology (e.g., Morris 2000 Ch. 1). There is clearly a problem in working within frames (culture/society) whose content no one can really account for (Hobsbawn 1979:250) and which simultaneously constitute concept, cause, form and outcome (Geertz 2000:13). The traditional division between cultural life and social organisation thus seeks to separate inseparable phenomena (Kroeber 1952:7). A holistic approach is, however, not necessarily very much of an alternative. Clifford Geertz stressed that anthropology is better off embracing the whole dimension of human practice, and employing 'thick' description, that treats social and cultural processes on equal terms (1973:3-30, 143). Geertz argues convincingly, especially in the case of the funeral of a Javanese boy, that simple notions of tradition, status-role relationships or social structure cannot account for, nor explain, behaviour in all social situations (1973:146-62). To acknowledge that human phenomena have a complex matrix of social, cultural, relig-

ious, political and economic dimensions is, however, not an explanation of the unfortunate distinction. Holistic or total studies like 'thick' descriptions that seek to encompass all elements of the social process often tend to be toot-less or self-explaining (cf. Geertz 2000:13).

We do need to make some restrictions and focus on a limited array of possibly important elements in a social analysis. Such *analytical fields* do not need to, and perhaps should not, follow traditional social taxonomies, like religion/cult, politics/social organisation, economy, technology and ideology/cosmology. These are more or less abstract taxonomies of a social totality; they have no given content or clear-cut borders that separate them from each other. They are based on contemporary or historically known conditions of action and disciplinary traditions of thought which may not be relevant to archaeological studies. The analytic dimensions of the social whole are thus not fixed but are, like most social aspects, parts of structuration processes. Similar arguments can be posed to avoid routine uses of social categories/factors, such as sex, sexuality, class, status, ethnicity and age (e.g., Butler 1993:168). All of these are complex, difficult to define and often heterogeneous and, most important of all, they may not be significant for all social situations. To restrict an analysis to a pre-given, singular, signifying category may thus be a-chronological or simply less useful. For instance, Haraway points to the fact that some of our "objects of knowledge have their own trajectories as historical actors; 'race' did not always exist, 'class' has a historical genesis, and 'homosexuals' are quite junior" (Haraway 1991). Social series are thus products of given sociohistorical contexts; it is sufficient to mention Foucault's work on the exclusion of the mad as a social category (1989).

From a microarchaeological point of view, the distinctions between social taxonomic dimensions are superfluous. Social action in terms of structural practices and structural positivities embrace the whole social context, because they are not based on emic or etic perspectives but on executed practice. To find valid objects of study (analytical fields) we need an element of creativity and imagination to avoid employing ahistorical, traditional, social taxonomies that may not fit the given sociohistorical condition of action. For archaeology, in contrast to much sociology and anthropology, we need to find ways of identifying the 'axes of difference' that were active during a given time and space. What kind of subject positions can we find, which corporeal aspects are socially significant as well as the significant axes of difference? It may thus be appropriate to speak about *fictions* rather than models as a way of acknowledging their virtual character. It goes without saying that archaeological fictions will probably turn out to be most favourable if they are

based on the given sociohistorical conditions, that is, the material traces of action. As Leach once argued about the ethnographic analysis: "Our concern is with what the significant social categories are; not with what they ought to be" (Leach 1961:27). However, our inspiration may come from other sources, such as contemporary social science or even popular fiction. In fact, such sources of inspiration may even prove to be better, because they are not so loaded with preconceptions of being anything more than fictions (which is often the case with anthropological models). For instance, Tilley (in the previously mentioned example) could just as well employed analyses and concepts of contemporary studies of industrial societies (e.g. Lynch 1960; Hägerstrand 1970; Soja 1996), instead of the anthropological references in his text on landscape phenomenology (1994).

Sartre's concept of seriality seems to offer a productive way of dealing with unknown, social configurations. By studying traces of serial practices and social subjection/subjectivation, polythetic, serial categories can be established which will, it is hoped, better correspond to the structurating practices that were executed in a given time and space.

Material evidence of structurating practice

The question of how can we reconstruct structurating practice from fragmented material traces will not be given a definite answer here. There are no general approaches to employ, but every given case needs to concern the specific material circumstances. I have already touched upon the potential importance of the layout of materialities such as monuments, houses, graves, etc. in the process of (re)production of structured practices. Similar socialness can be attributed to humanly modified materialities in general (cf. Bradley 2002:12). Clear and definite distinctions between a 'natural' and a 'cultural' environment, or between 'natural' objects and cultural modifications (i.e. artefacts) are not very easy to sustain. Fortunately, neither distinction is necessary. What is a projectile for some may be a plain stone to others. A megalithic tomb was a human creation to its constructors, while it was perhaps regarded as natural or supranatural by later generations/outsiders etc. I do not believe that it is essential to penetrate these issues to any great extent, but the general role of material objects in social formations demands a short discussion. I will mainly focus on the social implication of objects, although similar arguments can be attributed to the whole frame of activity of the landscape/environment. The materiality of the environment is more about the structuration of space and will be discussed separately in Chapter 4.

Archaeological approaches to materialities

The archaeologist's natural interest in materialities, especially humanly modified objects, has led to a number of approaches seeking to extract social information out of the material record. There is no common body of theory; the analysis ranges from symbolic relations to hard-data analysis involving quite different methodologies, which demands a short discussion. To begin with, we find *cognitive approaches* that emphasise the importance of materialities as influencing thought, ontology and social practice. For instance, Tilley & Thomas (1993) suggest that the body, and bodily parts (e.g. the torso), functioned as essential metaphors in Neolithic symbolism and hence influenced the design of artefacts and constructions. Peter Wilson suggests that the concept of the house and the establishment of permanent settlements were major formative aspects of the Neolithic, far more important than, e.g., the practice of agriculture. Wilson argues that the experiences of an enclosing concept such as the house (and tomb) initiated new social strategies (Wilson 1988). In a similar sense, Ingold (1992) has supported the view that social and cosmological systems are often hindsight constructions based upon the constraints and abilities of the natural environment. The cognitive and phenomenological aspects of materialities are perhaps most frequent in landscape archaeology (e.g. Bradley 1993; 1998; 2000; Tilley 1994; 1999). The potential of materialities as nodes for mythologies and memory has also been highlighted. Objects and natural features may serve as individual or collective memory in, particularly, non-literary, social formations (e.g., Tilley 1994:202ff; 1999:182; Bradley 2002).

Popular themes are also *symbolic* and *communicative* approaches to material evidence. Especially the concept of style has been of major concern (e.g. Sackett 1977; 1982; Conkey & Hastorf 1990; Carr & Neitzel 1995; Hurt & Rakita 2001; Cornell & Fahlander 2002a). Differences in outline and decor are argued to signify cultural groups as well as individual status or social identity, for instance, Polly Weissner's (1983; 1985; 1989) awkward division between 'emblemic' and 'assertive' style. The first category concerns standardised traits or symbols of ethnicity, while the latter represents more obscure individual aspects of identity (cf. Olsen 1997:186f). Another example is Wobst's (1977) claim that material culture, besides symbolism, also carries social information, a kind of indirect communication. In these perspectives, elaborated material culture is semi-active. It is involved in social communication, indicating (or masking) status, ethnicity and identity, as well as manifesting institutions or ideologies. A high level of symbolism and cultural homogeneity is often taken for granted – a fiction more or less based upon modernist ethnography. Examples of *metaphorical*

reasoning are found in the examples of Tilley and Thomas, but also in, for instance, Hodder's (1984:53; 1990:149-55) suggestion that elements of the houses of the living might be symbolically represented in the properties of burials (the 'house of the dead'). Recently, the concepts of metaphor, metonymy/synecdoche and irony have been advocated as some sort of alternative to simple symbolism (Yates 1990:169; Tilley 1999; Tarlow 1999:37-49). It is perhaps more fruitful to use metaphors, but it is certainly not an alternative. Metaphors and metonymies are often too general and mundane and it may be hard to find anything more substantial in them besides the simple fact that humans tend to think through one or another.

Besides the cognitive and symbolic approaches, we also find *hard-data analyses,* ranging from the social aspects of technology and function to traditional artefact-fetishism. There are many interesting, 'hard-data' analyses focusing on technology and function (cf. Cornell & Fahlander 2001a:67-74). One example is Grimm's (2000) discussion of the production of Palaeolithic lithic material. Grimm employs Leroi-Gouran's concept of the *chaine opératoire* in her discussion on novice vs. expert knappers, departing from refitting and the micro-scale distribution of flint refuse. Other aspects of hard-data approaches are to be found in experimental archaeology, like micro-wear analysis (Semenov 1964; Kristiansen 2002).

These general approaches, *cognitive, symbolic/communicative and functional/materialist,* are, despite numerous variations and exceptions, sufficient to cover most archaeological analyses of materialities. The general perspectives on materialities employ various aspects of the material evidence; some may be combined, while some are more or less incommensurable. Most of them suggest that material objects are important actants in the formation, interaction and structuration of social action. The question is in what ways materialities can be said to be socially active in these processes and how we can extract social information from the material record.

On the social character of materialities

Despite the emergent interest in them in the last few decades, material objects are still rather neglected in the social sciences. The predominating view is that social science is about human agency, in which materialities play only a passive part. For instance, Durkheim writes: "Things do affect quite much of the social development, whose speed and, as a matter of fact, direction varies according to their properties, but they lack prerequisites to actually make things happen. They are the physical materials that the vital forces of society utilise, but they do not release that power by themselves. The only remaining active factor is hence found in the human milieu"

(Durkheim 1895:93, 12). Durkheim's account of this matter is clear and precise. A similar conception is to be found in the work of Talcott Parsons, to whom cultural objects are parts of the individual's personality, but only 'indirectly relevant' (Parsons 1951:89, 4). Processual archaeology generally adopted this view, regarding material culture as a reflection of the *functioning* of culture (Thomas 1996:10). Material culture is merely viewed as a result of adaptation to the 'natural' environment. In post-processual archaeology, on the other hand, there seems to be a consensus that material culture is *active*, but not very much discussion of what it really means. Shanks and Tilley argue that material culture is socially active *per se.* "Material culture should be regarded as not merely a reflection of cognitive systems and social practices but actively involved in the formation and structuring of those practices" (1987a:85, 211; cf. Tilley 1996b:4). This is clearly formulated as a critique of the processual/functional view of material culture. The liberal element of post-processual theory prefers to talk about negotiation in general, while the more structuralist-oriented fraction suggests that material culture is used by élites and empowered groups to strengthen or sustain the social order. Materialities are manipulated to mask social inequalities, as well as simply to manifest the social order (Hodder 1982:11, 229; Shanks & Tilley 1987a:86). The difference from processual archaeology is summarised by van Reybrouck as follows:

> The principle of the active role of material culture was not so much about *inverted* causality as it was about the *absence* of clear-cut causality. It [post-processualism] did not state that material culture steered or dictated social action, but that it could be used as an instrument of social negotiation. It was not about material culture as the causing agent, but about the role bestowed upon it by human actors (van Reybrouck 2000:42).

Post-processual archaeology seems thus more concerned with a discourse *about* things than a discourse of people and things. The active role of materialities in the social process is not so much about their initiating and modifying agency, but rather about their presumed symbolic function. For instance, Hodder argues that pottery style does not *create* social differences but is rather *made* to occupy an active role within society. In his view, material culture does not by itself include any ability to 'act back' on society; it only works within 'frameworks of meaning within the society' (Hodder 1991:8). The general argument of post-processual or interpretative archaeology is thus that material culture is active because it is 'meaningfully constituted' (e.g., Hodder 1982:75; 1987:6ff; 1992:15). Hodder writes about general symbolic schemes that ensure a frame of reference to

which that meaning is related (Hodder 1992:14, cf. Shanks & Tilley 1987b:103ff; Tilley 1999:9). Meaning in this sense seems thus to refer to a common horizon of a group of people than the plans and intentions of the individual actors (Hodder 1991:150ff). I have previously discussed the problems of *understanding*, suggesting that meaning is generally heterogeneous and plural. If meaning is plural and multivocal, in what sense can we 'understand' the meaning of objects? What is at stake here is that materialities can be social in other ways than as symbols loaded with meaning. Materialities have, as pointed out by Sartre and Foucault, a *potential* of being active in the sense of stimulating, prompt or determining social action. Such a perspective is found in the work of Bruno Latour (e.g. 1991; 1992; 1998; cf. Cornell & Fahlander 2002a:53-67).

Latour criticises the social sciences for neglecting the objects (actants) in social-interaction studies. "…I argue that in order to understand domination [power relations] we have to turn away from an exclusive concern with social relations and weave them into a fabric that includes non-human actants, actants that offer the possibility of holding society together as a durable whole" (Latour 1991:103). Latour rejects the distinctions between material infrastructure and social superstructure, as well as sociologism and technologism. On the contrary, he stresses that social relations are made up from chain links between humans and non-human actants (vehicles for action). These chains form networks which operate in various ways; actants may function as a prolonged arm of a human being, replacing the human subject, or to facilitate, or constrain, certain tasks. The main point in the reasoning of Latour and others about 'the socialness' of things is that almost any material object can *potentially* take a more or less active role in a social event. As the archaeological evidence is mainly material, it suggests that we need to consider this potential in order to discuss and analyse social action above the mundane and simple. Here, we may find Sartre's serial perspective operative as a way of discussing the links between social practice and materialities. In the serial perspective, materialities are always involved in social practice. For the people in the queue at the bus stop, the sign and the bus have no particular meaning (although there may be such cases). The main point lies rather in the material constitution of buses and the ways they are used.

Space, time and context

Hodder's answer to the problem of situated meaning is that the interpretative method must be open for alternative readings (Hodder 1999:78; cf. Tilley 1993:23; Thomas 2001:180). It is, however, hard to see how multiple readings in practice could handle the problem of

the plurality and fluidity of meaning. The material culture of such a multivocal past is not arbitrarily constituted, with an indefinite number of plausible interpretations, but is limited by the context and internal relations of archaeological evidence (Hodder 1992:14). Hodder argues that the "structural 'wholes' need to be tempered with a fuller understanding of socially embedded, conflicting meanings" (1992:13). The problem for Hodder is thus not to capture a structural whole of a past social formation, but how to fuse the horizons of the past and the present. It is, however, doubtful to argue on one hand, for plural meaning, and on the other, for a common 'now' and 'then'. Interestingly, Hodder stresses that Gadamer's notion of horizon is 'equivalent to my discussion of context' (1991:151), defining context as "the totality of the relevant dimensions of variation around any one object that can be identified as the context of that object" (Hodder 1991:143). This context is, however, not equivalent to social wholes or units:

> … the boundaries around a group of similarities (such as a cultural unit) do not form the boundaries of the context, since the differences between cultural units may be relevant for an understanding of the meaning of objects within each cultural unit. /.../ 'Cultures' therefore, are components or aspects of contexts, but they do not define them (Hodder 1991:143).

Contexts are seemingly free-floating abstractions within (and cross-cutting?) 'a culture' or 'a region'. The meaning of an object may thus be interpreted within series of such contexts "through their relationships and contrasts with other items within the same text" (1991:144, 1992:14). How these demarcations are supposed to be made is not to be found in the work of Hodder. To argue that the social load bestowed on materialities is varied and polysemious is not the same as to propose relativism or the death of scientific archaeological analysis. Some aspects are more solid than others and the possible number of analyses is not endless, neither are they equally valid. The material we excavate and analyse possesses a point of reference in time and space which makes some analyses more plausible and others more unlikely (Pearce 1994:130; cf. Shanks o Tilley 1987b:245f; Hodder 1992:14; LaMotta & Schiffer 2001:44). To make use of these relations, we need to start from the *archaeological contexts,* not from general notions of symbolic schemes, ontological horizons, symbolic orders or emic concepts of culture or society. Here we find the core argument for microarchaeology. It is inherent in the metaphor of fibres and threads that we do not need to know all the fibres in a thread or all the threads in a rope to recognise their presence. There are often many different ways of analysing a 'thread', as it is more or less inherent in all social practice; there are traces that can be

combined or related to make a plausible argument from very small and fragmented material. The microarchaeological object is not simply the event; the spatial and temporal extent can be quite great. It is neither based on assumptions of emic social units or etic regional entities, like a parish, nor on natural borders of the environment, such as high plateaus, islands or river valleys. There is no fixed and perfect, geographical frame for a given analysis: we can always make it a bit larger and incorporate new traits, which may continuously alter the picture. The vision of a full coverage can never be realised; some aspects of social practice are perishable or not identifiable by standard, archaeological methods. There is no social totality or context to be distinguished, either in the past or in the present. The microarchaeological object of study is therefore delimited by the amount and quality of the archaeological evidence. The analysis is generally detailed, focusing on the contradictions in data, the anomalies, and the things that do not fit, as well as patterns and regularities. We do not need to think in terms of cultures, societies and ethnic groups, nor of regions or biotopes, etc. Social practice surpasses such entities by its general relations to individual aspects (cf. Cornell & Fahlander 2002a, 2002b).

Framing the object: Social formations and microecologies

Despite the many objections, there is nonetheless a need for some kind of analytical frame that will circumscribe the spatial and temporal limits *for a given analysis*. The most common way in the social sciences is a combination of social and spatial concepts, defined from either an emic or an etic point of view. Archaeologists have traditionally spoken of cultures or 'cultural areas' defined by the spatial distribution of artefact assemblages. However, as pointed out previously by Geertz, this tradition has the fallacy of reasoning in circles, because the primary source of analysis also defines the frame of interpretation. Other circumscribing concepts, like society, social system or polity, may seem to be better choices but are still subject to similar critique.[12] The main problem with either of these terms is that they give an illusion of distinct, homogeneous entities. Yet it would still be unfortunate to entirely abandon all sorts of 'social frames'. After all, there is something 'out there', imaginary or not, which stands out in bas-relief and which has unquestionable social effects (Giddens 1984; Barth 2002; Wright 2002:168).

We need a term to use in general discussions of social action. I have somewhat reluctantly employed the term *social formation* to connote such blurred and fuzzy 'bundles of fibres'. A social formation is never the object of study, it is simply a term for something that is hard to define, it accounts for nothing (Cornell & Fahlander

2002a:12f). The term social formation is thus only applicable on an abstract level, which means that we still need to find an operative frame suitable for archaeological analysis. Archaeology has for different reasons departed from firm and concrete, spatial frames. Modern, political, territorial concepts like Europe and national states are still frequent on the larger scale and different concepts of parishes are still deployed, despite their lack of relation to prehistoric social conditions. Others seem content to employ 'naturally' bordered areas such as biotopes, valleys, islands etc. (e.g., Renfrew & Wagstaff 1982). We find much criticism on this matter in processual archaeology and human ecology, which point to the varied resources utilised by many social groups of the ethnographical record (Lesser 1961:41f; Plog 1980; Butzer 1982). This variability is not restricted only to hunter-gatherers, but also includes farming groups.

Despite this criticism of territorially bounded groups and evidence of far-off contacts, different kinds of regions are still equivalent to social borders in much archaeology. For instance, Richard Bradley (1995), among others, has advocated a regional approach in an attempt to cover a 'wholeness'. In a discussion of burial analysis, Bradley argues that the analysis of one single cemetery can never sufficiently account for the complete social variability of a social formation. For instance, people of different statuses may have been buried at different places, and working on a single locality will cause us to miss this important aspect. However, there are some setbacks in this argument. A key problem with Bradley's arguments is that he still relates his analysis to imagined total entities of a culture, a region or a society. In Bradley's case, he seems to assume that the way in which a particular social formation deals with its dead could tell us something about the social structure in general terms. If a whole class or social category were missing in the analysis, since they were treated differently or buried elsewhere, it would thus (to speak logically, following the presupposition of social structure) certainly have a great impact on the interpretation. But if the social structure is not a homogenous or monolithic thing but rather constituted by a series of elements, of which only some are directly interrelated, the problem takes another form.

The intersocietal character of social institutions and structurating principles makes it less feasible to delimit the analytical frames by social parameters. Instead, the focus should perhaps be on the 'context of actions and results of actions' rather than on an emic or an etic unit (Barth 1992:31; Barret 2001:16). This is especially important in the study of prehistoric, possibly extinct, social practices. A focus on social practice in terms of structural practices and structural positivities, however, implies that we will more or less float freely in time and space. Social action may be performed at a very distinct

time and place, but its causes and consequences are not. As previously pointed out, humans are normally involved in many activities transcending a particular locale. Yet we need some sort of analytical frame to limit the scope of our analysis. Considering the problems of finding relevant social frames in prehistory, we may seek a frame departing from spatial limits. Indeed, our primary data, the archaeological evidence, is mainly spatially constituted.

An interesting concept is employed by Horden and Purcell in their attempt to comprise the large and heterogeneous, Mediterranean area (2000). Instead of trying to grasp the wholeness of the area only concerning the general structures over a large span of time, they advocate a *microecological* approach focusing on the 'micro foundations' of the Mediterranean (2000:54). As in microarchaeology they attempt to discuss the particular as a means of reaching the general: "...the kaleidoscopic mosaic of the Mediterranean is distinguished by the 'structures' which overcome the fragmentation" (2000:79). Microecology is the study of the relationships between living organisms and their animate and inanimate environment in what they term 'definite places'. These 'places', are smaller soft-edged, not crudely defined locales of varying sizes. Unlike the concept of *place* in phenomenological geography, the 'definite places' of microecology are not defined by the experiences or apprehensions of their inhabitants. The political, social or economic context is determined through the 'unbounding' of the systems on which it is to be brought to bear, so that the 'definiteness of places' is always qualified by their 'interdependence'. In other words, this simply means to invoke as much context as the locality seems to require (2000:54). The contexts of such definite places encompass a number of foci and margins but are subject to change in response to both 'internal' and to the pressures of the larger setting.

> Our definition of the microecology, therefore, is a locality (a 'definite place') with a distinctive identity derived from the set of available productive opportunities and the particular interplay of human responses to them found in a given period. It is not the solid geology or the characteristics of the climatic zone, the relief or the drainage, that of themselves define microecologies. It is rather the interaction of opportunities: for animal husbandry, foraging, hunting, intensive agriculture, forest management, horticulture, fishing, or whatever - and, as the final but by no means the least ingredient, for engagement in larger networks of redistribution (Holden & Purcell 2000:80).

Holden and Purcell exemplify their approach by discussing four different 'definite places' of the Mediterranean, illustrating both the kaleidoscopic character of the area and the common aspects.

Microarchaeology departs in a similar vein from the present archaeological information given at certain localities. In a microarchaeological perspective, we identify repeated patterning on a given locale; the spatial distribution over wider areas is certainly of greatest interest, but it is better that a quest for such patterning should avoid a priori defined locales or regions. Thus, starting from a series of such 'fleeting frames' patterns may be searched for elsewhere, and particular fibres, threads and ropes and their interrelations analysed. The relevant spatial and temporal boundaries of a microarchaeological study are based on the quantity and quality of the embedded information of a locale. The importance of such aspects will, of course, vary according to the aims of the analysis, an issue that I shall return to in the case studies.

The long, the short and the situational

The final point is strictly archaeological and related to the constitution of the material evidence of action. We need to find ways of making the sequence of events clearer to us. To be able to discuss structurating practices from a fragmented material record, we like to deconstruct the object and identify chains of events. The sequence and duration of different, more or less related events is, of course, of central importance here. It is not very important to have a fine-grained, absolute chronology (although it often helps), but we need to master the relative sequence of actions or events. The interpretation of data from areas of long-term use and discontinuous activities is always difficult and concerns a number of relations. The distinctions between *longue durée, conjuncture* and the *event,* as advocated by Fernand Braudel (1979), have been proposed by some archaeologists as general distinctions between different cycles in time (e.g., Bradley 1991:210; 2002:6; Bintliff 1991a; Gosden 1994:122). The stress on the long term was, to Braudel or Foucault a reaction to a 'thin' history that was based of series of events, battles, inventions, etc.

This critique is generally valid also for archaeology, but the arguments for the long-term perspective are put from another point of view. It is argued that the fragmented and disparate nature of the archaeological record implies that we need to compare and combine individual samples to establish enough data for a valid analysis (Thomas 1996:11). Some (I would say, pessimistic archaeologists) hence suggest that the long-term perspective is what suits archaeology best. In the long-term, the archaeological evidence is accumulated which 'prevents' freak events and anomalies being 'misinterpreted' as prevalent properties in long-term processes, such as technology development or changing settlement patterns. The question of the level of generality is a central problem in most ar-

chaeological situations, especially evident in Palaeolithic studies, where the scarce data is scattered over long distances and over long periods of time (cf. Cornell & Fahlander 2002a:82f). It is often interesting to take into account the constitution of long-term changes of the natural environment. The natural resources of an area may have great importance for the types of cultural activities performed there. The needs and interests of different kinds of resources, of course, vary according to the level of technology, manpower, social complexity or fashions. For instance, some resources may not be of any use under certain circumstances (Butzer 1982). In general, however, the *longue durée* or the long permanence of long-term processes go beyond social practice. For instance, the post-glacial elevation of the land is a poor factor in explaining individual social practice in the short term. Microregional and detailed analyses of smaller locales often prove to be interesting. There are a number of areas where a local, fine-grained sequence of events helps us to get a more detailed image of a practice. Terje Gansum (2002) has explored the potential in excavating burials in a single-context like manner. By identifying different stages or a sequence of different moments in the construction of a grave mound, he is able to discuss ritual processes of burial in greater detail (cf. Cornell & Fahlander 2002b:33f). Other examples of similar 'event-archaeologies' or 'single-context philosophies' are to be found in Leori-Gourhan's *chaine opératoire* and the Harris-matrix method (Harris 1979:87f). It can thus never be a simple question of favouring the long term before the short term. The nature and quality of a given dataset need to be concerned, as well as the questions directed to it.

It is important to recognize that the significance of time and the sequence of events differ from those in anthropological and sociological studies. The obvious difference is the nature of the data; material traces of actions versus actual observations of action and information from living informants. It is not surprising that materialities are seldom regarded as important in microsociological studies. The social *situation,* as analysed by Garfinkel or Goffman, is mainly a social encounter, a short-term series of negotiations or power exercises, whereas materialities simply form a general but quite non-essential frame, a stage on which social situations are performed. There are, of course, a number of such encounters, in which materialities play a significant role; a soccer game without a football would be meaningless and a weapon in the hand of one party can radically alter the outcome of a dispute. A social situation of this kind sometimes leaves material traces for archaeologists to analyse, but most social encounters probably never leave any persistent and direct traces. For instance, what evidence can we expect to find of Sartre's series of the bus queue? The outcome of social situations

and temporary series may, unintended or intended, conscious or unconscious, however, have effects on other practices and structurate social relations in the longer term. A future archaeological analysis of a present city may find traces of roads, bus-stop signs and even remains or images of buses. From their relations to other spatially situated data, it may be possible to partly reconstruct the presence of such series. In most archaeological cases, however, it is probably more fruitful to analyse traces of *events* rather than situations. Events normally involve performed practices and are thus more likely to produce material traces. What we can infer from such evidence are performed practices. A situation is difficult to define and frame; it is never a closed, isolated chain of events/practices, it is rather porous in character, as discursive elements tend to pour in and out of it, connecting the particular with the general. No sharp distinctions between the situation and the event can be established; the limits are floating. A burial can be seen as a situation, but also as a series of events depending on its duration in time and its later manipulations.

From the material traces of practices, we can abduct events or chains of events, some of which may hold as structurating practices from which we may reconstruct social situations and with the aid of additional data also reconstruct some structurating positivities. It is, on the one hand, a matter of levels of analysis, but only in an abstract sense, as the singular practice often carries general elements in the first place. The microarchaeological concepts are thus not equal to Braudel's threefold distinctions or to simple divisions into long and short term. Threads' or structurating positivities are generally more persistent, long-term phenomena and structurated practices or bundles of fibres are of shorter duration, but, as previously argued, they mutually constitute each other. The process of microanalysis seeks to identify distinctions between events and series of events, if possible. Social action is thus normally restricted to a certain time and place, although its causes and consequences may far transcend that place. We need to be concerned with how different material traces are produced. A single action may have as much impact as hundreds of years of repetitive actions, while others leave no persisting trace. In general, however, it is to be expected that repetitive actions will be more likely to be reconstructed from a deep temporal perspective. For instance, consider the simple example of one individual walking through the woods, leaving little durable trace, while, the action is if repeated, a trail may emerge.

An important factor in determining chains of events concerns previous activities that may be of importance for succeeding events, that is, if they are visible or remembered (e.g., Bradley 2002). Some previous activities may actually be initiated later, for instance, as in

the case of the situating burials close to ancient structures (see Chapter 4). The remains of a structure may also be used in later constructions just by being there (cf. Fahlander 2001:62) or, on the contrary, constrain or prevent the later use of an area. For instance, a burial ground may not be regarded as a suitable place for a domestic house. However, such former 'improper' activities are just as likely to be ignored. In short, there may be links between the activities of different times, but many activities at the same place are as likely as not to be non-related. Here we must keep in mind that our information differs from the knowledge of past agents. Our picture may be more fragmented and incomplete but may constitute a deeper, temporal perspective. What we see in retrospect with the aid of sophisticated instruments is not necessarily important or knowledgeable to all or some agents of the past.

The short term thus involves single acts and series of events and their material results are far from evident. The short term can be divided further into episodes, situations, chains of operations, serial collectives and the singular action. The episodic, only once occurring event may actually produce large quantities of persisting material evidence. Just as an example, a small site with large amounts of flint debris may actually have been the result of a hard day's work by an ambitious flint-knapper. A contrary example is larger constructions, like the ancient temple of Athena Alea in Tegea (see Chapter 4), which have a long history of activity, including rebuilding and modification of the structure itself (Voyatzis 1990:20-6, Østby *et al* 1994). Of course, individual artefacts have biographies; they may be re-used in various ways far beyond the intentions of their original maker/user (e.g., MacKenzie 1991; Jones 2002:83ff; Bradley 2002:15, 53). The biographical aspects are complicated and we have no general methods at hand to solve that particular problem. The history of each individual artefact and object must hence be treated according to its given context and its temporal and spatial relations to other objects.

We will never be able to account for the real extent of all activities, great or small, executed in a given microecology. Archaeological data can only open narrow apertures into the past. The vision is biased in several ways and cannot be corrected by, for instance, statistical calculations of probability etc. Nonetheless, human activities do produce persistent traces, of which a small part is available for the survey archaeologist. Here we may try to establish links between certain activities and their material results. The failure of Binford's (1968; 1978) Middle-range Theory to present any substantial results beyond simple anthropological analogies may prevent any further attempts, but, if followed less slavishly, the main idea behind MRT can be operative on an analytical level (cf.

Cornell & Fahlander 2002a:5ff). Some traces of past activities are more evident, like post-holes or burial mounds; others are implicit, like types of pottery or visual relationships between constructions and other materialities. The critical point of the chain of association lies in the models or hypotheses that we choose to investigate. It is important to be attentive to specific, local details and less bound within the frame of regional or universal models, in order to find the non-predictable, the unforeseen and perhaps the subaltern. This dimension can be found in the explicit or implicit links between different kinds of material evidence. Many places often have multiple uses and are experienced (that is, negotiated with) in various ways. In a similar way, material culture is often re-used or employed contrary to the initial intention or 'normal' use. Reconstructing action from such diverse and fragmented evidence can thus never be a straight-forward process but will rather emerge out of chains of hypotheses based on observable, sociohistorical data. The need of empirical data suggests that these aspects are better pursued further in the following case studies of Chapter 3 and 4.

Fig. 6. Agia Fotini, Arcadia, Greece. One of the most unusual, modern churches in Greece: it is a mixture of Classical and Byzantine elements, influenced by Japanese pagodas, and was built in 1972 by an engineer from Tripoli.

Summary of the particular discussion:

In the first part of the chapter, entitled *general*, I discussed a number of key concepts of social theory that may stand trial as operative in archaeological analysis. The first part is general in the respect that it concerns social interaction with materialities employable in any type of study, present or historical. The latter, *particular* part has mainly been concerned with the special circumstances of archaeological social analysis. One difference is the lack of direct observations of social practice, a condition that is also argued to be an advantage, as we are to a lesser degree tempted to lay emphasis on individual experience of action. The focus on practice rather than effects may seem anti-humanist and structural. To compensate for that 'lack', I have pointed to the possibilities of making 'thicker', first-person narratives based upon microarchaeological analysis. Another central issue is the process of model-building and choice of social categories. This is not a problem restricted to archaeological analysis, but the contemporary social concepts and social significance of corporeal characteristics are likely to differ and become more and more distant as we move back from present-day norms. The prime data of archaeology, materialities and material traces of action, have also been discussed in general terms. Emphasis has been put on their spatial and temporal relations rather than on symbolic 'meanings'.

Perhaps we as archaeologists tend to lay too much stress on the information that can be extracted from material evidence. It may be a burden that the material record cannot sustain because of its fragmented and non-representative constitution. But, after all, that is the major challenge that archaeology has to face in order to be a self-sustained, social science. In one sense, archaeological analysis is an impossible endeavour, as we seek to make sense out of data constituted in times beyond 'nodal points'. This does not necessarily lead to a 'pessimistic' perspective that gives up all claims to making a scientific analysis without falling into the bottomless pit of relativism. The spatial and temporal relations of material evidence are not randomly constituted and ought to provide a sufficient basis for social analysis. The temporal relations between different social *events* and materialities have also been discussed stressing the necessity of relative chronologies and seeking to establish *related* links between different events. The general intersociality of social practice and the porosity of social situations imply that any natural boundaries for social analysis are doubtful. The main idea behind microarchaeological analysis is to start individually from the executed practices of particular microecologies, in order to relate their individual results to a level of higher generality. The concept of microecologies has therefore been proposed as general, analytical frames, from whose particularity we may abduce more general, structurating

positivities, minimising the risk of circular reasoning that haunts much culture-historical, regional or national archaeologies.

Considering the aspects that have been adhered to here, notably the repetitive character of day-to-day actions, seriality, social structuration of space, etc., it may seem that an analytical field such as household archaeology would provide an interesting basis for a microarchaeological analysis. Nonetheless, I will in the coming chapters discuss the possibilities and constraints of applying the microarchaeological approach to another analytic field: the disposal of the dead. Burial archaeology is not exactly something that can be separated from other areas of social practice, but it is an interesting field of various theories and methodologies. After a period of development during the seventies and early eighties, burial archaeology today seems to be somewhat stuck in a traditional state. Instead of a continuous development of the theories and methods of processual archaeology, the same assumptions and techniques are still employed also in so-called post-processual archaeology (e.g., simple concepts of status-role or social identity, etc). The most notable difference is that the archaeology of death has once again become a field of religious studies. I shall thus employ the microarchaeological approach to two separate case studies: one semi-small Neolithic, burial ground (Chapter 3) and some recently discovered burials in the parish of Arcadia, Greece (Chapter 4). Note that these examples have not been chosen because of any outstanding qualities, but as small examples of 'normal' archaeological datasets that is simply chosen on account of personal interest.

3
Burial as Social Practice

> I know Death, I am an old employee of his, one overrates it, believe me! I can assure you that there is nothing at all in it. The mischief that precedes death cannot be ascribed to it, that is a concern for the living, which may lead to life and to recovery. But from death no one can come back and know anything in particular, that is because one does not experience him. We come from the darkness and go into the darkness; between them lie experiences, but beginning and end, birth and death, are not experienced by us. They have no subjective character; they are procedures completely of the objective realm, that is how it is (T. Mann 1991[1926]:732, *my translation*).[13]

The above quotation is from Dr. Berens, a most perspicacious character in Thomas Mann's novel *The Magic Mountain.* In the Berghof mountain resort, all practical arrangements concerning deceased patients are handled by professionals. According to Berens, death is not a concern for the living: Death is just the end of conscious, subjective life and we can only speculate about what may come after. Berens' pragmatic position is perhaps a bit prosaic, but his arguments are nonetheless impeccable. Still, there are many elaborated beliefs and ideas about death and the afterlife. Such speculations, as Berens would have put it, seem to be of major concern for many people. Most eschatological arguments, either metaphysical or rationalist, can generally be 'explained' in terms of psychology; as a fear of the unknown or as a repression of that same fear. Still, the fact of our mortality does effect the constitution of social life in several ways. The knowledge of our limited time–span and our ever-present mortality must have a great impact on general ontology and

cosmology. Some argue that we in the contemporary, western world have alienated ourselves from death and the practices of burial, while others argue that we are more occupied with death now than ever (Huntington & Metcalf 1991: 201; Baumann 1992). Death and burials thus affect most of us in one way or another. It is perhaps in this aspect that burials differ as archaeological objects from more mundane categories. It is hard to ignore the fact that the bones of a burial were once an individual like us or, more specifically, an individual who died (Baumann 1992). This may be one reason for the fascination of burials in the popular media, as well as among archaeologists. It is perhaps in this area of archaeology that we feel closest to the people whose material culture we excavate. That 'empathy' constitutes both an advantage and a weakness. The 'affection' that we tend to charge burials with seems to initiate inferences along certain lines. It is not surprising that burials are the main source for interpreting prehistoric religion and cosmology. That many burials consist of a 'set' of objects, a single individual with artefacts etc., seems to initiate interpretations of the social status or the identity of the deceased. These dimensions of burial data have in recent decades evolved into strands of thought more or less differing from other areas of archaeological data; a *burial archaeology* or an *archaeology of death.* But is such a subdivision fruitful from the viewpoint of the discipline? Does analysis of burials call for a special theory and method? It may very well turn out that such interdisciplinary divisions have a constraining effect and force the material into stiff models hampering the development of more elaborate and creative fictions of the past.

In the following text, I attempt to approach the practices of disposing of the dead as structurating social practice. I discuss graves as archaeological sources of social information and social practice rather than religion and eschatological attitudes towards death and the afterlife. That will, however, not imply that the existential dimensions of death and burial, nor the emotions (grief or joy) that they may evoke are neglected. As discussed in Chapter 2, such aspects are already incorporated in individual agencies and structural elements that constitute most social practice.

Graves as archaeological data

Since the very beginning of antiquarian work, burials have been of major interest. The pioneers of the 17th and 18th centuries, like Olof Rudbeck and Erik Pontoppidan, mainly excavated burials (Klindt-Jensen 1975:29ff). These early antiquarians were, however, not burial archaeologists *per se* but used the material for various inferences, including artefact typologies and chronologies (e.g. Montelius 1884).

The view of burials as 'closed finds' provides assemblages of different artefacts that are suitable for cross-correlations (although that assumption has been challenged, pointing to various post-burial manipulations). Such objections have not hampered so-called *culture-historical archaeology,* which draws on different construction elements and types of burials to distinguish different time periods or 'cultures' and discusses diffusion and/or population movements (e.g., Montelius 1909; Lindquist 1926). *Evolutionary* and *structure-functional* archaeology (old-school, social archaeology or the Binford/Saxe approach) discusses issues of normative social organisation and social structure from intra-site or regional mortuary variability. Differences or lack of differentiation in the grave interments, grave construction or energy expenditure are employed to distinguish traits of social structure (e.g., Saxe 1970; Binford 1971; Tainter 1977; O'Shea 1996). Another type of social archaeology focuses on hierarchy, seeking to establish whether a specific social formation was egalitarian, broadly egalitarian, ranked or divided into classes. Sub-issues here are questions of rank, status and social identities and related questions as to whether such issues are inherited or achieved (e.g., Wason 1994; Chapman 2000a).

Gender and *queer archaeologies* discuss how sex is related to social organisation and structure and question issues of power, normality, appearance and social identity (e.g., Shennan 1975; Whelan 1995; Stig-Sörensen 2000; Strassburg 2000). As mentioned, there is also a *psycho-social* branch that focuses on religion, cult and cosmologies. From this point of view, burials are generally thought to reflect attitudes towards death and the afterlife, which emphasise issues of burial rites, ancestor cult or veneration (Lagerlöf 1991; Kaliff 1992; Carr 1995; Tarlow 1999; Artelius 2000; Härke 2002). Another field that cross-cuts many of these strands is burial analysis, occupied with spatial relations. The general point of departure here is generally discussions about the location of burials and their possible function in landscapes as territorial markers or as symbolic features (e.g., Renfrew 1984:175ff; Bradley 1993; Tilley 1994; Branigan 1998). A little peripherally of these approaches, is the *physical anthropological* (osteological) branch, discussing questions of demography, making estimates of population ratios, life lengths, aspects of nutrition and wear, as well as the age, sex, body posture and race of the deceased (e.g., Manchester 1989). In recent decades, there has also been an interest, albeit on a minor scale, in molecular analysis of the genetic aspects of kin/race and sex (e.g., Horai, et al 1989; Stone & Stoneking 1993; Götherström 2001).

Graves are, and have long been, a major source of information for most archaeologies. But are all of these general areas of research equally valid? For instance, are burials a good source for discussing

the movement and interaction of ethnic groups, issues of individuality and social status, religion or social organisation and structure? Burials are a problematic set of archaeological data. Is the practice of burials primarily an expression of cosmology and religion? Or is it merely a display of social strategies? Do the properties of a burial represent the life and world of the deceased individual or are they mainly a business for the living? We have little means of grasping prehistoric peoples' attitudes towards death and what may come after; what we do have are fragmentary traces of how they dealt with corpses. In the vast body of texts concerning burials, there are fewer critical voices than there is optimistic analysis. This is despite the fact that the basic, critical notions have long since been well known to most archaeologists. The central problems are pretty well covered by two old texts of Kroeber and Ucko.

Comparative and intra-site studies

Kroeber's old article 'Disposal of the dead' (1927) still has far-reaching implications for the study of burials. In his short article, Kroeber stresses the point that burial practices are not a very reliable means of defining cultural groups. His examples, mainly based on ethnographic data, show that burial practices fail to conform to other cultural traits of a given area. Kroeber also found that burial practices vary in relation to rite and level of emotion, that is, there is no correlation with the view of, or the customs related to, the death and mortuary practices of a certain group. For instance, the practice of cremation seems to have originated independently and spontaneously in various places. Kroeber hence argues that change in burial practices is insufficient as an argument for diffusion or population movements. Another important notion is that practices regarding the disposal of the dead fluctuate in time, instead of showing the relative stability that is often attributed to them. Instead of being a cultural trait or directly reflecting cosmology, Kroeber argues that the "... disposal of the dead falls rather into a class with fashions, than with either customs or folkways, on the one hand, or institutions, on the other" (1927:314).

There are, of course, a number of exceptions to Kroeber's cultural relativism, because burial customs are both consistent over time and closely linked to religion and cosmology. But Kroeber's critique still has serious implications for archaeology. If there is no, or little, correlation between affection, attitudes towards death, mourning practices and the actual practice of disposing of the dead, it is hard to sustain interpretations of religion, cosmology, life-world, etc. based on burial data. In one sense, Kroeber's arguments are evident; burial practices are in essence simply a means of getting

rid of dead bodies in 'proper' ways. This is a fact that any social formation needs to handle in one way or another. Of course, such practices can be charged with emotion and affection. Death may provoke fear and ontological insecurity (Baumann 1992; Fuchs 1969; Huntington & Metcalf 1979), not to mention the personal grief at losing a dear one. Of course, death itself may incite some kind of ritual to cope with it.

Binford (1971) and others refute Kroeber's arguments about burials as fashion-like, more or less randomly constituted, and argue that they indeed reflect patterns of social organisation and structure. Binford and his followers mainly focus on the *internal* variability of a burial ground (or burial grounds within a 'culture area') in discussing questions of social identity, social organisation and structure (e.g., Shennan 1975; Goldstein 1981; Morris 1987; O'Shea 1996). Such studies are, however, not less problematic. Peter Ucko's article 'Ethnography and archaeological interpretation of funerary remains' (1969) is smashing critique of the use of burials as sources for social structure and identity. In general, he criticises the simple use of ethnographic parallels. Like Kroeber, he argues that there are no, or very few, cross-cultural regularities. The only sensible use of ethnographic data is to expand the interpretative horizon of the archaeologist. Like Kroeber before him, Ucko argues that burials yield poor data for interpreting attitudes towards death or cosmological beliefs in the afterlife, as well as cultural contact or heterogeneity:

> So much variability has been seen to exist both between cultures and within a culture that it becomes all-important to consider the question whether prehistorians are generally correct in laying so much stress on the usefulness of burial customs to identify different groups of people, and are right in assuming that burial practices are the kind of traits which can be treated as diagnostic of different cultures (and therefore of cultural contact and change) (Ucko 1969:273).

Ucko builds his arguments on a variety of ethnographical data but also from other sources. For instance, he questions the presumptive function and meaning of the grave interments of dogs and cats in the Woodlands Private Animal Cemetery (blankets, teddy bears, collars, favourite food in the form of chocolate and rubber bones) as gifts for use in an afterlife (Ucko 1969:265). Ucko points to the great variability in the ethnographical record of peoples' ways of dealing with death – from obsession with the bodies and bones of ancestors to mundane views of dead bodies as pieces of contagious trash. Neither end of that continuum necessarily signifies belief or lack of belief in an afterlife (1969:265). Ucko hence takes the arguments of Kroeber one step further. Apart from simply exploring the lack of fit between beliefs about death and actual mortuary practice, he also

questions the relation between mortuary variability and social structure and organisation. He argues that we have little means of really distinguishing rich graves from poor ones. Expressions of collective or individual wealth can both be subordinated to social and ritual sanctions and thus skew the picture. For instance, social inequalities can be masked by less fashionable burials for those who actually have the power or wealth (cf. Okely 1979:86, Trinkhaus 1995:57). Burial gifts may have been destroyed before burial (e.g., eaten, trashed or burnt), or simply symbolically 'thought to be there' (Ucko 1969:266; cf. Chapman 2000a). Furthermore, the differences between who you believe you are and what people think you are matters in a general sense, but the way that is signified or masked in burials is not easily predictable.

The two classic arguments of Kroeber and Ucko are somewhat pessimistic and cautionary, pointing out the many fallacies in burial archaeology, a critical stance that is not appreciated by all. Robert Chapman, for instance, simply dismisses the critique as pedantry: "No progress towards these goals [a general theory, MRT, etc.] will be made by negative, particularistic, cautionary tales drawn from ethnography" (1987:210). It is true that one is often able to find a freak example from the ethnographical record to contradict almost any hypothesis, but the known variability in mortuary practices still suggests that each feature needs to be examined on its own terms. Burials can hardly be seen as objective markers for the social dimension or being a neutral reflection of a given social formation (Sestieri 1992:10). There are surely general tendencies in burial customs, but yet too many non-predictable exceptions. It thus seems that comparative studies of burials are a dead end.[14]

The living and the dead: same, same, but different?

Despite much critique, many archaeologists assert that the properties of a burial are in some way related to the buried individual(s). That is a contradictory assumption in basic terms, as burials are constructed (and related to) by the living rather than the buried individual(s): The dead do not bury themselves (Fleming 1973; Bradley 1989:448). Leach (1979:122), among others, has argued that the mortuary variability rather reflects the social aspects of the *funeral organisers* of the deceased than of the buried individual.[15] But some individuals may be well engaged in the planning and preparations for their own deaths. The recent death of the Queen Mother Elizabeth (1900-2002) is one such example. The Queen Mother received a traditional royal burial and her coffin was placed in a vault alongside her husband King George VI. The interesting aspects of this event are related to the particularly old age of the Queen

Mother. Her life-span encompassed two world-wars and many important, social and political changes. Owing to her great age, her death was somewhat 'expected' and her funeral was actually rehearsed in secret during the last thirty years. The royal life-guards and civil servants secretly rehearsed the event and the Queen Mother herself was from time to time involved in the preparations (e.g., Summerskill 2002; *Socialism today* homepage). The long time-span over which these preparations took place raises interesting questions as to what parts of the Queen Mother's identity, personality and preferences (which are likely to have changed during the 30 years) were finally manifested in her burial. The same questions can be asked concerning the changing political and economic changes (e.g., from Tory to Labour government, from cold war to perestroika, etc.). Queen Elizabeth's burial was thus not a private affair, and was not even restricted to her family. It comprised varying interests; the interests of the common people and, of course, the interest of the national elite(s). This contemporary case is special in many senses, but the main theme is probably relevant also to many archaeological cases (cf. Parker Pearson 1982).

Fig. 1. A Roman (6th century) re-burial in a 4th-century sarcophagus from the northern cemetery of Corinth (Davidson 1952:181-5, 294f, Plate 16). The original skeleton and grave goods had been removed, with the exception of an Archaic bowl, and the coffin had been cut to fit the larger individual.

The general principle of the Saxe-Binford approach was that the treatment of the dead mirrors the general social organisation of the living. Tainter (1977:329) writes with confidence: "Indeed, to the extent to which a mortuary population contains individuals who held membership in the various structural components of a system, one can expect the mortuary population to reflect the structure of the extinct society. There is no other category of archaeological data for which this claim might be so confidently advanced." The processual approach saw individuals as caught in a web of statuses and roles, in which age, sex, achievements and lineage were more or less determining for their individual positions. The positions in the horizontal (roles) and vertical (status) matrices fluctuate during the individual's life; an individual may become a member of new fraternities, polities or brotherhoods at some age or after certain achievements (e.g., Crown & Fish 1996). These varying sets of social relationships and positions in the hierarchical matrix were supposed to be fixed at the time of death and many of these relationships were symbolised in the mortuary treatment. This presupposes the existence and significance of a modern concept of individuality and person, which may be questioned in some times and areas (e.g., Baumeister 1986). It also presupposes that the burial was a collective affair, involving a number of people, such as relatives, members of different fraternities or even a whole social community.

We may, however, consider scenarios in which only one or a few actually knew what happened to a dead body after the point of death (cf. Gansum 2002:252). The intermediate phase may be hidden from the major part of the population or even the burial itself can be a matter for a few, even excluding the next of kin, etc. There is also a possibility that the few who know what is happening actually lie or distort the picture of what they are actually doing. That would mean that the general ideology is constructed and reconstructed on 'false' premises. It is thus hard to see how we can ever relate meaning and cosmology to the practices of the disposal of the dead. There may be multiple layers of relations, for instance, one for the living and one for the dead. There may also be a third layer for the intermediate phase between death and actual burial etc. These different systems may or may not be reflecting or at least be related to each other. At least, they ought to be parts of a way of thought, that is, if we assume that death is always meaningful and that all individuals have access to what happens to the dead.

Burials can thus not simply be assumed to reflect the social conditions and ideology of the living. This *may* be the case but is not necessarily. Many have stressed the possible use of public burials as areas for intricate social displays. Burials may deliberately be used by élites to mask social inequalities ('in death we're all alike'). This

need not to be an active statement; social contracts and differences can also be negotiated in death on a less conscious level (e.g., Bloch 1982:218; Chapman 2000a:30). In other words; letting subjected groups and élites have the same treatment in death (and perhaps the assurance of an equally happy after-life) reproduces the illusion of a "good" (equal) society. These many social facets of burial imply problems for archaeology. We do not excavate ideologies or identities any more than we dig up kinship systems; our data are both subaltern *and* ideologically constituted. One example of such problems is to be found in the burial practices of classical Athens.

Houby-Nielsen (2000) has suggested that a change in the status of children can be traced from spatial changes in Athenian burial practices. The cemeteries around Athens generally display a differentiation in space; high-status burials are situated along the roads of the main gates, while less noticeable areas are of lower status. Few children's burials dating from the early Iron Age have been found and those few were normally confined to less attractive areas. Throughout the Archaic and Classical periods, however, children were increasingly buried in the high-status areas along the roads leading to the main city gates. Houby-Nielsen interprets this change in spatiality as a sign of increasing status for children during the formation period of the city-state and a corresponding decline when Athens lost its political independence (2000:161). This is a reasonable analysis but is not necessarily correct. It is obvious that structurating practices changed during this period, but it is questionable whether this implied a real change in status and possibilities of agency for children (and thus indirectly for *living* women). It may be an example of how social gender-contracts are negotiated during burials to 'shade' social inequalities. The latter interpretation is partly supported by the trend towards a liberal 'middling ideology' during the period, as proposed by Morris (2000). It suggests that the change in burial locations was no more than an 'empty gesture' to cover inequalities of gender and age. Notwithstanding which interpretation one favours, the example illustrates how small changes in practice may or may not lead to changes in ideology (and thus in structurating positivities).

Burial archaeology seems to be more about the living than the dead; it is nonetheless true that the dead can intervene in the life of the living. The ethnographical record is full of examples of ancestor cults etc (e.g., Hertz 1960). From a microarchaeological perspective, the dead may thus potentially be *actants,* not very different from other material objects (cf. Cornell & Medina 2001 regarding the adventures of the corpse of Eva Perón etc.). The living are subordinate to their biological bodies' needs and limitations, limitations which the dead have gone beyond, but it is only the living who are able to

act. If we view burials as the remains of structural practices associated with the disposal of dead bodies rather than reflecting cosmologies, we may approach the data from another angle, for instance, by a microarchaeological approach.

A microarchaeology of mortuary practices

In one sense, a burial can simply be viewed as the result of structurating practices, or perhaps bundles of structurating practices, related to more general, structurating positivities. To treat burials as the material results of structurating practices has many advantages. Burials can be analysed as single events and burial grounds as series of events that not need to be related to or compared with general ideas of social organisation etc. The corporeal aspects of the buried (age, sex, body posture, etc.) are normally considered as individual features, but, if viewed as actants, the corporeal attributes are no different from the other materialities of a grave. Such a perspective may lead us to rethink the whole concept of graves and burial. We thus do not need to relate the properties and interments of the grave to any individual in particular. The grave is rather a manifested statement of social practice, in which corporeal variables are not necessarily related to the individual's life. It would be a great advantage if we could bypass the awkward question of individual identity and status in burial analysis and instead focus on identifying subjectivation parameters and social categorisation processes. Here we will find that Sartre's concept of *series* has many advantages in discussing correlations in burial analysis. The problem of representation can thus partly be bypassed by emphasizing the *executed actions* involved in the construction of graves. The dead may very well interfere in the world of the living in various metaphysical ways, but the grave is still the material result of a series of actions.

In a broad sense (i.e. large-scale), we can use microarchaeological terminology to discuss general tendencies in mortuary variability. The complex process of the Athenian city-state of Iron Age Greece may be suitable as an example. The concept of the city-state can be regarded as a particular structurating positivity, linked to a rope or cluster of structurating practices. The social process from aristocracy to citizenship is intricate, involving intentional efforts and resistance by different social groups, but also affected by given sociohistorical and material conditions. Morris (1987) has produced an interesting study, discussing this process, based on the material evidence from burials.[16] Morris argues that a first attempt towards the city-state was in process around 750 B.C, but failed for various reasons; one could say that the fibres involved could not sustain a cluster that could form such a rope. For a number of years,

the process ceased, and the social structure returned to the old order, but only to be re-initiated and finally established with the reform of Cleisthenes in the sixth century B.C.

Morris' study is traditional in the sense that he views the process as a competition between two social groups, *kakoi* and *agathoi*. It would probably be more interesting to try to identify, on a broader level, the social practices involved in this process at large, rather than *only* discussing the power relations between two social groups. From the perspective of fibres and threads, the factors (fibres) involved in this process are many, but it seems evident that some lacked or were inconsistent with others in the early phase. Perhaps others replaced them at a later stage. It is important here that there is no strict teleology in the process (the extent and composition of the thread); the process might have taken other directions within the same sociohistorical context. In a recent book, Morris returns to his old study and refines his analysis by adding the notion of 'middling' ideology to the simple duality of the two competing groups. The speed with which the members of the different city-states embraced the new middling ideology varied, but Athens was in the forefront. Moreover, he expands his original discussion, focussing on the burial customs of the earlier Dark Age and Geometric periods and relates those of Athens to other, early city-states of Greece (Morris 2000:287ff). The burial practices are particularly messy and rapidly changing at this period. This variability has caused a great deal of puzzlement for the more traditionalist archaeologists. For instance, Robin Osborne (1996:85) is amazed at how the same person could be buried with the combination of strict Geometric pottery *and* orientalising patterns on gold bands. He understands eighth-century Athens as suffering from 'cultural schizophrenia' and a 'style war' (cf. Morris 2000:300). A noteworthy point here is that these 'messy conditions' might have been less puzzling if the frame of study had not been confined within the national borders of Greece, assuming a unified, ethnic group of Greeks. For example, the orientalia found in the burials hints at intermixing structurating fibres between the heterogeneous 'cultural' spheres of the Near East and that of Greece. As demonstrated in Chapter 2, it is difficult (if at all possible) to establish any absolute etic or emic boundaries (social or physical) in which social practice or experience form a homogeneous entity. We may as well depart from the microanalysis of smaller locales, keeping in mind the interrelations between the local and the regional, as well as between the particular and the general.

Detailed studies have become somewhat fashionable in the new millennium. One example is Chapman's (2000a) analysis of burials in later Hungarian prehistory. He argues that burial analyses generally are 'heavily under-theorized', especially concerning

agency-structure relations (2000a:162). Chapman's approach is similar to microarchaeology in some respects. He argues for detailed analysis of smaller groups of graves within a cemetery rather than analysing cemeteries as closed entities. In this way, he wants to illuminate differences in local *microtraditions* and global structures (2000a:28). The local is not simply a reflection of the global or vice versa; small-scale actions can form microtraditions which are related to general structure (2000a:69). The global structure is thus a 'post-hoc etic statistical summary' of a variety of local microtraditions (i.e. agencies). By contrasting a general and a particular analysis of the same material (burials of the Hungarian Copper Age), he finds great variability locally between 'households' but also general trends of global structures. Chapman seeks to show how global structures are actually results of 'emic decision-making', that social actors actually are the 'creators' of their 'culture specifics' by their active decisions and by their daily practices (2000a:161). Chapman's approach is methodologically interesting but is weak in theory. It is a bottom-up perspective, in which general 'culture' is built up via local microtraditions based on self-categorising agencies of locally situated individuals (2000a:161). The dialectic of the structuration process is thus skewed in favour of autonomous individuals who are able to subjectivate themselves. Such abilities are, however, more of a wishful thinking than a social reality, although some individuals, past or present, may believe that they possess such powers. Chapman's study also suffers from his use of generalising concepts like 'households' and 'culture areas', as if they were given, unproblematic entities.

Analyzing mortuary variability

Mortuary variability, in the sense of variation in the interments and properties of the graves, is traditionally the prime source in the search of social differentiation in burial analysis, which has provided a substantial body of literature (e.g., Shennan 1975; Chapman, Kinnes & Randsborg 1981; Alekshin 1983; O'Shea 1984; Morris 1992; Parker-Pearson 1999; Jensen & Høilund Nielsen 1997; Gansum 2002). There are a great many approaches to how to deal with such variability and how it may relate to the world of the living. For instance, how can we compare and value different sets of interments or differences in grave construction? Here we may find calculations of energy expenditure (Tainter 1977:332) or the rate of artefact-type variability (Hedeager 1990) valuable. Other questions are more awkward, such as distinguishing the aspects of the variability that are related to chronology and what is evidence of social differentiation? How do we categorise and quantify objects and artefacts in the

best manner? There are no definite answers or methodologies for dealing with these questions, but some ways seem better than others. In a general approach to mortuary variability, we can find inspiration in the work of Ian Morris (1992). Morris suggests five general axes along which we can quantify mortuary variability in the search for patterns and their significance (1992:24-8).

The first axis is *typology*, that is, the categorisation of the objects (not to be confused with the Montelian method of typology). This is not a straightforward task leading to only one final result, but it is still necessary for comparison and finding patterns. The process of categorisation often contains elements of selection and objectification (naming). An object can be apprehended in many ways, through several senses, and thus has a limited number but yet a multiplicity of possible significations (Ingold 1992). Willy Ørskov (1966:47) has discussed the process of objectivation in terms of 'killing' the object. When an artefact, let us say, a piece of bronze is classified as a 'razor', we have restricted the many other possible interpretations and cut a tiny slice of the possible variety of information and social load off the object (Cornell & Fahlander 2002a:3). Still, classification and typology are inevitable archaeological processes, from which we cannot escape; we need to make a number of objectivations during archaeological analysis. One problem is that we tend to make such categorisations with particular fictions, analogies and presumptions in mind. Objectivation and classification cannot be purely objective and valueless, but we may avoid some unnecessary reductions by registering as far as possible, the 'properties' of objects rather than types. We may also allow objects to be parts of multiple categories. The 'razor' can be a piece of bronze with certain characteristics instead of a personal 'toilet article'. Such element-analysis seems to be a pragmatic way of handling objects of another symbolic order. Notwithstanding what the 'razor' was used for, it may very well be present in a particular context for several reasons (as a cutting item, bronze piece, etc.). By using attribute-analysis, we may be able, at least momentarily, to 'sustain the life' of the objects (cf. Strassburg 1998).

Morris' second axis is *time*. It is perhaps the most complicated, as reliable chronologies or positive dates are often absent or insecure. The temporal axis is important, as we need to identify long-term changes in contrast to contemporary or short-term variability. In favourable cases, we may even be able to distinguish action events or chains of such events (cf. Chapter 2). Chronology and typology are in many cases difficult to separate from each other. One cautionary example is Tilley's analysis of the Fjälkinge megalith (Shanks & Tilley 1987b:105-71). Tilley organised a vast amount of TRB pottery deposited outside the entrance of the grave, according

to decorative elements (open and closed shapes). This structural analysis provided him with a conceptual platform on which to discuss ideological change in the 'TRB culture'. However, he mixed typology with chronology when he used the same attributes (ceramic pattern design) for social analysis that had originally been used for dating. The separation of chronology and 'horizontal' variability is thus crucial, although it poses problems that may not be surmounted even with well-dated data. For instance, where do we set the limit for contemporariness? One year, five years or the normal span of a generation? This is a question that must be attended to in each individual case.

The third axis, *contexts of deposition*, can be informative but is not always available or easy to define. Here Morris hade in mind additional local aspects, like the size and organisation of associated settlements and houses, public buildings, sanctuaries and hoard finds. The patterns of such contexts can be compared with patterns found in the burials. It is not clear whether what Morris has in mind is a process of validation by comparing evidence from another sphere to sustain an interpretation. It cannot, however, be a straightforward task to compare the fields of the living with those of the dead, but both coinciding and contrary patterns are interesting to discuss. As the object of study in the microarchaeological approach is social practice, it may be contradictory to separate the structurated practices involved in building houses from those involved in burial. There is, however, no reason why different practices performed at a locale should necessarily correlate. Context of deposition is thus more of a series of social practices that may in some ways be related to burials, but in others not.

The fourth axis, *space*, is an extension of the third axis involving comparisons with regional data in the search for general structures that can explain local phenomena. To employ correlations with general textual evidence, so-called 'contemporary analogies' (Andrén 1997), or archaeological information from other, 'similar' sites is, however, tricky. From a microarchaeological point of view, this is something that should be minimised and preferably applied only between independently micro-analysed areas. Chronologies and typologies are often constructed from a wide variety of data, implicitly suggesting that all types in a series are related, despite wide, geographical distribution. Regional chronologies do not contradict the idea of microanalysis, as they are normally constructed from stratigraphy or radiocarbon-dated, material evidence. Generalisations and comparisons between bundles of fibres, that is, typical series of practices of a cultural unit, are something different. Structurating practices are often common within or across regions, but we cannot presume that they are evenly distributed in time and

space. Moreover, their social significance and function are likely to be different. Structurating practices may be adapted differently to local conditions, misunderstood, distorted, developed or unknown as between different locales. The axis of *space* is thus better understood as a category of *local* spatial organisation on different levels (e.g., the outline of the burial ground or the distribution of objects within a grave).

The final axis, *demography*, is one of the most commonly used for burial analysis in the search for hierarchical structures, gender groups and individual status, but also for discussing life-length figures, diseases or nutrition. Here, I propose a slightly different approach, which may be termed 'corporeal socio-demography'. Demographic analysis in archaeology normally concerns population density, composition and spatial distribution as well as changes in population structure. Demography is not, however, only restricted to questions of population movements and density. As pointed out in Chapter 2 we may explore the concept of the body as an actant, that is, how individuals are subjectivated and categorised on the basis of phenotypic characteristics. In general, the traditional categories of the social sciences are predominant in archaeology: class, age, sex/gender, ethnicity and status/prestige (e.g., Binford 1971; O'Shea 1984; Parker Pearson 1999). These 'objects of knowledge' may be significant for social analysis, but they nonetheless have quite recent trajectories as historical actants (Haraway 1991). Also Bhabha (1994:1) has criticised the social sciences for being too narrow-minded on this matter, pointing to other important aspects, such as generations, institutional location, geopolitical locale etc.

These categories are seldom heterogeneous or stable but, on the contrary, encompass great variability and are thus difficult to define. Here we may again find Sartre's concept of series fruitful. As archaeologists, we can establish *analytical* series based on the corporeal aspects and their *associations* with material culture. One typical example of such series is that of age and age-groups. It is assumable that most social formations recognize at least some rudimentary, age phases (cf. Linton 1936:118). Age phases may have identifiable, corporeal traits but are in general very fuzzy. We may need to discuss 'social' or 'mental' age in relation to chronological age. There may be certain 'thresholds' that separate one age from another, such as puberty, menopause, first/last childbirth, grey hair, stages of knowledge, physical strength etc. (e.g., Crown & Fish 1996; Stoodley 2000; Joyce 2000). A particular case of a demographic series concerns the fuzzy category of children. Children have for a long time being neglected in archaeology (Johnsen & Welinder 1995; Sofaer-Derevenski 2000). The lack of grave interments and the generally less elaborate graves of the young can be interpreted as meaning

that children up to a certain age were regarded as being of less value (e.g., Stoodley 2000:495). Contrary to an analysis of adult burials are the young's grave interments sometimes not even related to them, but to some adult. For instance, children buried with tools that they could never have used in real life are attributed to an adult next of kin etc. Examples of evidence that actually indicates that children of some age were subjectivated as a group are to be found in Bronze Age burials at Branc and Britain, in which miniature weapons and tools have been found in the graves of children (Shennan 1975:282; Stoodley 2000:458). As corporeal objects, children may be contributing and constraining, but they are part of the structurating process in any society and thus need to be included.[17] We cannot be certain that grave interments are related to the buried individual(s), but, if we treat the individual as a composite actant like any other material object, we may find patterns in the relations between the corporeal aspects and the associated materialities. By such an approach, we do not need to confine our analysis within the traditional demographic categories but may also try out less presumptuous approaches. The main goal is to find which elements were active in the social subjectivation and categorisation processes of a given microecology.

Summary of the general arguments

Graves have generally been approached with a number of less evident postulates. Burial data are believed to have some general advantages for social analysis: they normally have a more varied and richer composition than most other archaeological objects and constitute a 'time capsule' (Johansson 1996:19; Sestieri 1992:9). Moreover, grave interments and grave constructions have generally been assumed to be related to the buried individuals' social personae. Burials are also commonly presumed to reflect a general cosmology, a given cultural, regional or ethnic community. Besides mirroring the social constitution of a given society it is also suggested that burial practices to reflect attitudes towards the afterlife and even religion. These postulates are, however, questioned by the 'cautionary' examples of e.g., Ucko, Kroeber and others. On the contrary, burials seem to be quite varied and multifaceted. The number and composition of the 'undertakers' of a burial and their relations to the dead are, of obvious reasons, difficult to establish. The same problem also regards the rate of public knowledge of the burial act and access to the grave or burial ground.

Despite the number of possible alternative scenarios that can be discussed on the basis of burial data, it is amazing how similar burial analyses frequently are. There are a large number of texts concerning burial and grave analysis, but there have still been sur-

prisingly few theoretical and methodological advances. It is thus, as Chapman points out, easy to criticise, but a great deal harder to come up with alternative approaches. It is therefore now necessary to leave the general discussion of examples and counter-examples and to discuss some examples in detail. One interesting example is the burials at Ajvide on Gotland. The Ajvide site has not been chosen for its outstanding potential for microarchaeological studies, but rather as an example of a burial ground that is of moderate size, yet complex enough to support elaborate analysis.

The burials at Ajvide

The burials at Ajvide are situated in Eksta parish in the south-western part of the island of Gotland in the Baltic Sea (see Fig. 2). The whole area of prehistoric activities covers about 90.000 m^2 and is situated c. 1 km from the present shore-line at 12-17 m.a.s.l. The shore-displacement processes changed the landscape in drastic ways during the period of occupation. During the Middle Neolithic phase (ca. 3000 BC) the site constituted a protected bay which a thousand years later had turned into a brackish-water lagoon (Burenhult 2002:31; see Fig. 3). The Ajvide/Jacobs site complex was discovered in 1922 and trial-excavated a year later, which indicated the presence of Mesolithic and Neolithic culture layers (Nihlén 1927:93ff). In 1958, a skeleton was accidentally uncovered by a plough, which brought about additional interest in the locale and since the early 1980s the large Ajvide site has been continuously excavated, exposing thick, cultural layers dated from different periods, as well as a burial ground (Österholm 1989:85; 2002a:17).

I will mainly focus on the burials at the site, although their possible relations to other previous and contemporary activities will be attended to, when appropriate. The burials are generally attributed to the Pitted Ware Culture complex (GRK), which is traditionally regarded as having been coast-bound, mainly utilising marine resources for subsistence (e.g., Kaelas 1976; Malmer 1975; Åkerlund 1996; Segerberg 1999). I shall not, however, contribute to the debate or speculate on the relation between the 'GRK, the 'TRB' or the 'STR' (for a critical discussion, see Simonsen & Munch 1973; Larsson, Olsson & Biwall 1997; Strinnholm 2001; cf. Malmer 2002). The people that buried their dead at the Ajvide will rather be regarded as one or more serial collectives defined by their relations in space, time and structurating practice. In some cases, it might be interesting to compare the Ajvide data with the general patterns of other Middle Neolithic sites, for instance, the number of contemporary sites on Gotland (see e.g., Janzon 1974).

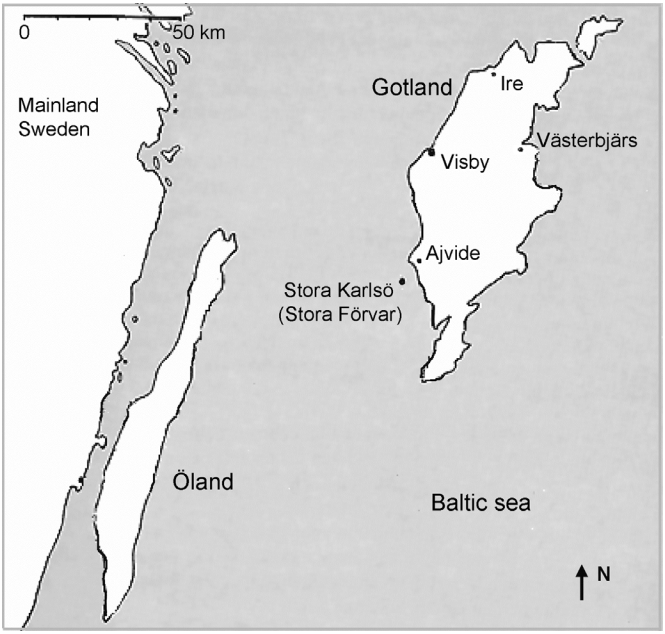

Fig. 2. Ajvide and other mentioned sites on Gotland.

Nevertheless, I will try to minimise the regional and general, contemporary analogies in my analysis of the local practices at Ajvide. The evidence is thus deliberately not set in a comparative context; my aim is to reconstruct local social practices regarding the disposal of the dead and to discuss associations between corporeal attributes and materialities. I shall start with a general survey of known data according to the five axes suggested by Morris and discuss possible interpretations after applying some analytical strategies and operational techniques.

Context of deposition

The local context of deposition is complicated at Ajvide. The relations between the different activity phases are generally uncertain. The evidence ascribed to the older Mesolithic phase suggests that the uppermost area (i.e., the Jakobs site) was populated from c. 5000 BC. There are no positive dates for this phase but what can be inferred from the typology of the axes found (cf. Österholm 1989:89). The axes indicate Mesolithic activity, but not necessarily habitation.

Burenhult argues that the large number of axes found in the area is too many to represent lost or trashed items of 'normal' habitation activities but is more likely to represent a place for depositing axes (1997a:20). The Mesolithic phase is clearly separated from succeeding phases by shore-displacement evidence. A mid-phase (c. 3000-2750 BC) is distinguished in the upper part of area D (Fig. 4). It is argued that it was of short-term occupation (c. 150-200 years), but was yet a very intense phase, judging from the thick and rich culture layers (up to 1 m). The burials at Ajvide are believed to belong to a succeeding phase, approximately between 3000-2000 BC. They are separated from the mid-phase, as some of them were dug through the culture layer.[18] The cultural layer of the mid-phase is also distinguished from the burials by differences in ceramic technology (Burenhult 1997a:19; 1997d:52f). No burials can thus be related to the two earliest occupation phases at Ajvide. There are, however, a cultural layer roughly contemporary with the burials, according to ceramic technology and radiocarbon datings at the lowermost part of the site (Ajvide D lower), situated c. 100 m south-east of the burials, that may be related to some of the burials (Österholm 1989:99; Burenhult 1997d:52; 2002:32; Österholm 2002a:21, 24). The present evidence does not suggest that there are simple relations between the habitation areas of the vicinity (i.e. a site and its burial ground) and the burials of area D.

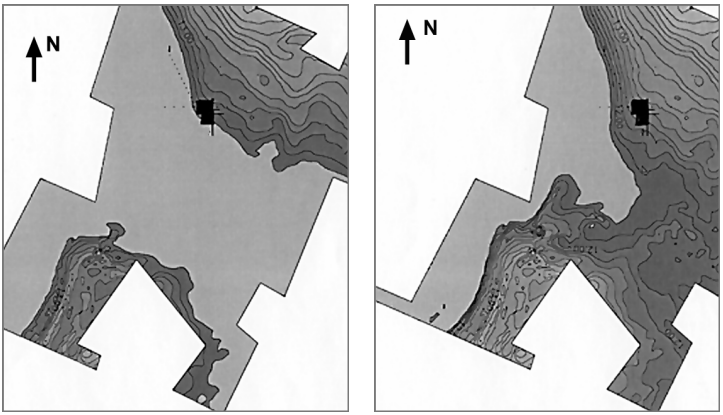

Fig. 3. The different shore displacements at Ajvide (site marked in black). The left-hand figure shows the shore-line at c. 2950 BC., and the right-hand shows the shore-line at c. 2300 BC (modified from the CD of Burenhult 2002, colour plates 5 & 6).

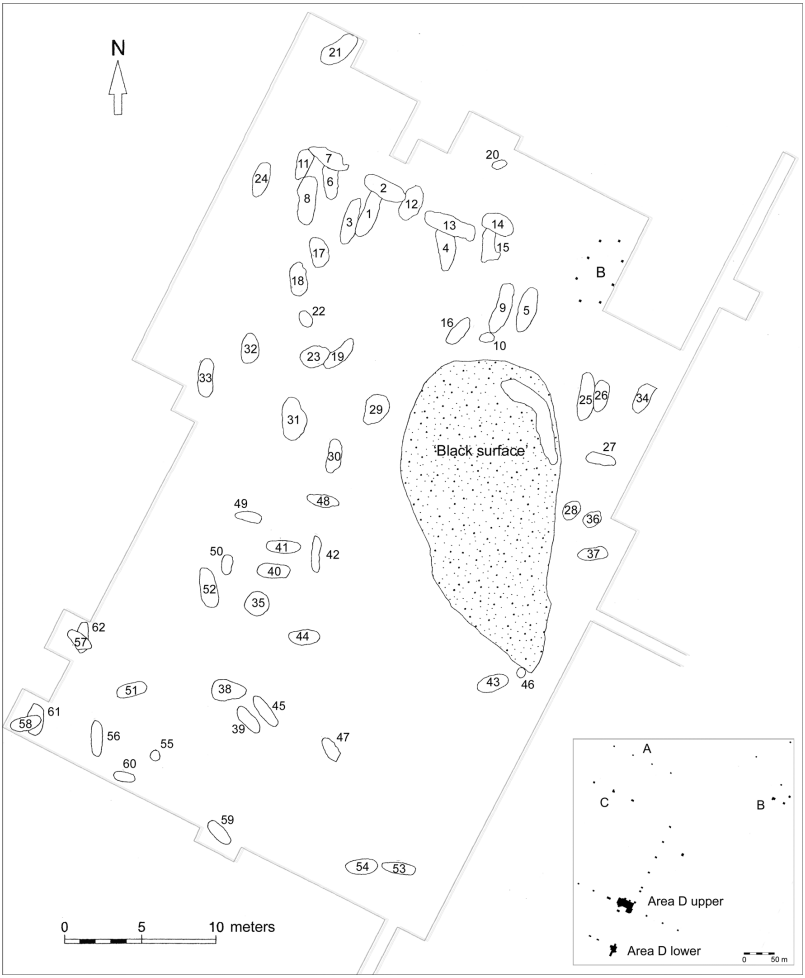

Fig. 4. Sixty-two of the 71 excavated graves at Ajvide D upper, the house (B) and the 'black surface' (after Burenhult 2002). The most recently excavated 9 graves (mainly situated at the south-western end of the area) have not been properly published yet, but some information can be found on the Ajvide homepage.

The central part of the Ajvide site (D upper) consists, in addition to the 71 burials (including 8 'cenotaphs') excavated so far, of what is referred to as a 'seal-altar' (a partly enclosed thick layer of black soil), c. 300 post-holes, and cultural layers with large amounts of bone and stone artefacts (Burenhult 1997a:18f, 1997d:54; 2002:32).

The post-holes have been differentiated in groups according to depth, shape and colour, which may provide a basic hint of their temporal or functional relations (see Burenhult 1997c:46). There are few indications of their function, but a series of larger post-holes in the north-eastern part of area C indicates the presence of a house attributed to the mid-phase (Österholm 1997:75, 77). A number of the registered post-holes may also be related the burials; Burenhult (1997a:18) suggests palisades, death houses, offering-platforms, carved images of ancestors). There is no clear patterning among them besides the fact that some of them seem to have at some point partly enclosed the black surface. The post-holes may turn out to be informative if they were subjected to more detailed analysis than what is feasible in the scope of this text. I have not placed any great emphasis on the post-holes, mainly because of the problems of distinguishing the ones related to burials from those related to other, previous or later activities. It may, however, be worth noting that the different types of post-holes show some spatial differences, notably the distribution of the 'purple' ones, which are found only in the northern part of area D upper.

Time

The general assumption of the excavators is that the burial ground was continuously used over a period of 450 years between 2750-2300 BC, separated from other activities in the area (Burenhult 1997d:54). This assumption is, however, questioned in a number of instances, to which I will return. There are a number of relative chronological traits and evidence of changes in burial customs and attitudes to death. Radiocarbon datings of human and animal bone have been taken from eleven individuals, six seals, and four terrestrial animals as well as from the cultural layers. There are also a number of mainly, preliminary thermoluminescence datings of pottery (see Österholm 1989:93, 123). The radiocarbon datings are somewhat complicated, as the reservoir effect needs to be treated differently as between seals, humans and terrestrial mammals. A higher degree of marine diet (i.e. higher C13) generally gives older dates than terrestrial diet. The rate of marine vs. terrestrial food, according to the C13 values of the human individuals of Ajvide, varies between 62% and 86%, which give a reservoir correlation value of 186 to 258 years prior calibration (Lindquist & Possnert 1997a:74; cf. Possnert 2002:169-72). Hence, human datings are being reduced overall by 200 years and the seal datings by 300 years (leaving the terrestrial animals unaffected). This is, however, not a simple procedure. Seals may have travelled long distances and perhaps spent time in the North Sea, which would have affected the reservoir ef-

fect. It may also be too much of a generalisation to assume, on the basis of the C13 analysis of 10 humans, that all humans were affected in the same way (i.e. mainly by eating non-terrestrial food). In fact, if we look more closely at Table 1 in Appendix A, we find that the rate of marine diet fluctuates from 62% to 80%. We can also see that the two graves (2 and 29) have been radiocarbon-dated from bones from three different sources (human, seal, hedgehog and pig). The human and the seal datings are correlated for the reservoir effect, yet the C14 datings of bones from graves 2 and 29 do not correspond. The reservoir calibration of the human bones of grave 2 makes the date correspond better to those of the seal and hedgehog, whilst the marine impact on the human in grave 29 is insecure. We are thus faced with a greater uncertainty than first meets the eye and need to be careful and avoid jumping to conclusions based upon the radiocarbon datings.

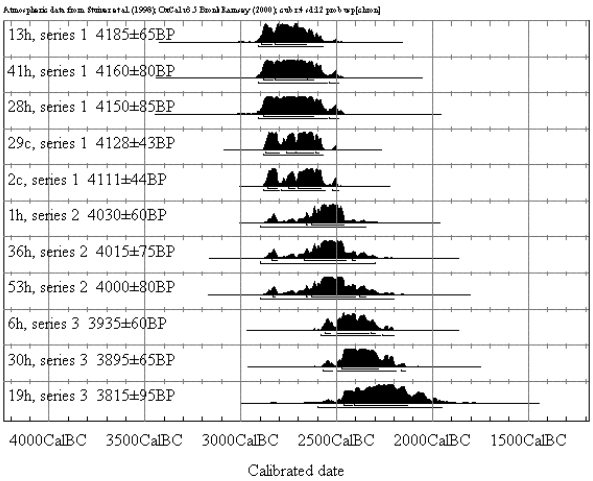

Table 1. Calibrated radiocarbon dates of human and animal bones; reservoir effect correlated (years in BP). Abbreviations: h=homo, c=dates of three different sources are combined. Data taken from Österholm 1989:93, 123; Lindqvist & Possnert 1997a:74; Burenhult 1997f:xviii-xix; Lindqvist & Possnert 1997b:55f; Possnert 2002:171f.

In general, the radiocarbon datings seem to fall into three rough series: 2900-2400 (series 1), 2600-2400 (series 2), and 2500-2100 (series 3) BC (cal. 80%). The middle series of radiocarbon dates cannot, however, be assumed to be independent; these graves are probably

better seen as belonging to either the first or the third series. The radiocarbon dates hint at a time span from the first burial to the last; a maximum span of a millennium (c. 2900-1900 BC) and a minimum of 500 years (c. 2700-2200 BC). In addition, the thermoluminescence datings from the cultural layer of D (upper) span from 3970-1620 BC and in area D (lower) from 2680 to 2820 BC (Österholm 1989:123).

There are also some relative traces of sequences that can be established by consulting the stratigraphy. The clearest chronological traces are those burials that intersect with older burials. Burial nos. 2, 7, 13, 14, 57 and 58 are clearly later than the burials that they are dug into (nos. 1, 4, 6, 11, 12, 15, 61 and 62). The question still remains, however, as to how far are they separated in time. An interesting aspect relating to this phenomenon is the difference in orientation of the graves. The older, disturbed burials are all oriented in the N-S direction, while the later are more or less oriented E-W (cf. Burenhult 1997d:54). It is tempting to make a distinction between older and later phases based on the orientation of the burials, but we need to consider that the radiocarbon date of one of the intersected graves of N-S orientation (no. 6) actually belongs to the latest category. The two general categories of orientation also show great deviations from the ideal orientations respectively. We need to discuss as to what factors may have determined the directions to evaluate their chronological or social significance.

We also need to find ways to get more fine-grained results and seek, if not the event, then at least the series of graves that can be assumed to have been constructed by a serial collective, that is, people with some common, social relations. At first glance, it seems that this is possible at Ajvide. The main problem is, as usual, to isolate the chronological traits from social or non-significant variables. There is a possibility of finding chronological significances in the spatial arrangement (lines, clusters, orientation) but also in associations of materialities. I will return to the matters of chronology and, with the help of relations of the data, seek to at least divide the graves into a number of phases in order to discuss changes etc.

Space

Orientation and alignment may not always be very important, but at Ajvide these particular aspects seem to matter; there is an unmistakable tendency of patterning. Orientation may be a significant, chronological trait that may help to divide the burials into phases or series. We cannot, however, assume that this is a general, chronological trait. For instance, are the E-W directed are burials not as consistent in direction as the N-S ones (Burenhult 1997d:54). Discrepancies and variations in this respect do not necessarily mean

anything at all. The orientations may be related to cosmology and ritual, but their meaning(s) are not necessarily very important. The graves may also be aligned according to a mythical or actual origin (which may differ between different series) or in relation to previous constructions in the vicinity (cf. Bradley 2002:18, 28, 32). What matters is what the variations in orientation may tell us about chronology and social differentiation. There are many possible candidates to explain orientation; the sun, moon or other interstellar objects, topographical aspects or relation to 'natural' or 'cultural' features (signifiants). If the sun is the prime signifiant, the orientation of the grave pits may vary, depending on the time of the year or the time of the day (cf. Hårdh & Roslund 1991). The burials at Ajvide do seem to fall within the regular variation of sunrise in the Gotland region (see Fig. 4). The spread may thus depend on the time of the year when the burial was conducted.

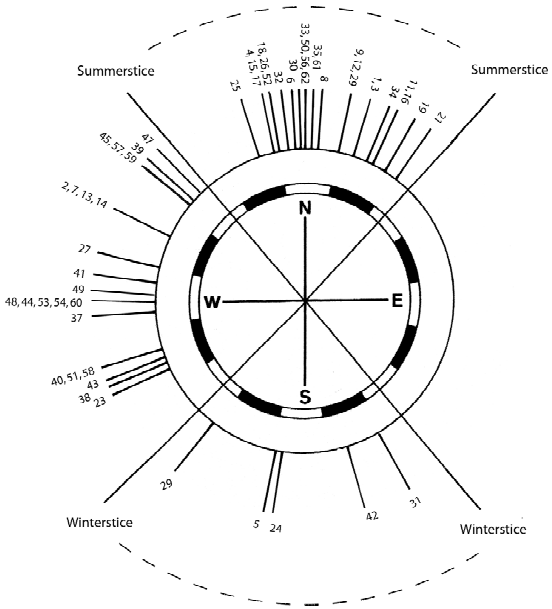

Fig 5. The variation of sunset/sunrise on Gotland and the orientation of the graves. Numbers 10, 20, 22, 28, 36, 46, 47 and 55 are omitted, as considered to have particular orientation. The figure is not complete regarding the place of the head of the dead individual; the graves are thus generally illustrated as either of northern or western orientation when data are uncertain (image modified after Janzon 1974:29; Österholm 2002b:182).

There certainly are some clusters of graves that share orientation, which may indicate that some of them were constructed in a single event. For instance, graves 2, 7, 13 and 14 graves 44, 48, 53, 54 and 60 and graves 30, 33, 35, 50, 56, 61 and 62. Some of these regularities are probably coincidental, but a few of them may actually turn out to be of the same date. This is an important issue that I will return to further on. Discrepancies between ideal and real circumstances may certainly account for some variability, but, if some interstellar objects are the main signifiants responsible for the orientation of the pits, we may as well analyse them from a schematic image of ideal orientation. In Fig. 6, all burials have been rectified to illustrate an 'emic' view of perfect N-S and E-W orientation.

Fig. 6. The graves rectified according to their possible, ideal orientation.

There do not seem to be any relations to the 'house', which is aligned in the NNE-SSW direction. There is, however, a possibility that the burials were aligned to topographical features, for instance, the shore-line, which may explain some of the variability concerning its fluctuations. There are a few local or distant, visible topographical features (*signifiants*) visible today, that can explain the orientation of the burials or the place itself. It is possible that the islands of Lilla Karlsö and even Stora Karlsö were visible on the past shore-line, but only a few of the graves are aligned in a direction that coincides with any of the islands. If visible signifiants were responsible for the orientation of the graves, one would expect a greater degree of regularity than in the case of fluctuating celestial objects. It thus seems likely that the cause of the orientation is not to be found locally, but rather in the celestial sphere. The N-S orientation is a general orientation for Middle Neolithic burials on Gotland. It is, however, true that no other sites on Gotland have as many E-W oriented graves as the Ajvide site (Österholm 1997:82).

According to early accounts of the excavators, the burials are placed N-S in an oval shape, following the contours of the black surface (Burenhult 1997d:53; Österholm 1997:81). This pattern is, however, restricted to the first excavated graves in the northern part of the area. The other graves seem to be more or less randomly distributed. There are no graves in the area of the 'black surface', which may indicate that it was visible on the surface for the 'undertakers' and was avoided for unknown reasons. But, in view of the fact that the earth of the black surface still smells of fat and train-oil, it would not be surprising to find that this particular area was omitted when digging for burial even if it was not directly visible. One may wonder which parameter predominated as between place and orientation; did they choose a place whose topography suited the ideal orientation or was it vice versa? Another curious, spatial aspect at Ajvide is the 'empty' areas which contain no burials (cf. Österholm 1997:81). One is found due north of the black surface that is surrounded by a number of tightly arranged burials. Another, conspicuous, empty area is south-west of the black surface. Even if the black surface itself was avoided, it does not explain why the area south of it was not used for burials. Is it possible that a shallow layer of similar soil actually covered a larger area than is visible today? The disturbed graves are relatively evenly distributed over the site and do not indicate any especially exposed zones, which might explain the empty areas of the site. The post-holes do not provide any clear indications that might explain these voids either (see Burenhult 2002: colour plate 13 on CD). Such empty areas may be the result of a number of unintended circumstances, but the fact that burials have been intersected at seven instances (10%) suggests that the area

for burials was in some ways restricted. It is thus likely that there were some visible, superterrestial remains from previous or contemporary activities which prevented the use of the area rather than grave-markers.[19]

A promising approach for the purpose of identifying internal sub-groups horizontally within burial fields is advocated by Chapman (2000a) and Müller (2002). They make use of the tradition of burying the dead in rows at certain cemeteries. This approach is, however, is not necessarily interesting at Ajvide, as the burials need not have developed in such a way. There seem, however, to be some possible lines of burials of different lengths, similar to those found at STR cemeteries that may indicate a sequence of events. The obvious problems are to know which end of the line is the first and whether such a system of lines was employed at Ajvide. Another, less distinct way would be to seek for spatial clusters of graves that appear to be more spatially or typologically related to each other than others (cf. Morris 1992:190-93).

Demography

According to the excavators, the social demography at Ajvide seems to mirror a 'complete Stone Age society' (Ajvide homepage: gravar). Indeed, the distributions of sex and age seem to follow a 'normal' pattern, including newborn children as well as elderly individuals up to 70 years of ages of both sexes. No traces of violence have been found on the human bones, apart from one fracture of an individual's right leg (Molnar Appelblom 1997:92). However, many bones show marks of occupational stress caused by excessive labour. This phenomenon has been suggested to indicate traces of spear-throwing or canoe-paddling but may as well be due to the dragging of fishing nets or heavy boats up on land etc. (Burenhult 1997e:179). The buried individuals are in general of normal stature; males have an average height of 171 cm and women of 156 cm.[20] A gross division of different sexes and ages is as follows: seventeen children (approx. 0-12 years, not sex-determined), twenty-two adult men (c. 36% between 50 and 70 years old), and nineteen females, (c. 37% over 40). To these numbers, the eight cenotaphs and six indeterminable individuals (incl. 3 adults) are to be added. The average age of adult men is 43 and of women 35, and the general life expectancy at birth (children included) is c. 30 years. The adult-child ratio is c. 3:1 (27% in general or 33% if cenotaphs and indeterminable individuals are omitted). These are quite normal values for agricultural populations, but maybe a little high for hunter-gatherers (cf. Morris 1992:183). The suggestion of Ajvide being a community burial ground for a tribe-like type of group is thus at first glance sustained.

The normal distribution of individuals at the site makes it less probable that other individuals of different statuses etc. were buried elsewhere, but such a scenario is, of course, possible. The main question that the demographic data pose is under what circumstances the area was used as a burial ground; for how long, continuously used or in separate phases, and by whom? The demographic numbers may thus change if the graves originated from separate series of individuals in different phases, instead of from a continuous use by only one social community.

The radiocarbon dates of the eleven individuals suggest that the area was used for burials over a period of c. 500-1000 years.[21] Assuming a continuous use, this suggests that burials occurred between every 7th or 14th year. The number of generations for the same time would range from 16 to 33. Do these numbers make sense? Österholm (1989:181) has calculated, on the basis of all known Neolithic sites, that Gotland encompassed 14 separate groups with c. 35 individuals in each. Counting four generations per century, this makes about 15-20 generations (Österholm 1989:193). The calculations are based upon the hypothesis that it takes a minimum of c. 500 individuals for an isolated population to constitute a healthy population. How do these figures fit with the evidence from Ajvide? Ahlström (2001:334) provides a simple formula to calculate the approximate number of individuals:

$$P = \frac{D \times e_0}{t}$$

P = population, D = number of buried individuals, e_0 = life expectancy at birth, and t = the time-span of the burial ground.

According to this formula, we find a maximum number of 4.7 (500 years) and a minimum of 2.4 (1000 years) individuals.[22] If we reverse the calculations and instead base them upon Österholm's estimates, we end up with a period of use at Ajvide for about 67 years (2-3 generations). These rather strange figures would, however, turn out differently if the burial ground was used periodically instead of continuously, for instance, a scenario of two or more phases of quite short extent. This is one aspect that I shall return to in the section on analytical strategies and operations.

Categorisation of graves and interments

All the burials at Ajvide were inhumations (except for the cenotaphs). The general practice was to put the deceased individuals on

their backs in extended positions in a shallow, oval pit. A number of the graves are quite long, up to 4 meters, while others are 'normal-sized' or simply round pits. The extreme length of some graves requires some sort of explanation (17 are over 2.5 m and 10 over 3 m). One hypothesis suggests that the dead were buried in some kind of boats, possibly logboats (Janzon 1974:12,22; cf. Malmer 1975:49; Malmer 2002:92). There is, however, no material evidence to support either assumption, except for the dark colour of the pits. Österholm (1989:93) has suggested that the coloration rather was due to train-oil, which may have been used in a part of the burial ritual. She particularly addresses the case of the bones in grave 16, which were black in colour and thus seemed to be impregnated with train-oil. This case is, however, not true for the majority of the burials and similar extended graves found at other sites on Gotland (e.g., grave 24 at Västerbjärs; Janzon 1974:17). The question of the construction of the graves is important, as it helps to analyse the material evidence recorded from the graves. The excavators have been careful in separating actual grave goods from accidental material that adhered to the layers through which the grave pit was dug. That is especially important at Ajvide in view of the fact that the burials were dug through a previous culture layer. Broken or fragmented artefacts may be deliberately deposited (e.g., Chapman 2000b), but, if some individuals were buried in boats, it is likely that many of the small fragmented, sift-finds, like broken fish-hooks etc., can be regarded as unintended depositions.[23] Such small items may have been dropped or trashed during previous uses of the vessel. A similar consideration needs to be given to remains of clothing' and 'personal ornaments', which do not need to be of same status as other intentionally, deposited interments. Material evidence of dress and clothing can, for instance, vary between summer and winter burials or indicate different social series.

The energy expenditure invested in the burials is, from the general point of view, relatively homogeneously distributed between the sexes and ages, which may indicate an egalitarian and sexually equal community (Burenhult 1997e:176; 2002:34; cf. Österholm 1989:182; Taffinder 1998:88). Axes, for instance, have been found in 14 graves (4 female, 6 male, 2 children, and 2 indeterminate). Janzon (1974:53) found hollow-edged, stone axes exclusively in male graves at other Gotlandic sites, a trend that is not sustained in Ajvide, whereas such an axe was found in burial 34 in association with a female individual. Battleaxes found in some graves seem unrelated to age and sex, an instance that perhaps makes them less likely to be status items, that is, for the *buried individual* (Burenhult 1997b:28; but see Malmer 1975:73). Janzon (1974:104f) reports of no finds of ceramics in children's graves in her survey of Middle Neolithic graves

on Gotland. Janzon proposed that they were mainly 'utility articles', suggesting that the smaller pots were used as lamps. Nonetheless, some children were accompanied by other kinds of utility articles, like fish-hooks and arrow-heads, that they seem to have been too young to have ever been able to use (for instance the infant in grave 10). Burenhult takes such instances as evidence of inherited prestige (e.g., Burenhult 1997d:62). This is, however, to jump to conclusions and to ignore various, possible, symbolic and social aspects of materialities (cf. Strassburg 2000:200). It may very well be that 'unrelated' individuals placed 'valuable' grave interments like axes etc. in order to gain respect from the living. That would suggest that a burial was rather an 'opportunity' for all than a matter for the next of kin etc. Grave interments may, of course, also represent the communal deposition of a series of individuals.

A general feature of this kind of Neolithic graves is the relatively high frequency of imported goods (Malmer 1975:108). Possible imports on Gotland are amber, dentalium shells and beaver teeth (Janzon 1974:46; Malmer 2002:83). Ortivican flint is local, while cretaceous flint is imported (Taffinder 1998:86). In addition, one pot from the Ire site has been found to be of a type of clay not known to exist on Gotland (Taffinder 1998:87). Amber, dentalium shells and battle-axes are assumed to come from Denmark or southern Skåne, whilst bear and beaver teeth, slate and clay figurines are believed to have northern origins (Österholm 1989:187). Lindquist & Possnert (1997a:73) have also suggested that the tubular pearls of fowl bone found in grave 2 at Ajvide are possibly imitations of the dentalium shells and are thus pseudo-exotica (which may also apply to the so-called 'flutes'). Taffinder has analysed 104 of Gotland's Middle Neolithic graves in terms of exotic (non-local) interments. Her general result suggests that exotica are mainly found in adult burials and that more men than women are buried with exotic interments (Taffinder 1998:88). The exotic elements in the burials do not necessarily indicate status differences but may be employed to separate series of individuals who were involved in more far-ranging, serial routes than others. This instance may prove to be interesting in relation to other properties of the graves. The exotica may, for instance, turn out to constitute a chronological trait.

The main source of chronology is normally ceramic design. But the so-called GRK ceramics are not very suitable for traditional typology of shape and décor, because they are normally quite plain with few significant traits. The place and situation of the pots in the graves may, however, be significant. The practice of putting some of the pots upside down in the graves is perhaps a result of unintended consequences or of formation processes but is yet common enough to imply a deliberate practice. This may have been done to

cover or protect something or, as Malmer (1975:42) suggests, to display the ornamentation. According to Österholm, ceramic decor generally changed during the last phase - from being modest to 'vulgar', covering the whole pot (Österholm 2002a:22; cf. Österholm 1989:99, 121). Is this a hint that burials with pots placed upside down are later than the others?

To sum up: The question of whether the dead actually owned or used the artefacts (or similar) that accompanied them in their graves is not really the most interesting one. It may perhaps be possible to search for traces of particular wear on the bones which may be deduced to typical working positions and thus put down to the use of certain artefacts, but that is rather far-fetched enterprise (cf. Janzon 1974:126). Discussions of individual status are also problematic. Burenhult (1997d:61) suggests that it is possible that the individuals of highest rank were buried with few or no interments, because they were inherited, while the individuals of low rank were buried with all their less valuable personal items. Nonetheless, he believes that it is irreconcilable with general human behaviour to put fish-hooks in the grave of a woman who had never fished during her life (1997e:177). But, as I pointed out in the general part of this chapter, there are many ways in which objects may find their way into a grave: apart from the manifestation of the status/role/identity of the dead or the 'buriers', we may consider the possibilities of a two fold system, one for the living and one for the dead, or ideological 'empty gestures' signifying equality. In either case, it seems that the traditional categories of sex and age do not indicate any clear differentiation at Ajvide. Does this really mean that the series who buried their dead at Ajvide were egalitarian and equivalent to 'happy families'? Perhaps a corporeal analysis of subjection and subjectivation may provide another picture. Such an alternative approach discusses *associations* between corporeal and material attributes, in order to expose subjectivated categories and possible social relations. We may also study social change by tracing differences in such associations. Associations may go both ways. We are often inclined to assume that the interments are associated with the buried individual, but we might find that depositions and hoards are 'completed' by the remains of a human. The cenotaphs at Ajvide may be interpreted in this way. The approach makes it easier to deal with the double burials as well.

The material evidence of the Ajvide site thus seems to be interesting in many respects for microarchaeological analysis. The variability is, however, too complex to master without help of additional operations. A primary goal is to make the relative chronology better in order to make social analysis less general (shorter time-

spans) and thus constitute a platform for discussing social change (on the local level).

Analytical strategies and operations

To make progress in the analysis of the complex Ajvide data we need to employ a series of operational techniques that make use of as much of the potential social information as possible. The primary goals are:

(a). to establish a finer chronology and divide the graves into a number of temporal series.
(b). to find and discuss associations within the series and discuss changes in structurating practice and subjectivation.

To achieve these goals, the data need to be quantified in some manner. The questions regarding the quantification of archaeological data were widely debated during the 1970s and the discussions yielded a variety of approaches and ways of looking at cultural data. This is not the place for extended discussions regarding such matters (but see Cornell & Fahlander 2002a:104-9); it must suffice to mention the most promising approach for this case: element analysis. Element analyses do not need to be complex procedures. The main issue is to avoid, as far as possible, the classification of artefacts according to their assumed functions and relations to other materialities. Instead, their material properties are registered, as well as several hypothetical classifications. Element analysis thus suits the microarchaeological approach well. As we pass several 'nodes' of changing symbolic orders, we cannot know for a fact what to expect. The result needs to be analysed in some way to make sense of the patterns and relations found and will, of course, still be based on contemporary fictions and uses of appropriate data. It will, however, probably be less predictable and biased, as the 'killing' of the objects will be done at a later stage of the analysis. The procedure is similar to hypothesis-testing, but it may also be used for more or less random analysis in the search for 'unthinkable' or unexpected patterns. Of course, such possible relations still need to be analysed and validated according to traditional praxis.

An interesting study that makes use of such an 'intuitive approach' is Mary Whelan's discussion on the Black Dog burial site in Dakota, USA (1995). Whelan's main point of departure is to analyse sex and gender separately. The study follows more or less common sense praxis in assuming that the variability of the burial site will 'reveal 19th-century Dakota gender values'. That is, that the arrangements and grave goods will in some manner vary according to

the buried individual's social persona. However, Whelan employs a somewhat backward method: she divides the artefacts into 'likely gender categories', before comparing her series with the anthropological sex estimations. In this manner, she ends up with more, possible, gender categories than by the 'normal' way of procedure.

I may briefly mention some other aspects of registration and classification. The data of Ajvide have already been classified and thus 'killed' to varying extents, but the analysis will seek to expand this information when appropriate. For instance, axes are analysed in several ways: as a tool-category, as well as according to type and material (e.g., ground-stone/flint and local/exotic etc.). In a similar sense, pierced seal teeth are registered as pierced teeth, as well as according to the kind of animal etc. Table 2 shows the principal elements involved. Unfortunately, extensive corporeal associations are not possible, as the osteological information is mainly restricted to sex, age, stature and occasionally extensive wear.

Type:	**Interments:**	**Osteology:**	**Other:**
Grave no.	Harpoon	No of ind.	C14/C13 dates
Orientation	Fish-hook	Body position	Fishing gear
Cenotaphs	Axe	Stature	Hunting gear
Dimensions:	Arrow-head	Age	Exotica
(L/W)	Bone-point	Sex	Clothing
Shape	'Flute'	Wear	Ornaments
Double graves	Amber bead	Extra	Tools
	Fowl beads		Pseudo-exotica
	Flint tools		
	Other stone tools		
	Pottery		
	Pierced teeth		
	Teeth		
	Animal bone		
	Birch bark		
	Ochre/coloured soil		

Table 2. Principal elements of analysis (data from Burenhult 1997d; 2002).

The quantification of the elements is conducted in a soft, 'Spauldingian' manner rather than with a strict, 'Malmerian' approach. This means that continuous values are generally transformed into nominal; the actual statures of the individuals are lumped together in height-groups, and, when appropriate, artefacts are divided simply into 'small' and 'large' or 'short' and 'long' (cf. Spaulding 1977). Some of these categories are trickier than others. Age-groups are almost impossible to construct in a relevant manner. To simply em-

ploy groups of 0-10, 11-20, etc. is, of course, a meaningless division. To employ terminology of physical anthropological (foetus, juvenile, adult etc) is possibly better but may still not be relevant to any given case, as corporeal subjectivation is socially constituted. Perhaps corporeal thresholds are a better alternative? By this, I do not refer only to, for instance, maturity and menopause, but also to the average age of first social speech, ability to walk, to manage without the help of primary others, a certain level of fine motor ability or level of knowledge (tacit and social) and so forth, until the decline towards death. In this case, I shall settle for a tripartite division between young, middle-aged and old, based approximately on corporeal abilities and traits, but also according the nature of the given data.In the study a number of 'constructed' categories are also analysed, for instance, exotica (imports), fishing or hunting gear, possible clothing, ornaments, etc. The result is analysed from a dot-matrix, in which some awkward cases can be present in more than one column. The resulting, amorphous series are thus defined polythetically. In addition, particular information and 'peculiarities' in osteological data, artifacts, etc. are given in a special section.

General tendencies and patterns

A quick review of a number of single variables implies the possibility of discerning a few horizontal agglomerates. As may be suspected from a general survey of the data, a good number of single aspects are more or less evenly spread among the graves. Most clusters and patterns are found in relating categories of elements to single or compound aspects.

Regarding the *demographic* and *corporeal variables,* some interesting patterns emerge, while others seem insignificant. The stature (height at death) of the buried individuals shows no general significance, besides the absence of the tallest series (3) in the northern part of the site. The different age-series show only faint patterns: the majority of the oldest individuals (>60 years) are found in the northern part with one exception (grave 53). The younger series (1) are mainly found in the southern part of the area, and the semi-adults (2) are mainly found in the mid-southern part. The graves with more than one individual are found only in the middle part of the area. As regards the importance of biological sex, there are no distinct patterns; they seem randomly distributed between the different series of artefact and construction details. Individuals with traces of wear and muscular stress are somewhat related to age, but also to lifestyle in general (i.e. executed practice); it may be worth noting that there are no such individuals east of the black surface.

The varying *grave types* also show some patterns, such as the different lengths of the grave pits: the longest (>2.5 m) are generally found in the northern part of the area that was first excavated. Perhaps the length of the pits may have something to do with different excavation technique and stratigraphic interpretation of these graves, but the practice is nonetheless interesting. Somewhat surprisingly, there is virtually no distinct variable between the general N-S and E-W series regarding numbers, type or assemblages. It is also interesting that all the cenotaphs have N-S orientation and are found only in the northern part of the area. Concerning the orientation of the graves, there are no correlations between their general directions. There is a horizontal difference between the northern and southern parts of the area D upper, in which the southern has a greater ratio of E-S aligned graves (16 vs. 5; 10 vs. 22 N-S). It may also be worth noting that all clusters of the corresponding alignment are situated in the middle and southern parts of the area (with the exception of the cluster of graves 2, 7, 13 and 14).

Some of the *grave interments* reveal some clustering and patterns, while others are more evenly spread. *Bone points, flint items, ceramics* and *pig bone* are frequent and are found in graves throughout the site. There do not seem to be any spatial patterns of various species (e.g., seal, hedgehog, fish, elk), but three of the four graves (2, 18, 21 and 62) that contain remains of hedgehog are found in the north. Grave 62 is distinct from the others spatially, as it is the only grave without pig bones but containing remains of hedgehog and bones of elk. Seal teeth are found in c. 30% of the graves, but not in the mid-part or east of the black surface, a pattern that is even clearer when only the pierced ones are concerned. There are no pierced teeth in any of the graves with more than one individual. The finds of *amber* correspond to the finds of pierced teeth, a pattern that is also found in the distribution of the category of *'exotica'* (axes of cretaceous flint, amber, etc.) The category of pseudo-exotica ('flutes' and fowl bone, etc.), however, are more evenly spread. Taffinder's analysis of other Gotlandic, Middle Neolithic graves suggests that exotica are mainly found in adult burials and that more men than women were buried with exotic interments (Taffinder 1998:88). This conclusion is not sustained in the Ajvide material. It is also interesting to note that finds of amber and dentalium shell are found only in the older graves of Västerbjärs (Malmer 2002:94). However, there is no similar pattern at Ajvide.

The category of *'Stone tools'* (other than flint items, e.g., wheat stones, pounding stones, etc.) is found mainly in the northern part. There is also a tendency for *'fishing gear'* (fish-hooks, harpoons, birch rolls) to be found mainly in graves in the northern part of the area. It is interesting to note that this category seems to be juxta-

posed to the category of *'hunting gear'* (bone and flint arrow points). Graves that include elements of the category of 'hunting gear' are, however, quite few in number. The *Birch rolls* are found only in graves in the northern part of the area, perhaps a result due to varying conditions of preservation? Also occurrences of *ochre* and yellow or red soil are mainly found in the graves of the northernmost part.

It is increasingly clear that the graves of the northern part of Ajvide D upper are different in many respects from the others. Most obvious is the greater number of *different* artefact types. These patterns are interesting as such, but in order to discuss social change and the development of the burial ground, they need to be related chronologically. We need, however, to be more precise to make sense of the contradictory evidence of C14 and stratigraphy. A possible way of achieving this is to recognize a number of *hypothetical series* of two or more graves that seem to be related as a chain of events on the episodic level or were constructed by similar, structurating practices. Here, I mainly focus on the varying practices behind the construction of the graves and their relation in space.

Serial practices at Ajvide: Tracing series of events

As there are very few, if any, generally distinctive, chronological traits at Ajvide, we need to follow another path in order to make sense of the apparent entropy. By focusing on the *practice* of making the graves, we may establish a number of series based upon the social practices of construction that are hypothetically related in time. Nine series call for attention (**A** to **H**). These are attended to in order and those that seem solid will be related to each other, in order to discuss the social change and development of the burial field. The first series (**A**) encompasses graves nos. 6, 19 and 30. These are all of the latest series of radiocarbon dates which may distinguish them from the older ones.

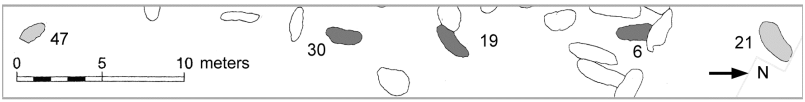

Fig. 7. Series A: The graves of latest radiocarbon date, aligned in N-S direction?

What distinguishes these three graves, besides their late radiocarbon dates, is that they possibly form a line oriented N-S and share the general orientation. It is interesting here that grave 19 has the only radiocarbon dating that is clearly later than the others and that

the grave include a faceted grindstone of STR-type (Malmer 2002:96). Taking these aspects as a starting-point, we may also add graves nos. 21 and 47 at the northern and southern ends of the same line respectively. Looking at other data, we find that the 'core' of the series shares other elements: all buried individuals are males, buried in long pits, which contain neither ornaments nor exotica. Of the two candidates, grave 21 is quite different in many instances, not only because it is a cenotaph, but also in the types and categories of grave interments. The other candidate, grave 47, is different, insofar as it is a small grave of a child but does not raise any other objections to being included in the series.

A second series (**B**) is those graves that form a circular bow or even a circle around the empty area in the northern part (1, 3, 17 and 18, and possibly 12, 19, 22, 23 and 29 or 4, 16, 9, 10 and 15). Here, we find that grave no. 1 is in many ways different from the others, which also applies to grave 16 in respect of interments and spatial position. Grave no. 29 is a triple grave and, like 16, is also spatially separated from the others. Grave 12 is the only cenotaph. The remaining graves, however, share a number of features: the pits are aligned N-S (except no. 23), and they are 'poor' in that they contain unusually few interments. Grave 19 of series A seems to be a later 'intruder' to be included. This series is perhaps the fuzziest, with many possible candidates and alternative combinations. This assemblage, however, leaves us with the graves 3, 4, 18 and 29 as constituting series B.

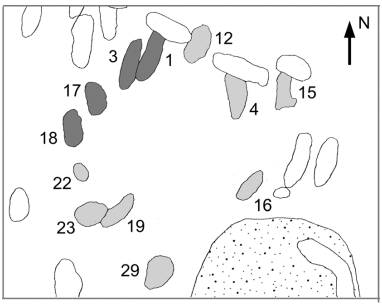

Fig. 8. Series B: the graves around the empty area NW of the black surface.

Series **C** consists of graves nos. 2, 7, 13 and 14. They are all quite alike, except for no. 14, which differs in more respects than being 'out of line'. All have the same E-W orientation, most are burials of old individuals of male sex (except for no. 2), the pits are of extended length, and all, except 14, contain fish-hooks (but no harpoons), pierced seal teeth, and various animal bones. It is nonetheless an interesting line of graves that possibly constitutes a chain of events of short duration, especially as regards their exactly corresponding

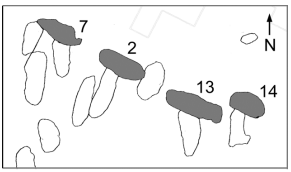

Fig. 9. Series C: A line of aligned graves, that may contitute a single event.

orientation. It is worth noting that grave no. 2 contains an amber bead in the shape of a 'double-axe', which suggest that grave 2 (and perhaps series C) is actually one of the oldest graves at Ajvide (cf. Malmer 2002:83).

The row of graves nos. 1, 3, 4, 8, 15 and 24 constitutes series **D**. They are all of extended length, aligned N-S and share similar kinds of interments. Grave no. 1, however, once again stands out from the others in several ways: it is the only one that contains animal bone, flint, pierced/unpierced teeth, exotica (axe, amber) and thus ornaments. Also no. 24 is slightly different as regards closeness in space, with the head oriented in the southern direction and the presence of fowl bone and muscular wear. Here, it is also interesting that two of the graves (8 and 15) are cenotaphs.

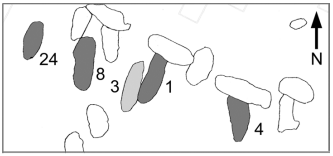

Fig. 10. Series D: a possible row of graves.

There is also a series (**E**) of graves separated from the rest, as they are situated west of the 'black surface' (25-8, 34, 36 and 37). These graves are fairly consistent but form pairs rather than a series. One example is nos. 28 and 36 which are both hocker burials in round pits. It is interesting to note that no. 36 actually differs most from all the rest; it is the only one containing fishing gear, as well as 3-4 times more interments than the others (which are unusually low in numbers). The individual in grave 36 is also the only woman. The two graves do not share the same series of radiocarbon date (series 1 and 2 respectively) but may still be contemporary and thus hint at an older date.

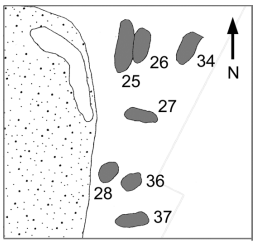

Fig. 11. Series E: the graves east of the 'black surface'

In addition to the general spatial series, we can also pursue others constituted by distinct social practices. For instance, the series **F**, which consists of the cenotaphs (8, 9, 11, 12, 15, 21, 32 and 33). The last two differ in the respect that they are separated from the others in space and nos. 11, 12 and 15 are aligned in a row. It is interesting to note that the cenotaphs do not contain any pierced teeth or 'flutes'; only one of them has amber and fowl bone (no. 21). This may sustain the assumption that these elements are ornaments or clothing, that is, items belonging to a body and thus not deposited in a similar sense as the other interments. The grave with fowl bone and amber is also, together with nos. 32 and 33, separated spatially from the others. Cenotaphs 32 and 33 also stand out, as they do not

contain any material at all (neither bones nor interments), which makes them questionable as 'real' cenotaphs. Also no. 9 stands out from the other cenotaphs in a similar, but not so extreme way as no. 21. There is no animal bone in the cenotaphs, which may suggest that unworked animal bones are generally remains of food offerings. The practice of making 'empty' graves seems to be a chronological trait in general in which the row of graves nos. 11, 12 and 15 may constitute a sub-series.

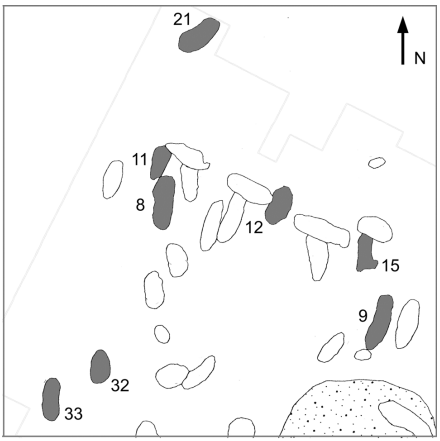

Fig. 12. Series F: the cenotaps.

Another serial practice that diverges from the 'normal' practice is in series **G**; the graves with more than one individual (nos. 23, 29, 30, 38, 42 and 52). These are both consistent and varying in relation to each other. The most conspicuous trait that they have in common is that they cluster in the middle of the area due west of the black surface. Grave no. 52 has the highest degree of differentiation from the others and is the only one that does not contain any adult. It is interesting to note that double burials 30 and 42 contain no interments at all but share the same orientation. The radiocarbon dates of graves 29 and 30 adhere to different series (1 and 3 respectively).

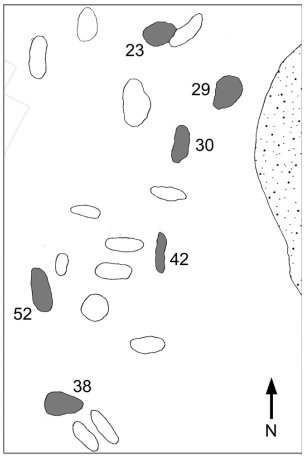

Fig. 13. Series G: the graves with more than one individual.

The extraordinary long grave pits (1-9, 11-13, 15, 21, 25, 31 and 33), mainly found in the northern area, constitute series **H**. Two graves over 2.5 m long are excluded (42 and 52), as they are double burials. The extended length of the pits is a quite distinct practice that separates them as a series. It is an open question why they were made twice as long as necessary; perhaps these individuals were buried in some sort of boat or, as Malmer suggests, a wooden coffin

or a 'symbolic' boat (1975:49; 2002:92). They are concen-trated in the northern part of the field with only a few 'outliers' in the middle part. There are no other individual attributes than extreme length that distinguish these graves from others. Three graves belong to different radiocarbon dated series (series 1: 2 and 13, series 2: 1, and series 3: 6), which is a confusing circumstance that does not fit well with the stratigraphic evidence. These graves surely constitute a series of their own, although the extreme length may not necessarily prove to be a straight, chronological parameter.

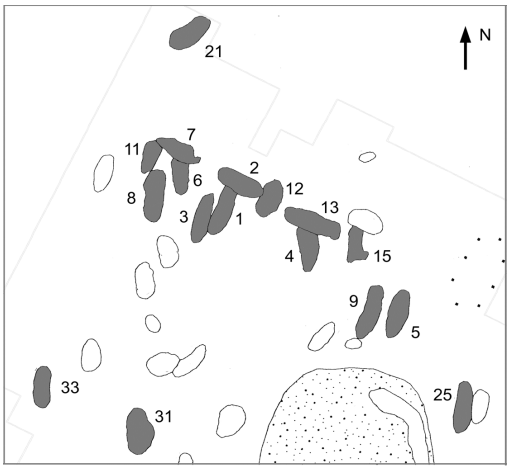

Fig. 14. Series H: The extraordinary long grave pits.

The analysed series (A to H) give a confusing result. Indeed, identification of patterns is a means, not an end. The series cannot easily be put into a relative, chronological frame, but there are a number of indications of several distinct phases of use. The radiocarbon datings make little sense and cannot directly be used to sequence individual graves or series of graves. The only instances for which we have clear dates are graves 2 and 29, which are radiocarbon-dated from three independently sources each. It is also quite safe to assume that grave no. 19 is one of the later graves, both according to artefacts (the grindstone) and the radiocarbon dating. At first glance, it seems that the northern part of Ajvide is older than the southern part (divided roughly in the middle). A similar, horizontal division between an older (northern) and a later (southern) phase of use is, as mentioned, found at the Västerbjärs site (Malmer 2002:94f). Indeed, the northern part of the Ajvide area is special in many ways: the buried individuals are generally of old age, but include none of the tallest individuals; it encompasses all cenotaphs, the majority of non-flint, stone tools, birch rolls and ochre and generally the extreme lengths of the pits. It has much greater NAT values (Number of Artefact Types) in relation to the average. It is interesting to note that the lion's share of the E-W oriented burials have been found in the southern part (ratio of 16:5). However, if we are concerned with possible chronological traits, such as the series

of C14 datings, finds of amber, stratigraphy, etc., we may end up with a rough, tripartite division of the Ajvide site. The southernmost series of graves seem as a general rule to be more or less contemporary with the northern series, leaving the middle part as the latest. In short, the northern graves are the oldest, then graves were dug further south and finally the middle series was constructed – *which also span over the older northern and southern parts.* Indeed, the disturbed graves are found in the north and the south. The middle area contains all double and triple burials but no cenotaphs or extended pits. It is tempting to suggest that series C actually belongs to the 'southern' phase; that is, after four attempts which all intercepted with older graves, the 'undertakers' continued in the southern part.[24] Of course, such a tripartite, chronological division is very crude, with many exceptions, but yet seems valid in a general sense. It is not obvious how the area east of the 'black surface' may fit onto the general scheme; it is separated spatially from the rest and marked by the absence of children (0-10 years of age), as well as any individuals showing osteological traces of wear. The C-14 datings of these graves span from the later series to the older. It is not very likely that this series constitutes a temporal phase on its own.

It thus seems likely that the burial ground developed through a series of more or less independent events of constructions, in which the participants had varying knowledge of the extent of the past uses. Such general structure and development will surely encompass many anomalies and exceptions. This is what we must expect if the graves were constructed in several phases and perhaps by individuals with little or no knowledge of each other. Some burials are possibly solitary ones, constructed when individual deaths occurred. They must not be separated by any long time from the others, but enough to make the older ones invisible. I shall deal with a few of them here before I sum up.

Peculiarities: The tooth, the whole tooth and nothing but the tooth

Statistical and comparative analyses have many advantages in finding patterns in complex datasets, yet tend to fail to identify the unique and cannot automatically discern elements of *social* importance. There is thus a need for a brief elucidation of some of the peculiarities at Ajvide and for a discussion of how they may relate to the general patterns. There is a general problem of how to relate the unique and 'odd' to the general, but the fragmentation of the archaeological record also brings about an uncertainty as to how frequently a given practice really was executed. Solitary acts are normally not structurating, but they are nevert performed in a social

vacuum; they are always on some level, subversive or not, related to some elements of active fibres and threads. Archaeologists are generally inclined to emphasise unique objects if they contain rich finds, valuable metals or material that is normally not preserved. This is not my point here. I want to cover the odd and the outliers of Ajvide in order to complement the general analysis. Some peculiarities can perhaps be 'explained', but there are still some practices at Ajvide that, to use Lacanian terminology, resist symbolisation. This is not the place to account for every facet of the Ajvide burials, nor is there much room for individual photographs or drawings (but see Burenhult 2002), but I would like to discuss a few examples in some detail.

One example concerns graves 16 and 23, which indicate signs of reburial. Grave no. 16 contains fragmented and dislocated parts of a skeleton. Österholm (1989:183) suggests that the individual, a woman, seemed to have been 'folded' into the grave and that the bones were black in colour (possibly from train-oil). It is not clear whether the state of the grave is due to later, natural or cultural processes. Missing parts may have perished or been removed. The other grave, no. 23, is more interesting. It is recorded as a double burial of a man, 50-60 years old, and a young boy of 12-13 years of age. The extremity bones of the adult are missing and show signs of having been decarnated. The adult is laid out across the chest of the young boy and the femur bones have been rearranged to fit into the pit. It thus seems like a two-event burial. Perhaps an older grave of the adult was accidentally or intentionally intersected while making room for the young boy and the older bones were relocated to fit into the new grave? Another possible scenario is the opposite; the grave of the young boy may have been intercepted when burying the adult. Perhaps the undertakers did not for some reason want to disturb the older grave but rather made efforts to make the adult fit into the same pit? The latter hypothesis is perhaps far-fetched but, if correct, it can tell us something about the importance of orientation. The bodies are not placed along the same axis, but at an angle of 45°, despite the outline of the pit.

Graves nos. 16 and 23 have otherwise little in common; they do not share the same sex of the corpse, interments or orientation, but are relatively close together (c. 8 m apart) in the northern part of area D upper (Fig. 4). It rather seems as if they belong to the same category of accidentally disturbed graves as the other intersected graves rather than intentional, reburial and post-burial activities. There is, however, one example of a clearly manipulated burial. Grave no. 6 contains the remains of a young man of 18-20 years of age with the upper vertebrae and cranium missing. This would seem to be due to the fact that the upper part of the grave is inter-

sected by another (no. 7) in a vertical direction. The strange aspect of this grave is that the teeth of the young man have been laid out as they would have been if the cranium had not been removed. Even stranger is the fact that two canine teeth have been replaced by two animal bones (pig and seal). There are no indications of capitation and the teeth seem to have been rather carelessly removed, which suggests that the missing parts were removed post mortem. This is a disturbing and unintelligible kind of practice that is hard to relate to. It is very possible that grave 6 was accidentally disturbed while digging grave no 7 and that the skull was removed. This is the case of, for instance, grave no. 4. But to take the time to remove the teeth and put them back in place with replacements for two missing or kept teeth is a strange act, about which we can only speculate.

Fig. 15. Vertical photograph of grave 6. Detail of the cranial area with extracted teeth *in situ*. Photograph: Göran Burenhult (modified from Burenhult 2002: colour plate 22d of the CD-Rom).

There is nothing else that distinguishes this particular grave from others. We have no clues as to whether this act was unintended and improvised by one individual or something that was known and sanctioned by others. What we do know is that teeth in general were collected and utilised in many ways during the whole period of use. It is strange for us to imagine that the skull of a dead human could be removed and used for unknown purposes but that the teeth needed to be put back in the grave. This instance, along with the other patterns found in the presence of animal teeth and jaws in some of the graves, may indicate that teeth in general were loaded with social significance. If we follow this idea to the extreme, we may suggest that seals and other animals were mainly hunted because of their teeth rather than for food.

Burials nos. 28 and 36 are also distinguished from the rest as the bodies have been placed in the hocker position rather than stretched out (grave 51 is semi-hocker). The two graves are placed next to each other, due east of the black surface. No. 28 included a boy, 18-19 years of age, placed with his head towards the south, The individual buried in grave 36 was a woman of 20-25 years of age with her head towards the west. Grave 28 was nearly empty of artefacts, while no. 36 included fishing gear, flint tools, a hedgehog jaw, etc. The common location, form, size and burial position of these two graves suggest that they are roughly of contemporary date. The radiocarbon dates, however, suggest otherwise; the human in grave 36 is several hundreds of years older than the individual in grave 28. If, despite differing radiocarbon dates,[25] they are contemporary, this may tell us something about the possible importance of orientation of the head. The orientation towards the north and the west may be coincidental but may also have been intended according to corporeal associations (e.g., sex or age).

There are two individual graves that have been especially highlighted by the excavators. One is no. 60, which contains a child accompanied by no less than 32 pig jaws more or less randomly distributed at the foot end. The other example is grave no. 62, which contains an individual, 25-30 years of age. The sex is ambiguous, the individual is taller than the average woman and has a robust cranium, but the pubic bone indicates that it is a woman's grave (Molnar 2002:373). The special feature of this grave is the large amount of small, hollow bones found within it. The artefacts are interpreted as flutes but may just as well be imitations of dentalium shells, which suggests that these are dress ornaments (cf. Lindquist & Possnert 1997a:73). In general, this grave is unusual in respect of the grave interments which include a bone comb and mother-of-pearl ornaments not found in any other grave.

Indeed, there are some strange combinations of finds at Ajvide. Some may be discarded by alternative interpretations whilst others are still difficult to grasp. Are these subaltern practices, things that have little to do with the 'normal', structurating practices of the different series at Ajvide? Perhaps they are simply anomalies, acts of single (un)related individuals. Perhaps the 'strangest' practices were performed when the thread of structurating positivities was weak. Clifford Geertz has expressed confusion on this matter; he wonders 'What are we to make of cultural practices that seem to us odd and illogical? How odd are they? How illogical? In what precisely does reason lie?' (Geertz 2000:104). As Geertz stresses, we cannot satisfactorily distinguish 'strange' from 'normal' practices. Archaeology also faces the question of representativity, which also imposes problems in distinguishing singular from repetitive acts. There is no solution to this problems, somethings are perhaps better left uninterpreted(?).

Summary: Life and death at Ajvide

The graves of Ajvide present an interesting and thought-provoking set of data, which is by no means homogeneous or simple to analyse. The published documentation of the Ajvide data is thorough, although it contains some contradictions and question marks, for instance, regarding the sex of the individual in grave 59 (Burenhult 2002:42; Molnar 2002:373). There are always problems in analysing archaeological evidence without first-hand knowledge of the excavation process; some aspects have to be taken at face value, while others are more questionable. The documentation is partly detailed, while other information is general or non-existent. The problems of the kind of 'armchair archaeology' that I have conducted here lie, however, not only in the quality and quantity of data, but also in the chains of associations that have already been made by the authors. It is not simply a question of a double hermeneutic in the Giddensian sense, nor of any Foucauldian archaeology of documents and statements. Rather, the process may be described as an archaeology of hermeneutics. It means digging through the layers of interpretations, statements and chains of associations in order to reach a point where the nodes of the deductive chain can be identified and in which a lower stratum can be found from which we can pursue new chains of associations. We cannot always take the arguments of the authors/excavators at face value, as the information they provide follows deductive chains whose premises may be incompatible with microarchaeology. It is problematic, as the provided information is also 'filtered' through the logic of the argument. In other words: it is very easy to be led through the texts and to end up with or to high-

light the same elements. The 'archaeological' component in this process thus lies in identifying the 'nodes' in the chain of arguments, in order to allow other reasonable lines of thought.

But how about life and death at Ajvide? Seen from a traditional point of view, that is, as archaeological evidence from a culture group (i.e. Pitted Ware Culture) or a small, local community of such an entity, the burials give a picture of a peaceful, hunting-fishing community with little evidence of social stratification or inequalities (cf. Burenhult 2002:34). Indeed, the osteological data show no signs of violence and there are quite large numbers of old individuals. It was not necessarily an easy life for all, though, a number of skeletons show signs of extensive wear from hard labour. Does the microarchaeological analysis differ in any way from the culture-historical one? Indeed, it does. Although the analysis is not complete, employing all possible analytical strategies and operations and only considering a sample of all the excavated graves, we have managed to establish a number of points. The graves of Ajvide are unlikely to represent a communal burial ground in the normal sense. One rather gets the impression that new graves were dug during different periods of occupation or at random visits. The series of perfectly aligned graves hint that they are reasonably contemporary (i.e. little solar variation), while other, larger series of graves with similar traits, but varying alignments, may cover a longer period of a generation or two. The spatial division found at Västerbjärs may hint at a possibly similar division at Ajvide (although not homogeneous). However, by making the proposed, general, spatial distinctions, we can begin to discuss changes in structurating practices regarding the disposal of the dead.

It seems likely that the cenotaphs actually constitute a series in a temporal sense, although not all of them are likely to represent an event on the situational level. This may tell us something about the attitudes towards corporeality and death. The dead body seems never to have been an abject; in a similar vein the 'Real' of the body (blood, flesh and bones, etc.) is not likely to have been troublesome or traumatic. The series C (nos. 2, 7, 13 and 14) is interesting here, as the graves intersect and partly destroy only slightly older graves. This series is clearly later (but not necessarily much later) than series B of the cenotaphs. The 'undertakers' must have realised that they had come across remains of humans whose bones were either taken care of in some way or trashed elsewhere. The first three graves are aligned, but the last, no. 14, is not. This can be interpreted as meaning that, from experience, they tried to avoid destroying another grave. The irony is that they did so anyhow, which may have been a reason for not continuing the original alignment and thus to start burying the dead elsewhere. This could

also imply a sequence of events starting with the digging of grave 4 and ending with grave 14. The general numbers of artefacts may support this hypothesis, as they decline along the suggested sequence of events. It is indeed a small sample, but it is worth noting that none of the graves of series C are cenotaphs. This may indicate a change towards corporealities or beliefs about death and the afterlife, but also a shift in social practice. The graves of series C do contain fish-hooks but no harpoons, an instance that may slightly sustain the drowned-at-sea hypothesis of the cenotaphs (of which two contain harpoons). The graves of series C are, however, linked to series H, as three of the four graves are of extended length. Although the practice of extended graves is distinct from others with regard to energy expenditure, that particular, structurating practice may be of longer duration and spatial extent (cf. similar graves at other sites on Gotland, e.g., Janzon 1974). It may be worth noting that the extended graves in general contain fishing gear that may be an argument for the buried-in-boat hypothesis. It is also interesting to note that the graves of series C are also later than the 'line' of series A and the 'row' of series D.

The empty space in the centre of series B seems special with regard to the lack of burials and to the density of bones in the cultural layer. It ought to have been an area with some kind of visible marker above ground, perhaps a three of some significance? The series of graves aligned around this area does not, somewhat surprisingly, show any convincing evidence of being related or even contemporary. What we can assume at this stage is a possible, temporal gap between the graves of the middle part and a northern and a southern series of graves.

The double and triple burials constitute a semi-independent series (G) located in the middle part of the area and possibly partly of a later date. We can only speculate about the reason for this pattern. But they differ from the extended pits and thus the cenotaphs in respect of labour investment. The graves contain both pairs of adults, adults with children and children with children. This is generally interpreted as mirroring some kind of social relation, such as kinship or even family-like relations (e.g., Malmer 2002:93). The grave interments are few, which may indicate difficult times, a period of back-lash, perhaps epidemics or simply less food, but also unfavourable circumstances for digging. This may be one reason why we find more than one phase of use at Ajvide. Their orientation, stature, age and sex seem random and all but one indeterminate individual share the general back position and normal grave dimensions. No amber and no pierced teeth are found in these graves (as are generally found in graves with old radiocarbon dates), but one includes an imported flint axe and other pseudo-

exotica. What we may face here is a phase of 'rationalization', that is, instead of burying the individual corpses in a line of individual grave pits, they are all placed in one. Such a hypothesis hints at a period of stress or perhaps unfortunate circumstances. It may very well have been the case that these communal graves were dug during winter or in other unfavourable circumstances for pit-digging. Nonetheless, it is worth noting that none of these graves are of greater length than 'necessary' and that the only grave without any adult differs from the rest. There is also a social 'explanation' which may suggest the presence of a series of individuals or a phase with less stress on individuality, but that may be pushing the data too far(?). We may, however, consider a comparison with a hypothetical series of graves 5, 9 and 10. The infant buried in grave 10 received an individual grave in relation to the cenotaph and the female of grave 5. If we assume that these three graves constitute a related chain of events on the episodic level, they are distinct from the practices of double burials in the mid-field.

The proposed series (A to H) are in various ways linked to each other. Some seem to be more or less contemporary, while others belong to separate phases. Somewhat surprisingly, the radiocarbon dates are of little use here, as they do not match the evidence of stratigraphy. There may be variations between individuals in the extent of marine diet that produces mismatching dates. Here we are probably better off emphasising stratigraphy and horizontal parameters. The clearest difference is found between the later series C and series A, D and F (possibly also series B). These series do not need to differ very much in time; perhaps they were constructed within one or two generations or even in the same year. It may very well be that most graves were constructed during a relatively short period of time (as is hinted by the radiocarbon dates of graves 2 and 29 and the stratigraphical evidence). Such a perspective suggests that the differences are more of a social character than of changing practices. It is always problematic to differentiate temporal from social parameters. There is no way that we can be certain in this case without more radiocarbon datings, preferably of bones of terrestrial animals found within the graves and complemented by C13 values for each individual. This instance points to the ambiguity of the data and the importance of detailed analysis. Frustrated archaeologists commonly cry that they need more time, more money, etc. to be able to pursue an interesting line of thought. The lack of resources may be an obstacle in some cases, but it need not be a question of resources. With a careful project design, dedicated to the given circumstances of an object, it may be more a question of priorities. We may perhaps employ the metaphor of fibers and threads here, in the respect that some series of artifacts and

construction elements that have chronological significance are of longer duration than others, sometimes parallel, sometimes exchanged for others or simply cease to be important.

The data of the Ajvide site seemed at first to be very promising for an analysis of serial practice and corporeality, but unfortunately they were not perfect. For instance, the attempt to establish associations of corporeal attributes finds little support at Ajvide. At this point, the material remains of the dead do not allow for a thorough analysis of corporeality. The osteological data are meagre in relation to what can be recorded of better-preserved corpses (hair, eye-colour, tattoos, etc.). This does not suggest that studies of the social significance of corporeal attributes and social subjectivation are uninteresting. On the contrary, is the lack of corporeal series due to the question of what the graves really signify, the living or the dead? The data from Ajvide rather suggest that the grave interments are generally to be associated with the 'buriers' rather than with the buried. From such a perspective, we can imagine a number of individuals of different social series who repeatedly returned to Ajvide and perhaps other places to bury the recent dead. In some instances they brought only one or two corpses, but in other cases a greater number. This scenario is partly sustained by the number of 'pairs' of graves found at Ajvide. Also the cases of dislocated bones in some of the graves follow this line of thought, as they may have been temporarily buried elsewhere and moved to Ajvide at a later stage.

We should thus perhaps rather discuss the social constitution of the buriers that utilised Ajvide than analyse the corporealities of the dead. The variety of artifact assemblies between and within series of graves suggests that the buriers and participants in the burial events were not restricted to a special series of individuals. On the contrary, they seem to have included individuals from a broad series of various, corporeal constitutions, as well as from different, subjectivated categories. The unfortunate implication of this hypothesis is, however, that the extent and changes in subjectivating elements and parameters are difficult to grasp from the burial data. We lack information about the relations between the deposited items and the active subject (or the social series that they may represent).

A question that remains unanswered is why it was so crucial to dig so many graves within such a small area? There ought to be a reason for this. The Ajvide area was, in different periods of time a somewhat special place that initiated certain activities. We have traces of extensive slaughtering of seals etc and perhaps feasting rituals during five to seven generations at the site. For some reason, it was later transformed into a burial ground. We can only speculate about why this particular place was chosen, but it is possible that the islands of Stora and Lilla Karlsö are part of the solution. They

are quite unique and conspicuous features that may, for various reasons, have made the place suitable for burials. In the following chapter, I shall continue to discuss structurating practices in relation to archaeological data with special concern for such possible 'natural' *signifiants*, for instance, the possible potential in discussing the location of graves as nodes of structuration in relation to their local microecologies. We shall, however, move in time and space from Ajvide and the Neolithic to Iron Age Greece and the parish of Arcadia.

4
Making Room for the Dead
Structurating practice and the material environment

The size of the sun is the width of a human foot.
Heraclitus, fragment 3[26]

In the Introduction and Chapter 2, I touched upon the spatial aspects of the social structuration process, that is, the ways in which 'natural' and 'modified' materialities may initiate or prompt structurating practices. Here, we may find inspiration in Whyte's example of how material culture (air-conditioners) can be used to track general, day-to-day movement and find sociomaterial explanations of the spatial distribution of such materialities (see Chapter 2). We may also recall the seemingly irrational route of the old Greek woman in Chapter 1. The sociology of space now makes up an extensive body of literature concerning the spatial aspects of houses and landscapes in many different disciplines, including anthropology, ethnology, geography, architecture and archaeology (for a general introduction, see e.g., Peet 1998). This is not the place for a summary of the literature, but I will present a brief introduction to the general perspectives and terminology. Many theories of social space are not suitable for archaeological analysis; some need actual behavioural studies or living informants to make sense, while others are generally hard to make operative. Space is a relative concept in a social sense, which means that we may expect to find multiple and conflicting uses and ideas of space in any given area. Nonetheless, despite social multivocality, varying abilities and power strategies,

Whyte's example illustrates how some relations between social practice and the material setting can be identified and discussed. However, owing to imprecise and fragmented evidence, most archaeological examples are more or less bound to be tentative in character, which does not necessarily mean that they are not interesting and useful. In this chapter, I shall stress the spatial relations between cultural constructions and their position in a natural environment, with a special focus on parameters that structurate the location of burials.

Materialities, landscapes and social structuration

As in any question on social practice, there are a number of conflicting views on how to approach social space and the structuration of landscapes. The processual, settlement archaeology of the 1960s and 1970s advocated a culture-ecological approach and focused on formation processes (deforestation, coastal variations and cultural transformations of the environment), as well as on the utilisation of regional resources. By stressing the concepts of evolution and adaptation, processual archaeology placed much weight on environmental conditions as formative factors for the ways in which cultures developed (e.g., Binford 1982). The environment was mainly viewed as one-dimensional space, a provider of the necessities for survival and, to varying extents, constraining human action. The tendency of processual archaeology to stress adaptation as a general, unidirectional law is, needless to say, quite deterministic. Not surprisingly, the functional environmental approach has been criticised by geographers, anthropologists and archaeologists for neglecting the variability and creativity of human agency (e.g. Cosgrove 1984; Giddens 1984, 1985; Urry 1985; Shanks & Tilley 1987a; Rose 1993; Barret 1994; Macnaghten & Urry 1998).

A less deterministic approach to social space is found in the new landscape archaeology, 'the phenomenology of space', inspired by strands of thought from human geography (Tilley 1994:10). In this perspective, social action and the non-human context (the landscape) are seen as an integrated, mutually structurating process (e.g., Bender 1993; Tilley 1994:23; 1999; Saltzman & Svensson 1997; Johnston 1998; Bradley 2000). In contrast to the culture-ecological view, the landscape is perceived as embedded sets of space-time relations: "In learning about the landscape, it acts as a primary medium of socialisation, and from this follows the landscape's importance in the creation and reproduction of power" (Tilley 1996a:161-62). The phenomenological, landscape approach thus bridges social and geographical studies. Humans are not simply situated in landscapes; a more or less mutual relation is assumed, in

which, on the one hand, the landscape constrains and enables some social practices, but, on the other hand, the landscape is modified and rearranged by its inhabitants. For instance, tombs or other monumental constructions may enhance the social significance of certain special places. Richard Bradley has suggested that the building of monuments in Neolithic Europe changed the experience of time and place as an unintended consequence of the changes they imposed on the landscape (Bradley 1993:21).

The phenomenological approach in landscape analysis is not a homogeneous project; it lacks defined methodologies and operational procedures to deal with empirical investigations (Criado & Villoch 2000:190, 212). The focus is in general set on visibilities in the landscape; stressing sight-lines and visibility between monuments and their relation to environmental features, such as rock outcrops and other morphological features. The intricate question is *who* is observing; *how* and *why* are seldom concerned (cf. Criado & Villoch 2000:189). The circumstances and purposes of visibility may be important matters in some instances, but we should not overlook that this may be a specifically western, male way of seeing (Cosgrove 1984). What is more important, landscapes are not only 'visual'; their specific topography and material constitution are probably more important factors in the structuration of social space. Landscape settings often offer cues for types of behaviour (Peet 1998:58). The type of landscape (desert, forested, rocky, etc.) surely initiates different needs and evokes different ways of movement, building and organisation.

Probably the most valuable feature of the otherwise dim aspects of the phenomenological approach is the corporeal perspective on the material world. Tilley emphasizes the *process* of "interpreting the significance of place through the body: the hill in its physical reality, in my memory and in myths, the histories as stories told of it, the way in which I approach it, and from where" (Tilley 1999:180). The traditional search for patterns with the aid of two-dimensional maps or GIS data is generally too crude to identify such aspects, if restricted to recorded cultural features and terrain models (cf. Tilley 1993:56; Richards 1993). To discuss small-scale aspects of movement and activity in a landscape, we need a corporeal perspective to grasp those intricate observations that can help to make sense of the social landscape. The corporeal implications of phenomenological theory entail a general, 'in the flesh' point of view, which does not necessarily imply an emphatic or subject-oriented perspective. Nonetheless, the passive, *experiencing* subjects are much more frequent in landscape studies than active (e.g., working), *social* subjects (cf. Gosden 1994:80, but see Ingold 1993 on taskscape).[27] The emphasis on the symbolic aspects of the landscape would certainly benefit

from including aspects of the hard-fact reality of materialities, human necessity and activity/movement (cf. Peet 1998:60).

Structuration of space: Activities, movement and accessibility

The undoubtedly most discussed features of the landscape in archaeology are habitation sites, production sites and graves, but there are, of course, many other, just as interesting kinds of 'places'.[28] In any populated landscape, there are normally paths and roads, transfer-ways, fords and bridges. Such features may follow topography or brutally neglect it or constitute many alternative connections between different places or features. Movement and accessibility are thus important parameters in the social analysis of space. For instance, the path by which we approach megaliths may be as important as the site of the megalith itself (Tilley 1993:73; 1999:180; Johnston 1998:61). From a corporeal point of view, accessibility is to varying degrees enhanced or constrained by positive and negative 'friction' in the landscape. A steep slope, dense vegetation, etc., can impose *negative friction* (constraint), while the opposite, *positive friction* (enabling), is more closely tied to levels of technology. A boat or canoe may change the time-space ratio of waters (lakes and rivers) etc. Besides such practical aspects, ideological or metaphysical aspects may also be as constraining as a steep slope or marshland. It will suffice to mention areas and places that may not have been utilised or crossed, owing to a variety of mental or social considerations (e.g., ideological, superstitious, aesthetic or cosmological). Examples of these could be any given area (e.g., of waters, hills, marshy areas, etc.) or just places endowed with social significance (cf. Tilley 1999:182f). We should thus expect that the past landscape was used and appropriated in several different ways by different series of individuals who moved about, doing different things on different serial routes.

In addition to activity areas, paths and routes, we should also expect to find non-places and back-areas, etc. (cf. Giddens 1984:122ff; Augé 1995; Löfgren 1997). We can also add places or features loaded with social content, history and memory or ordinary places where something happened to occur (cf. Hornborg 1997; Bradley 2002). So-called *signifiants* of the landscape may take many forms; they may be *boundaries* (mountain steeps, waters, vegetation), *pathways* (passages, valleys, coastlines), and *landmarks* (special 'natural' features, a tree or a rock), etc. (Lynch 1960; cf. Gansum et al 1997; Lekberg 2002:233ff, Heimann 2002:44ff). All these more or less distinct *features, places* or *locales* may be important for the social analysis of space. Such places and features can function as 'nodes' in the structuration of space or as 'stations' on serial routes. We must,

however, expect that many such features will no longer be identifiable; roads, paths and such do not necessarily leave any durable traces.

The structuration of space, that is, ways of utilising space and the organisation of movement and activities in a given locale, is a social process, and, as such, we are likely to trace structurating practices and thus structurating positivities from the material residues of the structuration process. Cultural constructions are not randomly distributed in space; they are in one way or another related to the structurating practices of a microecology. *Nota bene*, I refer to *practices,* not people. Social practices are normally conducted at a definite place and at given moments (although some may extend over long periods of time). Their implications and results, however, are not so restricted in time and space. What we should expect to find when analysing traces of social practice in a landscape setting is thus a variety of structurating practices. Normally, such variations are attributed to different groups, cultures or time periods. The problem with such general perspectives is that the archaeological data seldom fit the observed significant pattern; there are always data that do not fit, diverging from the norm or simply not making sense. Such data may be the result of 'subaltern practices' or simply random traces of, for us, unimaginable practices. From a regional viewpoint, this variation may be interpreted as intra-group segmentation (women, warriors, elites, etc.), social change or perhaps variations in production. Surely, a 'standard' heterogeneity of any group is likely to yield a variety of social practices. As pointed out in Chapter 2, we would benefit from a *serial perspective* in which we can discuss the movement and agency of *series* of individuals and their differing ways of relating to the local, material environment. Regarding more general, structurating practices, we may find other explanations; we may even find that some variability is illusory. If variables that are more specific are to be included, we may find that different cases are more alike than they seem at first sight. We may speak of a matrix of structurated practices being involved; each of them may be combined with another or may exclude others. One example of this kind, which I will explore in this chapter, is the placement of burials in relation to other activity areas and the 'natural' landscape.

Places of the living and of the dead

In the remaining part of this chapter, I shall continue to focus on burials. Not so much as archaeological objects, but more as to their spatial relations to the natural and cultural environment. Graves are interesting, as they may change the significance of a place or may be

placed at significant places. As such points in space, they may be employed to identify structurating 'nodes' of reference in a given landscape. There is surprisingly little elaborate discussion of the question of where to place the dead, especially regarding less monumental, rural burials (Cleary 2000:127). The general models, more or less based on empirical evidence, regarding the placing the dead are often of a common-sense nature. The 'proximity hypothesis' is perhaps the most frequently applied. It seldom requires additional, identifiable relations other than closeness in space. Such a perspective is often combined with assumptions that there are 'natural' or typical places for different activities. Habitation areas, for instance, are often supposed to be situated close to natural resources (fertile soils, areas with plenty of game, etc.) or, when appropriate, situated in areas that are easy to defend. It is simply considered that burials are placed nearby (e.g., Forsell 2001:59) on less attractive areas, like rocky hill-slopes, or visually manifested on hilltops and ridges. Arguing from examples in Boeotia and Attica, Snodgrass has pointed to the tendency for graves to be placed in the interstices between clusters of small, rural sites during the Classical period (1998:39). The fact that they are often situated on arable land leads Snodgrass to suggest that they were located on field boundaries for different reasons. There are, however, a number of problems in arguments about closeness, mainly because of the often fragmentary and incomplete information. For instance, if a site not yet discovered had been known, another conclusion might have been drawn. The temporal depth also presents problems, especially when reliable dates for the features are lacking. Both habitation sites and burial grounds can be of very short duration and hence situated in the same area, or close to each other, without being directly related.

Another popular theme considers the visual aspects of burials. It has been argued that monumental tombs with high visibility were placed at boundaries in order to define territorial borders and to claim authority to land (e.g., Madsen & Jensen 1982:83). However, in a contrary way, it is also possible that burials were sometimes deliberately 'hidden', situated so as not to be seen from the settlement or other activity areas (cf. Tilley 1993:79; Parker Pearson 1999:124; Persson & Sjögren 2001:213). Another theme concerns orientation, that is, the relation of burials to settlements, sanctuaries or other activity areas. For instance, Axel Persson suggested that the Mycenaean burials of Dendra were placed west of the habitation because, if placed in such a direction, their spirits would not "disturb the living on their journey to Hades" (Persson 1942:152-153). Another common example of how burials are related to cultural elements is the deliberate use of older burial grounds for secondary burials, perhaps to relate the present to ancestors of a mythic, distant past (cf. Wells

1990:128; Antonaccio 1994). This structurating practice is best known in relation to monumental and superterranean burials like *tholoi*, chamber tombs and megaliths but may apply equally to subterranean burials accidentally discovered or remembered (e.g., Hägg *et al* 1980:114, Gates 1983:32f). Other examples of common locations of graves are along main roads and routes (Kurtz & Boardman 1971:91-93; Schneider & Cancik 2000:196ff; Cleary 2000:128, 137; Rudebeck 2002). There are certainly a number of simple relations between the placings of the living and the dead, but the material evidence seldom supports generalisations from a regional viewpoint. Many of these examples are found in different areas and times, often in combination with parallel practices. For instance, the idea of Persson and others concerning the orientation of tombs and settlements is a little too general. Indeed, the majority of the Mediterranean Bronze Age tombs seem to have been placed west of the habitation: examples are to be found in the cases of Berbati (Schallin 1996:171) and Athens (Kurtz & Boardman 1971:30, 34-36), but there are nevertheless numerous exceptions (e.g., Branigan 1998:17; Cavanagh & Mee 1998:43, 61). There are few, if any, cases in which there is a perfect match between one or two single variables. We should not forget that individual, situated knowledge, what we see and believe is real, certainly differs from the views of others.

Zubrow (1994) argues that the reasons why some specific locations were chosen for activity depended on a mix of *ideal* and *real* circumstances. Real (material, topographic) factors often interfere with the preferred ideal (cognitive, mental) circumstances for the location of a habitation area or a cemetery. "Settlement and housing location are the result of a series of personal and cultural decisions. The ideal pattern of settlement, in the minds of the natives, may be tempered, adjusted and transformed by topographic reality /.../ The prehistoric landscape is the result of numerous compromises between the ideal and the real (Zubrow 1994: 108). Despite the fact that Zubrow's argument is marked by a simplified duality, he still makes an important point here, very much neglected in settlement archaeology. Instead of 'real' circumstances, we may prefer 'local material context' (microecologies) and as a substitute for the term 'ideal', we may think in terms of bundles of structurating practices. There are certainly many kinds of social aspects (e.g., religious, traditional, economic, functional, etc.) that can be active components in the structuration of space, but they are not alone sufficient to account for all the factors involved in the process. It is thus essential to make the arguments more fine-grained in relation to given, local, material contexts. The notion of 'suitable space' is a complex question, which involves materialities, attitudes to death and dead bodies, social negotiation and power relations with many variations.

The social significance of natural features/materialities

To restrict spatial analysis to orientation and relation to cultural variables is probably seldom sufficient. Besides the obvious factors, such as arable soil etc., there are a number of ecological elements and features of the landscape to consider (Bradley 2000). In the anthropological literature, we find reports of how small-scale groups use certain features of the landscapes as 'mnemonic pegs', i.e. as material references for oral histories (Tilley 1994:202ff; Tilley 1999:182). Structurating practices must not be focused on or around such features; they may simply function as nodes or points of reference in the landscape. Such features may be conspicuous elements, like ridges and smaller hillocks, but also less prominent features, like rock outcrops, caves, isolated trees or groves, marshy areas, springs and large rocks, which are charged with social significance. For example, a mountain peak may be one node of a sight-line that directs an approximate location for burials. The practice of charging natural elements of the landscape with social significance or content is known from ancient sources and the ethnographic record in general (Tilley 1994:24-25; Bradley 2001; Aijmer ms.). We must add here that it is awkward to make sharp distinctions between 'cultural' and 'natural' features; such distinctions are likely to vary between individuals and over time, especially in seeking relations to older constructions that may seem 'natural' to some, while 'natural' formations on the other hand may seem constructed (cf. Fahlander 2001:47f).

Tilley has on many occasions proposed various relations between burial monuments and geomorphic features of the landscape (1993; 1994; 1996a; 1999). In a study of the locations of southern Scandinavian megaliths, he finds some regional tendencies. In the parish of Västergötland, the frequency of megaliths are great and they seem to be sited to maximize views over mountains, mimicking or duplicating the landscape in various ways (1993:63). In Bohuslän, the tombs are generally smaller, often placed in the margins with views over valleys and the sea or related to rock outcrops. They are in a sense 'embodying commentaries upon the landscape' (1993:67ff, 78f). In the flat terrain of Skåne in southern Sweden, the Neolithic tombs are mainly located on productive arable land close to the sea and to rivers (Tilley 1993:59). The regional differences in location may have many explanations, and one of them may be that their location was not very important or 'meaningful' after all. However, the general patterns suggest that they represent different 'bundles' of structurating practices, which to some extent vary according to the local milieu. This is perhaps something to expect; the Neolithic of southern Sweden was not very homogeneous, although there surely were some common fibres and ropes. Different re-

sources, topography, etc. may yet be significant parameters. Criado and Villoch (2000) have made some interesting notes on these matters in their study on the location of Neolithic barrows in the Barbanza mountains of north-western Iberia. Their approach falls between phenomenological studies and structural analyses of social space. They do not seek to study perception on the individual level, but rather on the social scale (2000:190). They investigate four types of possible relations underlined by visibility and proximity:

1. Association with natural lines of movement,
2. Association with rocks and significant natural features,
3. Association with other previous monuments and
4. Association with the villages of the builders (2000:199).

With the aid of formalised access diagrams and permeability diagrams (Fig. 1), they sketch out an ideal, topographical scheme of natural rooms and pathways connecting them: "the link between monuments and movement indicates that the barrow acted as an artificial reference within a complex code of signals which transmits information about routes" (2000:211). The paths linked the dead with the living.

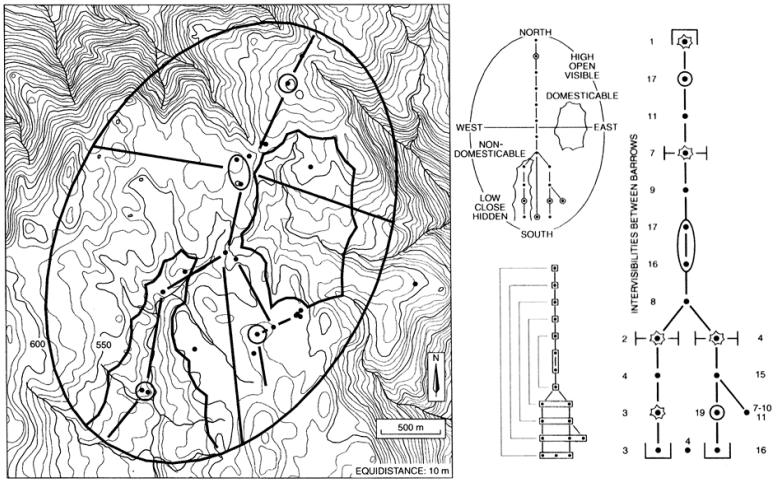

Fig 1. Formalised outlines of the Sierra de Barbanza area: diagrammatic scheme of the landscape (top centre), translation of this into physical space (left), and representation of the symmetrical correspondences of the barrow system (bottom centre and right, Criado & Villoch 2000:207).

In the Barbanza mountains, natural features, like rock outcrops, seem not to have been significant in the organisation of space (although the authors point out that such relations are known to been important in related regions, 2000:201). Points 3 and 4 are more suggestive; cup-marks, are suggested to outline topographical borders. The barrows of the Barbanza area are divided chronologically into two groups; the first type was constructed between 4200-4000 BC, while the second type was erected between 3500-3000 BC. Criado and Villoch argue that the later barrow-builders made strategic use of the first group in an "act of reaffirmation of what has come before, and negotiating with it or overcoming it". They claim to have found a general structural pattern in the organisation of space, based on a re-use of natural spaces and the construction of artificial spaces based on circularity. All barrows in the area share three central features: 1. a circular vista, visually enclosed, close to watercourses, 2. a border of a panorama of natural features, visible from the horizon, and 3. a well-defined, centre point. These defined panoramas are circularly organised as two opposing halves: one open for domestication, the other hidden and enclosed. This general model is repeated in smaller variations (2000:207). Criado and Villoch's study is impressive and innovative but nevertheless leaves a number of questions unanswered. One is how they defined the routes and paths of the area, as there seem to be many alternatives. Another problem lies in their formalisation of space; it seems that their model is self-fulfilling and abstract and present a general pattern that the prehistoric inhabitants would have had problems in apprehending from their situation within the 'real' landscape. The circular principle and opposition between domesticated and wild would thus be an unconscious imperative typical of Neolithic Iberians, but *why* and *how* are questions that Criado and Villoch leave unanswered. Instead, it seems as if the model that was applied to help the analysis rather became the conclusion. It does not seem very likely that the inhabitants on any conscious or unconscious level could have acted in accordance with a circular principle which is only identifiable after a series of top-down general abstractions (habitation equals domestic area and the 'unmarked' landscape is supposedly wild and undomesticated). It would undoubtedly be better to speak in terms of certain structurating practices, instead of ascribing a general, circular cosmology or mentality to the Neolithic barrow-builders. Nonetheless, Criado and Villoch's study is still interesting, as they point to a variety of material and cultural aspects that may have functioned as nodes in the structuration of space.

The significance of waters as structurating media

One example of a natural element that is seldom accounted for in detail in spatial studies is the potential use of different kinds of waters as structurating nodes. Many archaeologists have pointed out that waters may be important elements in burial practices. Richards has discussed the ritual aspect of waters in relation to late Neolithic burials in Britain, and Pikoulas has suggested that some burials were placed on the other sides of waters to prevent contamination (Pikoulas 1988:188-191; Richards 1996; see also Hall 1976:361; Tilley 1993:59; Parker Pearson 1999:125, 132). Water is a complex aspect of the environment with many different and changing characteristics and may have different effect in relation to different activities.

Mike Parker Pearson (1993) has explored such relations in an analysis of Iron Age Jutland (500 BC to AD 400). This is a regional analysis that involves relations between burials, settlement, topography and proximity to water (1993:208). The settlements of Jutland are generally situated within 300 m of fresh water but diverge over time in topographic situation and orientation. The location of burials is more varied; in the early pre-Roman period (PRIA), they were generally situated on the upper slopes of hills or plateaus, c. 200-500 m away from settlements and far away from waters (up to 1000 m). This variability is decreased in the following periods, in which the locations of both burials and settlements tend to get closer and to use similar topographical settings. In the late Roman period (LRIA), there is also a tendency for burials to be placed on hilltops rather than on middle and lower slopes (Parker Pearson 1993:211). Parker Pearson finds no significant patterns with regard to the location of burials above or under settlements or orientation from settlements (1993:212). In the early period (500-100 BC), when the dead were placed far from the settlements, the corpses were cremated and accompanied with only simple, metal or bone, dress fittings (1993:224). Around AD 200, however, when the burials were situated closer to the settlements, the burial customs changed towards inhumation and a gradual decrease in the portable values of grave interments. In the later period, the internal layout of the burial grounds also reveals spatial differentiation by gender (1993:224).

Parker Pearson thus identified a number of differences in the disposal of the dead, internal changes in the graves, intra-site, spatial differentiation, and different relations to waters and topographical settings. He identifies a change of the situation of burials in relation to waters; in the early PRIA, they were placed at distances of up to 1000 m but in the final PRIA and LRIA periods, they were placed closer to water (100-200 m). Parker Pearson interprets this variation mainly as a social development, a growing concern with lineage and individuality and gender differentiation.

The structuration of the landscape is thus seen as a strictly social (human) process which only utilises the properties of the environment. The layout of the landscape and previous activities play a passive role in the process and the spatial analysis is two-dimensional neglecting local visibility and the fact that distances are relative according to the level of friction in the landscape. Of course, it seems better to incorporate and combine as many aspects of the environment as possible, including visibilities and accessibility.

Summary of the general arguments

The examples of Parker Pearson, Criado & Villoch and Tilley are based upon larger samples of excavated and chronologically situated information. They cover extensive time-spans and the data are interpreted within regional and cultural frames. This synthesising approach is, somewhat surprisingly, widely accepted in archaeology, bearing in mind the many objections to such generalisations (cf. Chapter 2). Differences between local structurating practices and material circumstances make such general patterns less significant from a microecological point of view. Topographical analyses of activity areas need, generally speaking, to be relative and adjustable to compensate for 'ideal' and 'real' compromises between the materialities of given microecologies. To discuss such relations, a dot on a map will not be sufficient. One time-consuming but effective approach is to employ 'phenomenological associations', a corporeal experience of their situation in the natural and cultural landscape. From a situated viewpoint within the topographical context, visibilities (e.g., sight-lines) towards special, surrounding, cultural and natural features and alternative ways of movement can be registered. It is admittedly a little strange to use the bird's-eye perspective of two-dimensional maps since pre-modern individuals seldom had the ability to act and think through such a perspective (cf. Horden & Purcell 2000:10). But, indeed, corporeally adjusted maps can also be generated with GIS software in various ways (Zubrow 1994; Fahlander 2001:56f;).

Another central issue, which is well illustrated by Parker Pearson's data, is the temporality of topographical relations and how quickly they can alter within a culture group and region. An aspect that is not pursued is the question of the actual relations between settlements and burial grounds. Closeness in space and time seems sufficient for assuming actual relations. Such parameters are probably relevant in a majority of cases, especially regarding formal cemeteries, but as we have seen at the Ajvide site, such spatial proximity can be illusory. There is no way that we can be sure that, for instance, a group of graves next to a settlement actually contains

dead individuals from that settlement. It may be a likely assumption, which may be reinforced by similarities in datings, materialities, etc., but closeness in time and space can be more complex than we tend to believe. Just to give an example, during the expansive period, the Roman military needed quick routes to transport armies and hence built roads across some strategic areas in occupied territory (Alcock 1993). These constructions were in some respect 'alien' to the local farmers and inhabitants of mainland Greece but ran close to their settlements etc. They were likely to have been used locally for many purposes than the initial, main objective but were still imposed on them and their local setting by 'outside' powers. We would thus expect to find burials of the workers who constructed the roads, as well as burials of wounded or sick individuals of passing troops who happened to die while moving from one camp to another. There is a multitude of possible variations that we need to consider in our analysis; perhaps the most important are to develop more 'intuitive' approaches, in order to grasp the possible relations between settlement sites, burial grounds and attributes of the landscape.

Asea and Tegea: Two microecologies of Greece

In the introduction, the recent discovery of a number of cist burials in the Asea Valley was discussed. I shall continue here and discuss the structurating practices involved in placing the dead in relation to the spatial position of empirical data. The data are primarily taken from the parish of Arcadia in the middle of the Peloponnesian peninsula of Greece, particularly the high-plateaus of Asea and Tegea. Asea is a relatively small (c. 50 km^2), hilly valley surrounded by high mountains, while the much larger Tegea includes a varying topography and a longish extent that comprised several individual, ancient *poleis*. The discussed data were mainly acquired by traditional, intensive, surface surveys, in which I participated. The Asea Valley was surveyed during the period 1994-96 and the area around the ancient *polis* of Tegea was surveyed during the period 1999-2001. Very little of the material discussed here has thus been excavated and the information from each feature is scarce. But because of my in-the-flesh experience of these localities in general - and the graves in particular - I have found Asea and Tegea to be interesting microecologies in discussing relations between the placing of the dead and the environment. There is little meaning in comparing directly the results of these two rather different cases. I shall rather take the opportunity to demonstrate the fallacies in making regional generalisations even between two neighbouring locales like Tegea and Asea. The study would certainly benefit from a larger sample, but,

owing to the workload that such extensive fieldwork would require, I have to settle with these examples and minor excursions. I will begin by briefly discussing the practice of surveying and the specific problems in finding and dating graves from different periods, before I discuss the two microecologies in turn.

Fig. 2. Asea and Tegea and other discussed sites in the Peloponnese.

Survey fieldwork is not a straightforward approach with a consistent, uniform methodology. The degree of intensity, the recording procedures and the sampling strategy vary both between and within survey projects (for a general critique of field survey methodology, see e.g., Bintliff 1991b; Alcock 1993; Cherry & Davis 1994; Bintliff 2000; Iacovou 2001). The traditional, main object of modern field survey has generally been to pin-point settlements or activity areas of different time periods. The scale for analysis is normally a 'terri-

tory' or environmental region, a 'logically' limited area (e.g., Cherry 1994:105). The concluding result is generally presented, with or without the help of GIS and statistical tools, as a number of dot maps or interpolations showing artefact distribution or the spread of settlements of each period within the region. The interpretative analysis is thus conducted on the meso- or macro level, although the actual fieldwork may be quite fine-grained in detail. The detailed knowledge and experience of the landscape derived from fieldwork and the information retrieved from large quantities of sampled artefacts and other traces of activity is reduced to general, spatio-temporal models. What is finally published is thus the end of a series of interpretative chains based on various fragmented sources, from the classification of a single potsherd to remains of structures or fragments of literary accounts (cf. Hodder 1999). These chains of associations and hypotheses are rarely accounted for or discussed in detail, although anyone working with survey data is well aware of how yesterday's conclusion may be overturned by the re-interpretation of a single variable. For instance, the date of an artefact may on closer examination suddenly turn out to be a couple of centuries older or later and re-visits may provide contrary or new information. I shall present the case of a more detailed account at the end of this chapter that illustrates the complexity of different, simultaneous and separate uses of a small area. At this point, I settle with emphasising that these overall, problematic circumstances of surveyed data are bound to make the following discussion tentative and illustrative rather than to present a culture-historical account of the local burial customs.

Surveying burials

Traditionally, survey projects have not been particularly interested in locating such non-habitation features. The practice of modern surface surveying has its main origins in the settlement archaeology of the 1970s, and its traditional goal has been to discover habitation areas in order to discuss long-term, settlement patterns (Barker 1991:1). Graves have traditionally been regarded as objects that need to be excavated to be of importance (although there are regional analyses that start from the positions of graves of estimated dates). Burial archaeology as a sub-discipline generally discusses population ratios, life-spans, social identity, gender relations and social structure by relating burial structures and artefacts within or between areas or time periods. This kind of analysis is hardly possible for burials registered in surveys, mainly owing to the paucity of retrievable information. It is therefore not surprising that burials are seldom recognised or explicitly discussed in traditional surface sur-

veys. A quick look at some of the recent, published surveys carried out in Greece shows few newly discovered graves in relation to the number of habitation sites and other activity areas. The numbers of registered graves and grave-like features vary from 0.1 to 1.12 per km^2 (Table 1).

Survey	Area	No. of graves	Graves/km2
Keos	c. 40 km^2	5-8	0.12-0.20
S. Argolid	c. 44 km^2	11-16	0.25-0.36
Laconia	c. 70 km^2	9-11	0.13-0.16
Berbati-Limnes	c. 61 km^2	20-23	0.33-0.38
Nemea	c. 80 km^2	16-19	0.20-0.24
Asea	c. 33 km^2	18-23	0.55-0.70
Tegea	c. 50 km^2	56	1.12

Table 1. The number of registered graves and grave-like features in recent published surveys (Keos: Cherry, Davis & Mantzourani 1991; S. Argolid: Jameson, Runnels & van Andel 1994; Laconia: Cavanagh *et al.* 1996; Berbati-Limnes: Wells 1996; Nemea: Cherry, Davis & Mantzourani 1996; Asea: Forsén & Forsén 2002).

The numbers in Table 1 are tentative figures and are not directly comparable. Important factors that affect the number of registered features are the extent of the area that has been surveyed and the detail in which the work has been conducted (intensive/extensive). Another factor is the degree and quality of the information provided by the local villagers. Many graves have certainly been lost due to recent, destructive, formation processes, such as agricultural and building activities, but the major reason for the low number of registered burials in surveys is most likely to be the extremely low visibility of many types of graves. The visibility is dependent on both burial traditions and social aspects. Monumental burials, such as megaliths, chamber tombs and Bronze Age *tholoi* or mounds, are obviously more easily spotted than subterranean ones of more modest construction. This factor also skews the balance between different time periods, as the visibility of burials varies according to changes in burial practices.

The dead may, during some periods, have been disposed of in such a way that the remains are not retrievable in the archaeological record (cf. Morris 1992:196). For instance, some individuals may have been buried at sea or in rivers (Bradley 1995:vii, with references; Kyle 1998:214, 252.). Another factor is, of course, variations in

population ratios and social strategies that permit only parts of the population to be 'properly' buried. Not surprisingly, the majority of recorded burials are either of superterranean type from the prehistoric periods or graves from later, more populated periods. The time factor is also affected by mode of construction. Stone-built cist graves and, to a lesser extent, tile graves are more durable than, for instance, simple pit or earth-cut graves. The visibility of subterranean burials also depends on the rate of modern utilisation of the environment, such as intensive farming or different kinds of construction work (e.g. Kristiansen 1985:7ff). Generally, subterranean burials are visible only because of recent, destructive activities. Many of the registered features are commonly visible in road scarps — or, as in one case in Asea, at a modern quarry. A further important factor is the extent of looting; many features would never have been found later if the burials had been left intact.

What we are facing is complex relationships between several unknown factors, which make it hard to draw any substantial conclusions from graves registered in surface surveys. The conditions of such graves are commonly disturbed and seldom contain bones or burial interments, rendering a positive date for the structures difficult to establish. Hence, too, it is sometimes problematic to determine whether the discovered feature is *de facto* a grave at all. For these reasons it may seem a waste of time to devote effort to finding burials in surface surveys. Nonetheless, burials are not only interesting with regard to their interments and types of construction. Their location in time and space, and their relationship to other activity areas and elements of the landscape, can also provide interesting social information. The locations of burials are seldom randomly chosen but are related to the properties of the landscape and to the structurating positivities of the particular social formation. The structurating practices involved in placing the dead are most likely to be a mixture of the practical, economic, social and cosmological considerations of a heterogeneous population. In such a time-space perspective, burials can supply valuable social information, as well as enhancing methodological strategies to increase the number of registered features during surface surveys.

Chronology: Dating and typologies

A central problem concerns the dating and typologies of burials from different periods. Settlements and other activity areas are generally easier to date from the type of tile, ceramics or other artefacts. Burials found during surveys are often more difficult to date. The construction elements of the less elaborate burials are unfortunately seldom sufficient for dating — even if they are found nearly intact.

Cist graves of stone, for instance, were employed in prehistory, as well as in historical and modern periods, in a large part of Greece and beyond (e.g., Kurtz & Boardman 1971; Cavanagh & Mee 1998). There seems to be no clear, chronological correlation between different types (e.g., pits with cover slab, cists built up of rubble, more elaborate cists of larger slabs, etc.). The tile graves are also difficult to date; tile graves of varying constructions are recorded from the Helladic to the Late Roman/Byzantine periods.[29] Tiles have apparently been deployed in burials for as long as they have been used in buildings. There are no indications of specially designed burial tiles; all these graves seem to be built up of ordinary roof-tiles (Backe-Forsberg 1978:124; Coleman 1986:152). The tiles themselves can, however, offer a rough date for a burial. For instance, black and red brush-painted tiles are generally ascribed to the Late Archaic to Hellenistic periods, while light-red, dip-painted tiles are a typical Roman feature. In some cases, incisions, like finger-stroke marks or stamps, may hint at a date. A problem here is, of course, that the tiles are often re-used in later constructions. The different types of tile-graves: flat-tile cover, gabled-tile cover, flat-tile cover, over and under body, and tile cist (Backe-Forsberg 1978:124) are represented from different periods but may still carry chronological traits. Hägg, by comparing published Classical with Late Hellenistic material from Corinth, Olynthos and Eridanos, found correlations between the uses of flat and curved tiles and that of gabled and flat graves. His analysis indicates a change in the late Classical/early Hellenistic period from the Laconian towards the Corinthian type of tiles, as well as a more marked change in the same period from gabled graves towards a greater number of flat graves (Hägg *et al* 1980:119). There are many examples of how the grave or elements of it imitates houses (Vermeule 1979:48f). Hodder (1990; 1994), among others, has argued that elements of the houses of the living are symbolically represented in the properties of burials (the 'houses of the dead'). The third type of tile graves (flat-tile cover, over and under body) may thus hint that such graves were constructed in periods when the houses were floored with bricks or tiles (i.e. Roman).

However, the type of construction is not only a chronological trait; it also involves social aspects. For instance, at Asine, the gabled constructions are used only for adults, whereas the flat- and single-tile burials are mainly for infants. Hägg also found some local correlations between the orientations of the graves. In Athens, the early (i.e. Classical), gabled graves are mainly orientated east to west, while the later (i.e. Hellenistic), flat burials are of north to south orientation (Hägg *et al* 1980, 120). Hägg's analysis is altogether interesting, but it also points to the fallacies of pan-Hellenic or regional comparisons. The burial practices of ancient Greece seem

generally to have been very much locally constituted and to have varied considerably, even within quite small regions (cf. Vermeule 1979: 6). In the cases of Asea and Tegea, as well as most surface surveys, these chronological traits are thus of minor help. The type of construction or orientation is generally undeterminable or vague. Here the most promising approach is to date the bones, when they are present (as has been done with features S 44: 1d and S 42: 2b in Asea).

Period	Approximate time span
Dark Ages (Geometric)	Second half of the eleventh century to eighth century BC
Archaic	Seventh century to early fifth century BC
Classical	Second quarter of fifth to later fourth century BC
Hellenistic	Last quarter of fourth through first century BC
Early Roman	First to third century AD
Late Roman	Fourth to early seventh century AD
Byzantine	Seventh to early thirteenth century AD
Ottoman	Second half of thirteenth to early nineteenth century AD

Table 2. Chronological table (after Alcock 1993:36). The different periods are used here only as general temporal divisions, not as cultural signifiers.

The graves of Asea Valley

The Asea Valley has not until recently been subjected to any major, large-scale, archaeological analysis. The locations of some ancient remains are known from the diaries of the early travellers of the 17th century (see Forsén & Forsén 2002), while the first archaeological work was conducted by Erik Holmberg in the 1930s and 1940s (Holmberg 1941; 1944). He excavated the remains of Bronze Age houses and burials on top of the acropolis Paleokastro and made a plan of the Doric temple on top of Mount Agios Elias. In the post-war period, some rescue excavations have been carried out by the local *ephorate* (county administration), especially in relation to the construction of the modern highway that runs through the valley. An extensive survey conducted by Pikoulas in the 1980s also included the Asea Valley (Pikoulas 1988). The first large-scale investigation of the valley was performed in 1994-96 by the Asea Valley Survey Project (AVS), directed by Jeanette Forsén of the university of Gothenburg. The intensive survey of c. 18 km^2 (with an additional, extensive survey of c. 15 km^2) of the valley, aiming "to reconstruct the settlement history and the interplay between settlements and landuse in the valley drawing not only on material

culture distributed in the area, but also on geological information collected in a geomorphological survey accompanying the archaeological field work" (Forsén *et al* 1996). The AVS project has continued by additional analyses; in 1997 the Doric temple was re-excavated (Forsén, Forsén & Østby 1999) and in 1999 the remains of the city wall of the polis Asea were documented (Forsén, Forsén & Karlsson 2002).

The burials in the Asea valley constitute a total number of c. 25 features distributed between 11 locales.[30] Some of the features are registered as 'possible graves', while others are more likely to be remains of ancient burials. In some cases it seems plausible to apply the proximity hypothesis, for instance, the burials at S3 which are contemporary with the large medieval settlement on the hill slope. There are no circumstances that suggest that these features do not 'belong' to that settlement. In a number of cases, however, the relations between settlement and burial grounds are diffuse and not at all obvious. This may be explained by the nature of the survey, which can never reconstruct or present all activity areas of all times. We may, however, suspect that the proximity hypothesis is not the only parameter in the structurating practices of placing the dead. As previously argued, there are more likely that a matrix of a series of parameters is involved (we may use the metaphor of fibres and bundles of fibres). Some parameters may concern distance or visibility from settlement or other activity areas, others may be related to natural and/or cultural aspects of the environment. In theory, we may add a whole range of metaphysical, economic and social aspects that may be coherent or contradict each other etc.

Notwithstanding the difficulties of separating the aspects of structurating practice and material, biotopic parameters, it may be interesting to discuss the location of a few of the burials at Asea in relation to elements of the landscape and cultural features. For instance, it is tempting to relate the Roman burials of S 42 (features 1a-e) to the nearby Roman *villa rustica* at S 1.[31] The burials are situated about 300 metres north-east of the site. This would fit well with similar proposed relationships of the Roman burials that are associated with a Roman *villa rustica* at Berbati (Hahn 1996:219-20). In this case, orientation may have been a main, structurating parameter during the Roman period at these locales. A further hypothesis is that the burials were placed along what was possibly an ancient road from the Tegea valley to Asea. The *kalderimi* (a stone lined medieval donkey trail) at S 42 may be a later re-use of an older path dating from earlier times. This hypothesis is based on another discussed relation: the placing of burials along roads. The burials may in either case be related to the *villa rustica* at S 1 but be placed ac-

cording to either of, or by combining, two general structurated parameters.

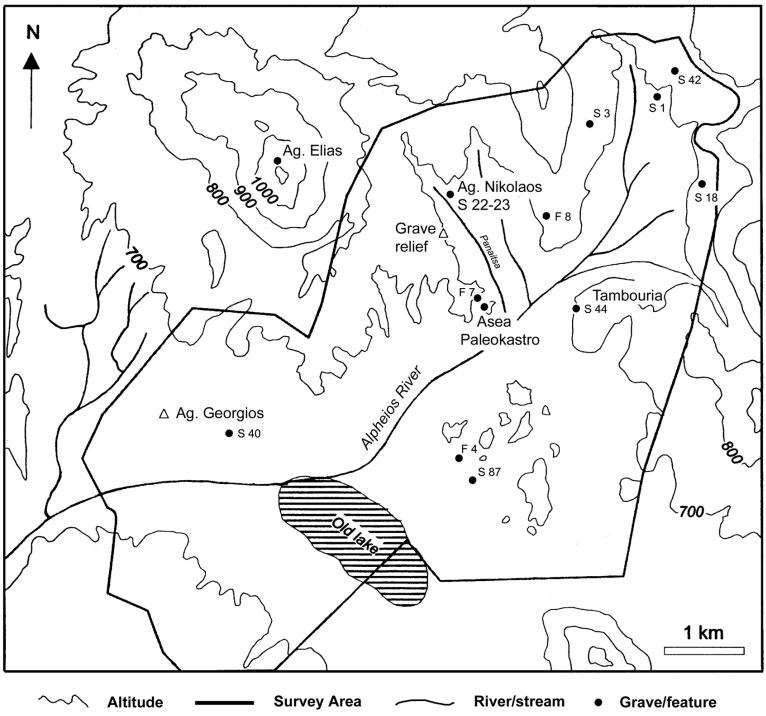

Fig. 3. Map of the Asea Valley, the survey area, and the locations of discussed features.

Similar reasoning can be applied to the Roman or Late Roman grave relief that was found by villagers west of the Panaitsa ravine at Chorafambela (Forsén *et al* 1996:92). If the information of its origin is correct, it may indicate a series of undiscovered burials along the suggested ancient road that stretches from Paleokastro to the west (Forsén 2002:79-104). These burials may be related to the contemporary site S 22-23 on the other side of the Panaitsa ravine.[32] Here, we may be dealing with another set of relations: burials placed along a road *and* on the opposite side of waters; unlike S1 and S 42, they are placed south-west of the settlement, which may indicate that the parameter of orientation is less important. The case of S 22-23 is quite complex: according to oral information from the villagers, there are

many possible burials in the area of which only a few were located during the survey (Forsén & Forsén 2002). Here, we face chronological problems, as the burials may be older, contemporary or later than the settlement. Unfortunately, the burials of S 22-23 provide no chronological traits or datable material.

Relations to water and roads and orientation are, however, not the only parameters that may play a role in choosing the locations for burials. Another, just as interesting aspect is the relation between the *signifiants* of the landscape and the location of burials. In this particular locale, such a possible feature is the oddly shaped rock, popularly referred to as the 'Gypsy Rock' or the 'Black Rock', situated c. 100 m north-east of S22-23 at Agios Nikolaos (Pikoulas 1988:62). This conspicuous, small rock sticks out of the landscape and, as its names hint, has obviously stimulated people's imaginations in the construction of local myths. It is plausible that this special feature was, consciously or semi-consciously, charged with social significance also in the past, operating as a structurating node, and that it seemed appropriate to place burials in its proximity. As a single case, this hypothesis may be stretching such relations too far, but it is nonetheless provocative.

The 'missing' necropolis of Paleokastro

Probably the most interesting example in the Asea valley concerns the question of where the inhabitants who lived on, and around, the Paleokastro were buried. When Holmberg excavated the site in the 1940s, he discovered a number of intra-site burials on the top of the acropolis. He excavated seven cist graves, two *pithoi* (with children) and 22 earth-cut graves at the centre of the plateau. One of the cist graves contained a Late Helladic vessel and three of the earth-cut graves contained vessels dated by Holmberg to the Middle Helladic period. Several of the burials are associated with the Middle Helladic buildings and are hence of the same date. The other graves were dated to the Middle Helladic on the premise that the acropolis lacked settlement during the Late Helladic. However, Holmberg also suggests that they may be attributed to another, unknown, nearby settlement. The cist graves were of varying types of construction. Apart from the common type of lined stone slabs with covers, some were in the form of cists with a floor of smaller, flat slabs without covers. Other graves were built-up stone enclosures and at least eight individuals were buried underneath the floors of the buildings (Holmberg 1944:22-26). These dated, prehistoric burials have unfortunately no direct parallels among the other registered features in the valley.

A curious aspect is that Holmberg did not find any burials of the Neolithic and Early Helladic settlement phases. "In spite of energetic searching in the slopes of the nearest hills north and northwest of the hill of Asea, we have not succeeded in finding traces of any necropolis there" (Holmberg 1944:21). Also Pikoulas surveyed the area without result (Pikoulas 1988:56). Could it be that another structural parameter overruled the handiest solution of placing the dead in close proximity to the settlement? Indeed, there are cases in which burials are situated quite far away from the settlement. For instance, the distances between Bronze Age settlements and their related, tholos burials range from within 100 m up to 1,500 m (Cavanagh & Mee 1990:54; Wells 1990:128, Branigan 1998:17), even though, as in the case of Paleokastro, there are suitable places closer to the settlements.

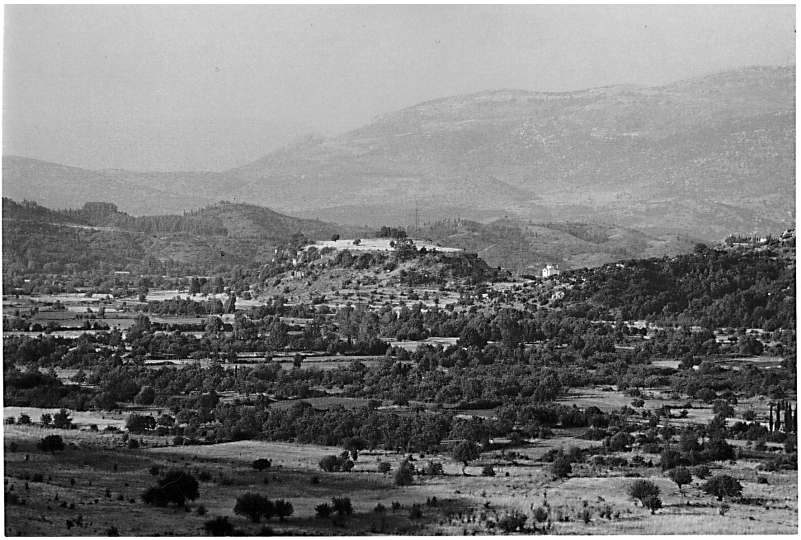

Fig 4. Asea Paleokastro seen from north-east (Forsén et al 1996:79).

Bearing these considerations in mind, it may be possible to attribute the graves on the north-western slopes of Tambouria (S 44, features 1a-e) to the settlement around and on the Paleokastro. S 44 is situated c. 800 m east of the Paleokastro on the other side of the Alpheios river. The parameters involved here would include the water of the Alpheios, functioning as a separating medium between

the living and the dead. Indeed, the closest area on the opposite side of water is the north-western slopes of Tambouria. The reasons why people would have bothered to transport the dead for long distances are far from obvious. It may have been as Pikoulas suggests, to avoid contamination, but other metaphysical and cosmological aspects may also have been involved. A factor that contradicts a relation between the early (Neolithic and Late Helladic) occupation phases around Paleokastro and the burials at S 44 is the radiocarbon date (UA-15485) of one of the buried individuals (feature 1d, cal. AD 540-645). However, as previously argued, it is not necessary to attribute all burials to the same period. It is suffice to mention the cremation burial 1e of S 44. There may thus be cases of deliberate or unconscious, secondary burial in the area.

Alternative candidates to the burials at Tambouria are the graves at Agios Nikolaos (S 22-23), which are separated from the Paleokastro by the Panaitsa ravine. There is even a possibility that both clusters of burials (S 44 and S 22-23) may be attributed to different periods of settlement around the acropolis. The two burial grounds are similar in construction, being simple cist burials, placed on opposite sides of waters, but differing in visibility in relation to the acropolis.[33] The Tambouria and its eastern slopes are highly visible from Paleokastro, while the small hillock of Agios Nikolaos is less visible. The difference of visibility, too, suggests that the two burial grounds belong to different settlement phases around the Paleokastro. If the radiocarbon date of feature 1d is representative of the whole group of burials at S 44, they may be connected with a Late Roman settlement phase. The problem is that there is little evidence for a Late Roman, occupation phase around Paleokastro; only a few sherds of that period were registered during the survey. It is, however, plausible that the area was settled also during that time but which at the present is buried under the silted area east of Paleokastro (Forsén & Karivieri 2002:491ff; but see Drakopolous 1997).[34] If this was the case, some of the burials at S 44 may have been placed according to the parameter of division by water but also by a principle of visibility from the settlement. The same principles would be true for the burials of S 22-23 but with the difference that they are 'hidden' from the settlement. This may indicate a change in structurating practice between different, occupational phases or perhaps a social differentiation between different, social series.

Traces of extra-local, structurating practice

The acropolis of Paleokastro is an excellent candidate for an object that can function as a signifying node in the structuration of space. Indeed, the Paleokastro is an impressive sight from the lower slopes

of Tambouria. There may very well be a recollection/association and glorification of the Bronze Age acropoleis, similar to the hero cult of Bronze Age tombs (e.g., Morris 1988; Sourvinou-Inwood 1993; Antonaccoi 1994), which make the site special and loaded with significance. We must, however, consider the possibility that some of the graves originated from temporary visitors, i.e. extra local series of individuals. Possible candidates may have been Roman soldiers, traders or other kinds of 'passers-by', etc. According to this hypothesis, it is thus possible that the site was chosen for burials in order to make claims to the land, as well as to establish a connection with its past.[35] The low visibility of the type of graves would not necessarily make such a practice less suited for ideological statements. Relations of this kind (water - acropolis) exist only in a limited number of contexts. It is therefore possible that the dead were transported to an appropriate setting. In such microecological matrices of cultural and natural elements some relations may be more important than others.

Fig 5. Feature 8a at Agios Nikolaous (S 22-23). Photograph: F. Fahlander.

At this point, we can only speculate about who were buried on the slopes of Tambouria, and by which kind of series, but it is interesting to note a parallel phenomenon at Pylos in Elis (Coleman 1986). As in the case of Asea, a number of Late Roman burials were dis-

covered east of the acropolis on the opposite sides of waters. The settlement phases of the Pylos acropolis stretch from Classical times and it was resettled during the Byzantine period. But there are no traces of domestic buildings from the Late Roman period. It thus seems as if these culture-nature relations are somehow related to certain structurating practices in the disposal of the dead.

The graves of Tegea

The Tegean plain is situated on a mountain plateau c. 670 m above sea level in the midst of Arcadia (Fig 2). As mentioned, it is larger than the Asea valley, but the area in question here is restricted to the south-eastern part (Fig. 6). The ancient *polis* of Tegea, situated c. 10 km south-east of the modern city of Tripolis, extended over a quite large area that at present is occupied by nine smaller villages (Fig. 6). Tegea was excavated several times during the late 19th and early 20th century (e.g., Bérard 1892; Dugas 1921; see Voyatzis 1990), and the remains of the old temple of Athena Alea, the Tegean agora, a theatre, etc. have been exposed. A shrine was also excavated on the Agois Sostis hill north of the ancient Tegea. Despite these and other archaeological works in the area, many constructions and places still remains to be identified. For instance, from the written sources, we know that there ought to be additional shrines, a gymnasium and a stadium in the area (Voyatzis 1990:14-17).

After a period without any major archaeological fieldwork, the area was once again the object of archaeological investigation in the 1990s. The temple of Athena Alea was re-excavated (Østby *et al* 1994) and the ancient city area with its surroundings was surveyed by the Norwegian Arcadia Survey project (NAS) between 1999 and 2001 (the results are not yet published, but see Ødegård *et al* ms). The data of the Tegea survey present a different kind of material and environment from those at Asea. The ancient *polis* of Tegea was larger and occupied a more strategic location between other ancient city-states. The archaeological information recovered by the survey suggests that the central area was more or less continuously populated from archaic times to the present. The area is also at present more densely populated and suffers from greater, recent 'disturbances' in the form of intensive agriculture and a larger quantity of domestic housing. There are also differences in extent and methodology between the two surveys. The Asea project was a typical, large-scale, survey seeking to cover as much of the land as possible while the survey of Tegea was more of a small-scale site survey. The survey of Tegea had a number of goals, ranging from identifying the hydrological and botanical development of the area, to determining the extent of the ancient city among several subsidiary projects. One

of the latter aims was to locate ancient burials, especially the main Tegean cemeteries. The result was in some respects less impressive than that at Asea, but still yielded c. 56 possible grave features distributed at 10 locations (see Fig. 6). It is tempting to apply the hypothesis from Asea to the neighbouring microecology of Tegea, to see if any of the suggested relations are also to be found in this area. If not, that would tell us something about the fallacy of regional generalisations regarding the placing of the dead or perhaps sustain the hypothesis of the importance of the topographical environment. We may also find the data of Tegea interesting, as the different circumstances evoke other types of questions regarding the importance of grave location.

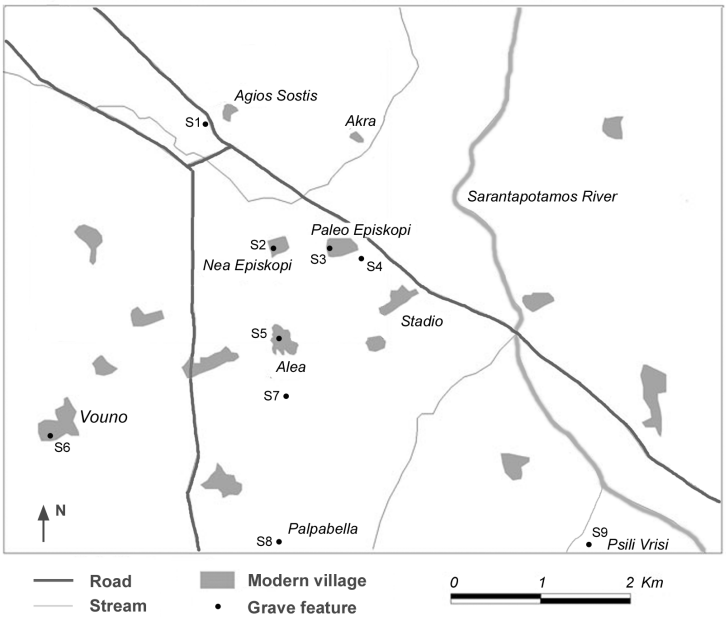

Fig. 6. The locations of the graves of Tegea. The extension of the ancient city has approximately the shape of an oval covering the modern villages of Nea Episkopi, Paleo Episkopi (the site of the ancient theatre and agora) and the northern edge of Alea, the location of the temple of Athena Alea (Modified from Ødegård *et al* ms).

The environmental setting of Tegea differs, as the ancient *polis* of Tegea is situated on a plain with few possibilities of finding likely *signifiants* that may have had a function as structurating 'nodes'

(e.g., small hillocks, rocks, etc.). In the area of ancient Tegea, there is no monumental acropolis like the Paleokastro at Asea. The hillocks of Acra and Agios Sostis (north of Tegea) may have served a similar purpose, but their inconspicuous and less monumental constitution makes them less likely to have been loaded with metaphysical significance. They rather serve a purpose as a 'northern barrier' separating the *polis* of Tegea from visible contact with the rest of the plateau. Also the burials found in the Tegea area differ in many respects from those in Asea. The major part seems fairly recent (e.g., Slavic/Medieval/Turkish of whose burial customs in this region we know less in comparison with the Bronze Age to Late Roman customs). Within the presumed city wall, there are a number of later burials at two locations. A number of burial pits were discovered during the excavation of the temple (S5) and fragments of similar features were found in a scarp next to the ancient theatre (S3). It is hardly a pure coincidence that we find these burials here, as both the temple and the theatre during this time were converted into chapels. It is, however, also plausible that the very existence of ancient structures initiated a practice of burial in their vicinity (cf. Cleary 2000). There are parallel burials (possible Slavic) found during the recent excavation of the temple on top of Agios Elias in Asea (Forsén *et al* 1998; 1999).

Traces of more ancient burials in Tegea are, in contrast to the more recent ones, all found 'outside' the assumed limits of the ancient city. The registered features comprise one stone-lined cavity on the slopes south of the temple of Alea (S7), two grave *stelai* incorporated in houses in the villages of Nea Episkopi (S2) and Stadio (S10) or the shattered remains of an elaborate burial found south-east of Palaio Episkopi (S4) and west of Agios Sostis (S1). It is difficult to make any associations regarding the locations of these burials; the hydrological situation is complex in the area, but it is likely that at least some of them are situated according to similar parameters to those in Asea (separated by waters and placed along roads). However, their positions highlight another positive aspect of the benefits of locating burials. They are in general placed *outside* the presumed city wall.[36] The general pattern of later Iron Age city-states was the practice of extra-mural burials. The location of ancient burials can thus be helpful in determining the extent of the ancient city wall of Tegea. Some of these scattered traces may indicate the locations of the cemeteries that ought to have been located outside the city (the position of the grave *stelai* may hint at the possible locations of an eastern and a western cemetery). Their positions would actually fit well with the proposed location of the 'Pallantic gate' in the west (Callmer 1943:113) and it is also likely that there was also an eastern gate towards the Argolis area. A problem with such speculations is,

however, the fact that ancient marble pieces are known to have been transported for quite long distances. It will suffice to mention the grave *stele* found in Asea at Chorafambela, which at present is situated in the floor of a goat-pen c. 5 km up the Agios Elias mountain in the village of Ano Asea (Forsén *et al* 1996:92).

The somewhat few traces of graves near ancient Tegea is partly due to the flat topography of the area, which give rise to problems in locating ancient graves. The slopes of the small hillocks surrounding the ancient *polis* have all been searched for traces of burials but with little result (the stone-lined cist S7 in the south and the Roman burial S1 in the east). The most promising location, the southern slopes of Acra and Agios Sostis, was scanned thoroughly without result. These slopes are separated from the city area by a small stream that runs from the north-east, following the contours of the hill. Perhaps some aspect(s) prevented this area from being utilised for burials. There are nonetheless traces of activity on the hill, for example, scattered remains of a Roman *villa rustica.* It is situated on a ridge of the northern slopes between Agios Sostis and Acra, c. 5 km east of the *polis*. No burials were found in its vicinity. Perhaps the closeness to the polis made it more suitable to bury the dead in one of the main burial grounds than to establish a private burial plot? However, oral information has been received from local farmers that suggests that there were once Roman burials on the hills in the area, but with no details of their exact locations or age. Both these examples are awkward; we do not know for sure whether the lack of registered features implies that there never were any graves in the area.

The cairns of Papabela and Psili Vrisi

Although the Tegean plain shows little evidence of the normal types of grave that one would expect (stone cists and tile graves), it presents us with a more unusual kind of structure. The most peculiar structure found on the surrounding slopes of the Tegean plain are clusters of *cairns.* The cairns are constructed of small stones and pebbles, ranging from 5 to 20 cm in diameter and forming mounds with varying diameters (1-3 m) and heights (0.5-1.5 m). Constructions like these are not unique to the Tegean plain, although they are in unusually high numbers in the area. A resembling feature was found in Asea situated c. 20 m north of the Paleokastro. It was first registered as a possible grave but was later dismissed as possibly a clearing cairn or perhaps a property marker. However, the cairns in Tegea may alter that interpretation. On the northern and northwestern slopes of the Palpabela hill, about 40 cairns were registered as possible burial cairns (S8). Some smaller ones are probably just

property markers and others are likely to be remains of collapsed terraces. The internal relations, situation and form of the majority of the cairns, however, apparently indicate something else. They mainly appear close to each other and often in the middle of the terraced fields. In addition, they occupy only the northern and north-northwestern parts of the Palpabela hill, south of Alea (Fig. 8).

Fig. 7. One of the larger cairns on the top of Palpabela, ruined by a large tree growing in the middle (seen from the SSE). Photograph: Fredrik Fahlander.

The largest cairns (up to 6 m in diameter and 3 m in height) are situated on the eastern crest on top of the Palpabela hill with a panoramic view of the southernmost part of the valley, including Lake Taka in the west. This evocative pattern suggests a hierarchy of the cairns with the most prominent (or first?) situated on the top and thus visible from long distances. These circumstances suggest that the cairns are possibly graves. That interpretation is also sustained by the reports of locals who claimed to have found bones (human or animal) inside 'stone structures' near Ag. Dimitrios on Palpabella (Moraitis 1832). Some of the cairns also show traces of looting or at least attempts to reach the core.

Similar clusters of cairns are also found on the slopes of the village of Vrouno between Lake Taka and ancient Tegea (S6) and at Psili Vrisi in the south-western part of the Tegea plain (S9). The lat-

ter case also presents an interesting pattern. The cairns are at present situated in a cypress grove, which is enclosed by a low wall of pebbles and stones of unknown date. The enclosure includes two of the cairns, leaving one on the outside. The wall actually *avoids* the cairn by making a circle around it, instead of simply letting it become a part of the enclosure (Fig. 8). This phenomenon also suggests that the cairns are something of a less mundane character.

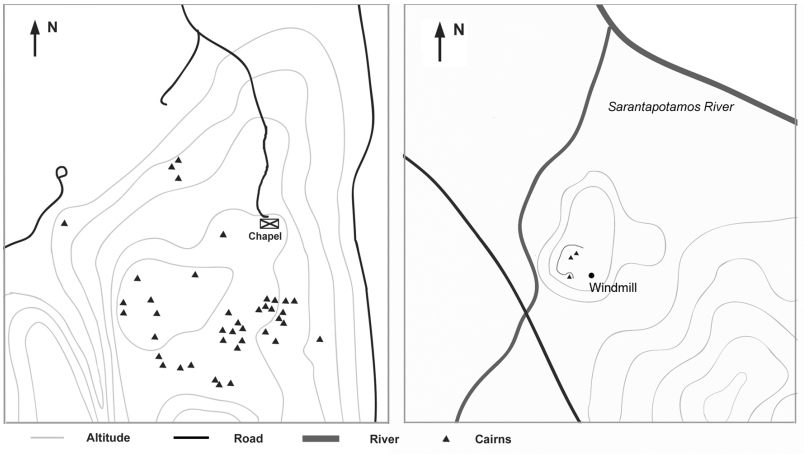

Fig. 8. The cairns of the Palpabela hill, south of Alea (left) and at Psili Vrisi with the enclosing terrace wall (right).

The dates of the cairns of Tegea cannot be established at this point. During the survey, a few, medieval, glazed handles and rims were collected in relation to them, both at Psili Vrisi and at Palpabela. The local villagers who 'excavated' one of the cairns attributed them to the Slavs for unknown reasons. That account must be taken at its face value or at the most hinting at a rough date. From their different appearances, the cairns give the impression of being of different ages; a few of them are overgrown and some are covered with lichen, while others are 'clean' and seemingly more recent. It is interesting to note that, if these cairns actually represent burials, their number on Palpabela suggests that they belong to a larger community, perhaps a later settlement phase of ancient Tegea. To sustain such an interpretation, however, we will need to excavate and get proper dates for the features.

The particular circumstances of Tegea, as being one large habitation site with a long time span, make it less fruitful to discuss

individual, habitation-burial relations, as in the case of Asea. Tegea may not provide many examples of complex matrices involving relations with the 'natural' landscape, but the data can nonetheless be employed to illustrate the complexity of analysing relations between different practices and events. It is, however, interesting to note that water may have played a similar, yet different, role here, as in Asea. Unfortunately, the area east of the Sarantapotamos river has not been intensively surveyed, although extensive scans for traces of burial have been made. We do not know if more burials of the Tegean *polis* were ever located so far from the settlement area. It is, however, interesting to note the differences between the *poleis* of Asea and Tegea and those of Corinth or Athens, where several large cemeteries of different periods are found outside the city walls. There may be many different explanations for this lack. But it is hard to believe that similar constructions in layout, extent and types were ever present in the two microecologies. We may suspect that there were some differences in burial customs, not accounted for in the literary sources, which can explain the present data.

As previously mentioned, comparing two neighbouring locales may not necessarily sustain general patterns of regional burial customs, but rather point at local variability. The differences in constitution and data of the two microecologies thus call for different questions. The cairns found in Tegea (burials or not) have only one known counterpart in Asea, which makes that particular question more favourable in the Tegean context. Instead of seeking evidence of the discussed relations as being local, structurating practices rather than single anomalies, it suggests that we need to attend to any given microecology on its own terms (cf. Holden & Purcell 2000:80). This also provides an opportunity to take the surroundings of an ancient *polis* as serious as the main city itself.

In Chapter 2, I pointed to the need for detailed discussion of the order and relation of different activities at a certain locale and so far I have only briefly touched upon the ambiguity of the survey data. I will therefore take the opportunity to end this chapter by narrowing the scope and discussing how such data may be used to adduce chains of activities on the microlevel. Apart from smaller excavations in the Tegean surroundings, it is the *polis* of Tegea that has been the main object of study. The 'outside' hinterland has seldom been highlighted despite the fact that a considerable number of inhabitants of the *polis* probably did not live within the city walls (cf. Purcel 1995). It may thus be interesting to present a slightly different discussion of past activities in the outskirts of the city, that is, discussing the particular geology, hydrology, and the sociocultural formation processes of locales and places in a given, small area. Such a microarchaeological analysis will imply that we minimize the

importance of 'external' elements in the chain of arguments, as well as general presumptions of what was the proper or common practice of a certain time and region (see Fahlander 2001, Cornell & Fahlander 2002a). One such interesting microlocale is situated on the Tegean plain c. 1.5 km north-west of the ancient agora (i.e. outside the presumed city-wall), due west of the Agios Sostis hill. It embraces the suggested burial ground S1, which will make it interesting to follow that particular chain of arguments in detail.

Adducing activities from surface scatters

The area in question can somewhat haphazardly be limited by two modern asphalt roads: the smaller road from Tripolis to Tegea in the east, and the road connecting the two running towards the modern village of Palaio Episkopi in the south (Fig 9). The ridge of a small hillock delimits the area in the north and a stream in the west. About 80% of the area was walked during the survey and, despite varying visibility, the extensive coverage of the surface provides a good basis for detailed analysis. Most interesting is the north-eastern part of the area, which yielded large quantities of ancient material on the surface (fields 116, 118, 236, 238, 240, 242, 244 and 246). At present, the area is mainly used for agriculture; cereals are grown on the slopes, but there are also some fruit-tree plantations on the lower plain. There are only a few modern houses in the area, mainly situated along the eastern road, but otherwise few modern constructions in the area, except for some dirt roads. During the survey, some fields were ploughed (fields 118, 244, 246, etc.) and some had been recently harvested (fields 236 and 238), but some parts were not walked over, owing to low visibility. Close to the area, a few hundred metres east of it, near the top of Agios Sostis, a concentration of miniature cups, figurines and fine ware of the Archaic to Hellenistic periods was found, indicating the place of a small sanctuary.

The hydrological situation of the Tegean plain is generally complicated. Rivers have changed their courses and flooding has been severe during some periods. The area west of Agios Sostis, however, seems not have been affected by this. At present, a small, dried-up stream runs south of Agios Sostis and Akra and then diverges to the north, approximately following the Tripolis-Sparta highway. The stream seems not to have caused flooding in this area. The lowermost 20 m of the western slopes close to the stream are empty of finds and the soil is more silted. In between fields 130 and 132, close to the stream in the lowermost part of the area, there is a recently dug ditch c. 1,2m deep. In the scarp of this ditch, a number of painted tiles and sherds of black-glazed pottery were found, situ-

ated c. 60 cm below the present ground surface. The tiles lay horizontally and were thus probably undisturbed by the plough. Altogether, this indicates that there has been no major flooding in the area, except for small occasional overflows. In addition, the small ravine running down the Agios Sostis hill towards the eastern asphalt road does not seem to have disturbed the area. There are two minor aqueducts under the road, constructed of ancient slabs of marble, but there are no visible signs of silting or flooding west of the road. At present, a ditch along the road diverts any occasional water from the ravine in the southern direction.

Fig. 9. The slopes west of Agios Sostis with surroundings. The dotted line shows the extent of the *kalderimi* and the curly grey line the small stream. Walked fields are enclosed by rectangles.

Traces of ancient constructions and activities

There are a few remains of large constructions still visible on the surface. The most striking one is the traces of a *kalderimi* running in the SSW-NNE direction. It is a 1-1.5 m deep gorge lined with stones stretching from the eastern road close to the ridge, but diverging in the SSW direction (see Fig 9). After c. 50 m the stone lining ends and the kalderimi changes into a dirt track for an additional 40 m. At this point, all clear traces of its further extension end. There is, however, yet another 30 m of a small, asphalted road in connection with the kalderimi, but the direction diverges towards the east. A dirt road in the same direction begins on the other side of the asphalted road to Paleo Episkopi but leads into what must have been wetlands in past times. The area between the southern road and Nea Episkopi is low in altitude, reaching a level that is actually below the level of Lake Taka. The fields consist of fine, silted soil and yielded very few finds and seem thus to have been flooded at some point. There are, however, more traces of kalderima, on the slopes north of the area that runs in the NNE direction.

There are also a few remains of ancient buildings in the area. In the middle of the southern slopes of the small hillock west of Agios Sostis (field 236) are traces of an undefined structure. It consists of five concentrations of stones and slabs, forming parallel lines down the slope (see fig 10). Only the southernmost of them resembles a built wall, whilst the others are mere heaps or strands of stones. On and around the structure, a number of thicker tiles were registered. The structure was first interpreted as the remains of a building, but, because of its curious outline, it may just as well be the remains of another kalderimi. The date of the structure is uncertain; it may very well be quite recent. The thick tiles found on and around the walls suggest an ancient date, but they may also have been re-used in a much later construction.

If the structures in field 236 are uncertain in age, the large, rectangular slabs of marble visible at the southern end of fields 240 and 242 present better evidence of ancient buildings. These marble blocks along with those re-used in the aqueducts of the eastern road, are much more likely to have originated from the area, as there were no reasons for later transportation to their present location. A piece of a *sima* of Classical date, a number of larger tiles (one with a decorative motif of tongue and dart, possibly Classical) and thick bricks with finger strokes of Roman date were also found in the same fields. Other traces of large buildings are also a small piece of decorated marble frieze of possibly Byzantine date and large quantities of black- and red-painted tiles (Classical to Hellenistic). These finds alone suggest the presence of larger buildings of the Classical, Roman/Late Roman, Byzantine and possibly Hellenistic periods.

Fig. 10. Stone 'walls' indicating a 12x40 m structure built on the midst of the slope of field 236. Scale 1:1000

The 'small finds' sampled in the fields of the same part of the area (116, 118, 236, 238, 240, 242, 244, 246 and 248) further complicate the picture. The ceramics indicate activities from the Archaic, Classical, Hellenistic, Roman, Late Roman, Byzantine and medieval periods. In addition, one possibly burnished, body sherd and a fragment of a green-stone axe also hint at prehistoric activities. There are, however, no Bronze Age materials from the area. The majority of the collected material consists of cooking wares, mainly from Roman/Late Roman times, including *terra silligata* ware, a millstone and trade amphorae, but also later medieval material (stabbed handles, green-glaze ware). The finer wares are mainly of Archaic and Classical date (craters, hydrias, red-figure vases, etc.). There are also two fragments of Hellenistic figurines (a possible maenad and a horse?) and a mould for making relief figurines (possible Classical to Hellenistic date). There would be no logic in treating the material from the individual fields of this part of the area separately. The present-day fields are modern constructions which have very little, if any, relation to ancient circumstances. Furthermore, after fifty years of modern agricultural activities, the artefacts can hardly be said to be *in situ*; it is more probable that they have been tossed around from one point to another. The density of ancient material on the surface is as high as within the ancient city area. This suggests that the area was a place of intensive uses, both as operating in concert as well as conflicting.

The Agios Sostis area thus presents us with complex evidence of a diversity of activities over a long continuous time span. Here we must be concerned with the relations between the long-term, short-term and the situational events, as discussed in Chapter 2. The

multiplicity of activities that the material evidence suggests calls for a number of hypotheses that may or may not relate to each other. The diverse and sometimes contradictory, material evidence from the chosen area evokes a multiplicity of possible practices and events. Some may seem evident; others are less supported by material evidence or are based on weak chains of relations. At this stage of tentative analysis, a pragmatic way of discussing past activities on the slopes west of Agios Sostis is by discussing the bases for a number of analytical *fields of practice,* based on the present data. The first conserns the traces of ancient roads, whilst the second concerns the possible use of the area as a burial ground. The other fields discussed concern traces of domestic and 'cultic' activities, as well as possible, recent, formation processes.

Communications

We can safely assume that there are a number of paths and roads connecting the *polis* of Tegea with the surrounding areas (size, trade, military activities). There must have been some way of approaching ancient Tegea from the north, for instance, connecting it with the *poleis* of Orestesion and Mantinea or an east-west communication link between Argos and Asea. An intriguing observation is that a part of the eastern asphalt road points straight to the centre of the ancient agora. Could it be that the eastern asphalt road is partly based upon an ancient road from the north to ancient Tegea? Water from Agios Sostis may have posed a problem for transport during periods of much rain, but that seems to have been a limited problem. According to the present-day geology, the water from the hill would flood for only a few days per year. In addition, this was obviously not an insuperable problem for the kalderimi. Perhaps ditches along the road diverted the water, as at the present time?

An interesting feature is, however, the divergence between the kalderimi and the asphalt road. Could it be that the medieval kalderimi indicates older communication links? The initial direction points straight to present day Nea Episkopi. This is also the place where Callmer argues that the western city gate (the Pallantic gate), mentioned by Pausanias, was situated (Callmer 1943:115; cf. Voyatzis 1990:13). But why would later communication choose another route than a previous one? A possible scenario is that the ancient road followed approximately the direction of the kalderimi but that the direction needed to be changed in later times, perhaps owing to the known flooding of the western part of ancient Tegea (or that the kalderimi never reached Tegea but headed for another medieval or Turkish village west of it). It is also significant that the asphalt road follows a pretty straight route between Tripolis and

Tegea, which hints at a later date (Tripolis has no known, ancient predecessor).

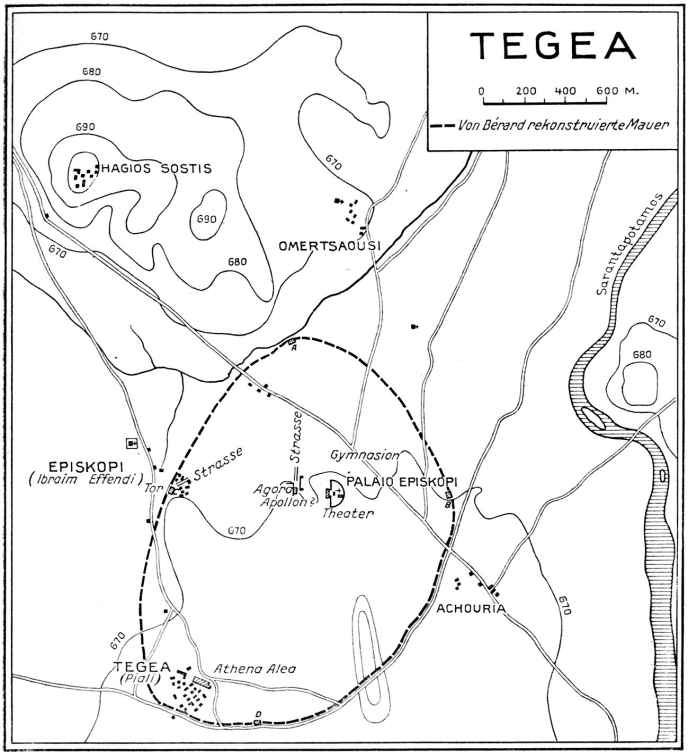

Fig.11. Bérard's map of the Tegean plain in the late 19th century (reprinted in Callmer 1943:113). The suggested outline of the city wall is not in agreement with the finds of the NAS survey but note the two roads passing Agios Sostis.

Another possibility is that the diverging roads (the modern and the kalderimi) may indicate an ancient crossroad. The fresh-water from Agios Sostis would have made this a good resting-place, offering water for the animals etc. The problem is that the area is very close to ancient Tegea, i.e. there was little need for a resting-place. There may, however, have been a need to keep animals (or individuals) outside the city area for different reasons, perhaps to avoid contamination? The high concentration of various artefacts may also

indicate that the area was closer to the polis than was previously thought, perhaps even right outside a city gate. The city-wall may have been situated where the southern asphalt road to Palaeo Episkopi runs at the present. This, however, is not very likely; it would not fit with Bérard's finds of city-wall remains (Voyatzis 1990:12f, see Fig. 11) or with the large slabs of stone found by the NAS north-east of Nea Episkopi. In addition, the southern part of the area is low in altitude and, as previously argued, easily flooded. Nonetheless, such a picture would explain the intensive, multi-period activity in the area, as well as the odd direction of the kalderimi.

Traces of burials

If we take a closer look at the smaller finds of the area, we find evidence that can be related to the suggested ancient communications. The finds from the area include a special category of material, indicating that the area may have been an ancient burial ground during the Archaic/Classical period. Among the finds, there are fragments of red-painted amphorae, Laconian craters, smaller pithos (for child burial?), and amphora lids made of re-used tile. Such objects are generally well known in burial contexts. Following this line of thought, it can be argued that some of the painted tiles and the marble slabs may have originated from monumental graves of the same period. For instance, the one piece of a decorated marble frieze found in field 244 is likely to have originated in such a structure. It has been preliminarily dated to the Byzantine period but may prove to be older. If the preliminary date stands, it may also indicate a continuous, or later, use of the area for burials. The burial hypothesis is further sustained by the fact that the area is separated from the *polis* by water (the stream running south of Agios Sostis and Acra), which, as previously pointed out, is a common factor in placing burials in Archaic to Roman times. Furthermore, the stone axe found in field 246 can be linked to the general burial hypothesis. The axe and the possible, burnished, body-sherd may at first glance indicate Neolithic activity, but the material is a bit too small and uncertain for such an interpretation. Interestingly, the axe was found amongst the Archaic to Hellenistic debris and seemed curiously to originate from the same context. A perhaps far-fetched, yet not impossible, interpretation is hence that the axe may have been re-used and may originate from one of the proposed burials. The re-uses of prehistoric stone axes in such contexts are rare but are known from several cases (e.g., Montelius 1874:158-62; Romaios 1954). It is interesting to note that the first four hypotheses, which are not based upon the same material, support each other. As al-

ready pointed out, monumental and upper-class burials are often situated along the roads outside the city wall of a *polis* (e.g., Kurtz & Broadman 1973:91f).

Domestic use of the area

The burial hypothesis thus seems plausible, even if we rule out some of the links in the chain of arguments, but there are nonetheless data that may not fit with such a picture. A striking feature is the quite large amounts of coarse ware found at the site (including a millstone and thick bricks with finger-stroke incisions) of Roman/Late Roman date. A plausible interpretation of such assemblage at extra urban locations would be a small *villa rustica* or hamlet. The diffuse structure of field 236 may have been a part of such an agricultural complex, although it does not resemble any known type of agricultural building from that period. The evidence of domestic use in Roman/Late Roman times does not necessarily contradict the burial hypothesis. As argued, it seems a little strange to place a domestic building in an old cemetery. However, Tegea may very likely have suffered from the general, demographic decline during the Early Roman period (Alcock 1993) and hence, owing to a series of events, amy have broken with the previous burial tradition. It is nevertheless a little unlikely that the burials would have been completely forgotten and invisible. It may be that also the Roman coarse ware originated from less elaborate burials in the area and thus point to continuity (which also would make sense of the Byzantine marble frieze).

Traces of cultic activity

A more confusing type of material evidence is the two clay figurines and the mould from fields 240 and 242. One figurine would have been easy to dismiss by referring it to the nearby sanctuary of Agios Sostis c. 200 m east of the area, but two is one too many. One likely hypothesis is that the figurines, the mould, the marble blocks, the fine pottery and the monumental tiles originated from a sanctuary, possible of Archaic to Hellenistic date. There are, however, at least two known sanctuaries with figurines on Agios Sostis; one excavated in the 19th century (Voyatzis 1990:16ff) and the site recorded by NAS in field 88. Nonetheless, the kinds of figurine like those that can be made from the mould are not known from either of the two sites. The area may thus also include a small, figurine-manufacturing site for the sanctuaries on Agios Sostis.

Recent formation processes

Some of the mentioned contradictions and anomalies may be 'explained' by recent formation processes. We know that soil from other areas has been spread over fields in the valley, in present and in past times. It cannot be excluded that some of the material in fields in the north-eastern part originated from somewhere else. The survey team experienced this kind of situation in the very first field that was walked between Alea and Paleo Episkopi. The field yielded a great variety of fine-ware sherds and even ancient metal fragments, but it turned out later that the owner had once transported soil from the excavations of the agora to his field. It is thus not unlikely that the finds from the fields in question actually originated from some other place. In fact, the concentration on F88 may also be a dump from the excavation of the figurine sanctuary of Agios Sostis. However, the marble blocks and the monumental tiles and the *sima* suggest that there actually was some larger, possibly public building in the area, perhaps a sanctuary and/or monumental burial(s). Even if we consider the possibility that the smaller finds may have been brought to the area in recent times, the blocks are unlikely to have originated from any known excavation in the valley.

Of course, other fields of practice than those discussed are plausible. At this stage, however, the hypothesis of ancient communications and a burial ground are most conclusive. The material evidence and their relational links suggest that the area includes some ancient communication links related to the *polis* of Tegea. It is also hard to neglect the possibility that many of the sampled finds hint at ancient burials, although the temporal extent of these activities is still uncertain. Certainly, these hypotheses are worth investigating further. At present, only half of the sampled material has been preliminarily dated, and revisits may provide additional information, both qualitative and quantitative. Georadar probing, especially around the structure of field 236, and more general botanical analysis may also provide enlightening information. It must, however, be clear from the varied amount of material evidence that the area west of Agios Sostis was an important place in ancient times. This emphasises the importance of discussing city-states in context, instead of restricting the analysis to within the presumed city walls.

Summary of the particular studies

The suggested relationships between burials, settlement and the landscape sustain only a tentative discussion. The data from Asea and Tegea are too small and fragmentary to allow of any substantial

conclusions. Yet the choice of location for burials seemingly contains interesting information that can be utilised in two respects. The first is the social information that can be extracted. If some relations that are not purely functional can be established, these may tell us something about the cosmology and social organisation of a specific microecology. The comparative data also suggest (not surprisingly) that the topography is of importance in choosing the location for the dead. They also suggest that some vectors of the structurating matrix are present at both locales but that others have to be rephrased or varied in relation to the given properties of local topography. It is therefore important, when discussing matrices of structurating parameters (or bundles of fibres), to view them as multidimensionally related and constituted. Some parameters may be of long term and span over large areas (that is, also beyond the ancient Greek cultural sphere), while others are of shorter duration and perhaps mainly concern local practices. Some structurating parameters may thus overrule other parameters in some regions but be weaker or irrelevant in others. Structurating parameters with different degrees of supremacy may coincide or contradict each other. They may work in concert, coalesce or initiate epiphenomenal fibres. In short, there can hardly be any clear and unambiguous relations between the structurating principles of a certain time or space.

The second aspect of the study concerns methodology. In northern Europe, burials have been the main reference for locating settlements, as the latter are more difficult to discover, owing to the low visibility of domestic material. Such an approach is also possible for the Mediterranean region, although the factors are generally reversed. Here, habitation sites are easier to locate, since concentrations of tiles and sherds are commonly visible on the surface, while subterranean burials demand more thorough efforts for their discovery. If some of the discussed structural parameters can be sustained, the known positions of settlements can be used in combination with properties of the local environment to find possible locations for burials. At present, we face a "Catch 22" situation: more data are needed for guidance to gathering more data. The experience from Asea and Tegea nevertheless seems promising. As mentioned, most burials recorded in surveys are visible because of construction work or looting, and this is also the case at Asea and Tegea. The number of registered features, despite the small area, lack of subterranean burials and relatively well-preserved rural environment, is the result of quite limited efforts. Revisits to areas with one or two registered features have often proved to result in the discovery of additional features, turning single graves into cemeteries (e.g., S 44 and S 42). This implies that more advanced strategies, based upon structural relations between activity areas and the prop-

erties of the local landscape, are likely to yield further burial features that have not been found during ordinary fieldwork.

The discussion regarding the fields west of Agios Sostis in Tegea also seeks to illuminate the problems in interpreting activities and actions from fragmented and incomplete, archaeological data. Materials from surface surveys are often contradictory and seldom support simple, one-dimensional conclusions. The archaeologist needs to consider at least three dimensions in any analysis of past activities: spatial, temporal and social. It is not sufficient to establish that a locale was, let us say, a sanctuary. Some sort of cultic practices may very well have been performed there at some point, but we need to establish the actual time span and question its continuous use. It may also be that it was a sanctuary only to some individuals, but something else, or nothing, to others. There are unfortunately no straightforward ways of determining such relations, other than from the known evidence in each given case. Human social practice is seldom simple, rational or straightforward; it is rather messy, contradictory, different, multivocal, etc. By keeping our analysis on the general level, we would perhaps be able to dismiss some irregularities as 'background noise', but on the microlevel, including short-term or episodic activities, we must take that 'mess' into account. It will thus be hard at this point to construct a historical account of the area that 'explains' and embraces all the known evidence. But it highlights the possibilities of constructing hypotheses based on complex and fragmented information, which can help to direct future studies in a more precise manner.

5
Summary
The small and the local are significant?

I think we agree. The past is over
George W. Bush 2000[37]

Personally, I like reading summaries. If they are good, they capture the main content of an extensive text and leave it to me to enter more deeply into details if I find it interesting. The very same reasons lie behind my agony in *writing* summaries. How can I summarise the preceding arguments in a way that makes them understandable and evokes interest in an impetuous reader like myself? The microarchaeological approach does not constitute a completely logical system which prevents any simple schematics or brief summaries. I believe, however, that Zizek is right in his argument that no system, of belief, ideology or scientific system, can ever present a complete set of arguments. Instead of trying to capture an imaginary whole by a flawless model, it is certainly better to employ a flexible model. Yet such a model needs to be operative, comparable and sustainable as a basis for comprehensive, descriptive narratives. But all right, I will provide something for readers like me, a brief recapitulation of the major themes that I have discussed in the preceding chapters, but I will also present a somewhat less traditional version that perhaps better grasps the 'Real' content of this thesis.

In Chapter 1, I postulated a number of general themes that I promised to discuss in the following text. These areas of interest will suffice for a general summary. The first theme was to explore the possibilities of the small and ordinary, the 'subaltern' objects of the past. This means both an interest in the non-spectacular and the

seemingly non-informative fragmented objects of the past, as well as highlighting everyday practice instead of general cosmologies, religious systems, etc. (i.e. conceptual imaginaries). In the theoretical chapter, I have thus focused on particular aspects of social practice. A number of concepts, like series, serial collectives and serial routes, have been elaborated in order to make sense of the regular aspects of social practice without falling into the trap of structure-functionalism and simplified notions of social identity. The serial perspective somewhat solves the problem of emic versus etic perspectives, as it is based on *executed practice* in relation to local materialities.

The interest of the small and ordinary also lies behind the choice of empirical examples in Chapters 3 and 4. The graves of Asea and Tegea have not previously received any great attention from archaeologists; they are generally regarded as indefinitely mute, subaltern objects. I hope that that view can change and that I have succeeded in my attempts to stress their potential despite their poor state of preservation. The suggested relationships between burials, settlement and the landscape seemingly contains interesting information that can be utilised in social analysis. If some relations that are not purely functional can be established, these may tell us something about the social structure of a specific microecology. By comparing the data between Asea and Tegea it is also suggested that the local topography is of importance in choosing the location for the dead. Some structurating vectors seem to have been present at both locales but others seems to have been rephrased or varied in accordance to the given properties of local topography. It is therefore important, when discussing matrices of structurating parameters (or bundles of fibres), to view them as multidimensionally related and constituted. Some parameters may be of long term and span over large areas (that is, also beyond the ancient Greek cultural sphere), while others are of shorter duration and perhaps mainly concern local practices. Some structurating parameters may thus overrule other parameters in some regions but be weaker or irrelevant in others. Structurating parameters with different degrees of supremacy may coincide or contradict each other. They may work in concert, coalesce or initiate epiphenomenal fibres. In short, there can hardly be any clear and unambiguous relations between the structurating principles of a certain time or space. I have also, in the discussion of the Tegea survey data, tried to illustrate the need to keep the chain of deduction 'open', as many areas (and artefacts) have multiple, overlapping, succeeding or contemporary uses.

The Ajvide data in Chapter 3 are more complex and can perhaps not be regarded as insignificant and ordinary, at least not from a regional and culture-historical point of view, that is. For a burial

ground of the Pitted Ware Culture, the Ajvide site represents about two-thirds of the known graves so far (Malmer 2002:91). But taken out of that context, the burial ground is quite modest, encompassing only 71 graves so far. In my study, I have discussed the practice of burials from the viewpoint of the social practice of the living over the social identities of the dead. This was done to, at least partly, overcome the awkward questions of what is manifested in a burial; the social personae and the social significance of the dead or the needs and wishes of the living. The study has proved to be interesting in many senses, and I believe that I have illustrated the need for thorough discussions regarding the sequence of events.

Another outcome of the Ajvide study leads us to my next objective: to stress social heterogeneity and find means of doing archaeology without confining the analysis to general cultural traits or regional traditions. The concepts of structurating practice and structurating positivities were developed for this purpose. Structurating practices are not confined to certain individuals or series of individuals. They are situated in time and space but are still often part of a larger pattern of structurating positivities or ideologies. These concepts are particularly open and flexible in relation to 'closed' concepts like culture, region, society or any other static and regular notions. The relation between the concepts of structurating practices and structurating positivities is illustrated by the metaphors of fibres and threads. The thread (i.e. general structural patterns) is built up of entwined fibres (i.e. structurating practice), of which only a few, if any, are sustained throughout the whole length of the thread. Some fibres may break and disappear without affecting the endurance of the thread and others may replace some fibres. The metaphor is thus flexible in time and space, working beyond the fields of emic vs. etic and general vs. particular. To be able to discuss clusters of fibres, I have also elaborated the concept of microecologies. Microecologies are analytical, fuzzy entities that can be employed as a way of discussing local social practice without a need to confine the empirical analysis within cultural, regional or natural boundaries. They point out the intersociality of social practice; it may be locally executed but their incitements and consequences can be much wider.

The summary presented so far is one version of many and, as promised, I shall also approach the text from another angle. Let us begin with a return to the burials of Tambouria that introduced the thesis. Do we actually know more about these inconspicuous remains other than what first struck me when we rediscovered them back in 1995? In one way, we do know more, in another, we stand even more confused. The radiocarbon dating of human bones from one of the graves suggests that they (or at least one of them) was

constructed approximately between AD 530-650, during what is generally termed the Late Roman period. After several revisits, we also know that the original graves are part of a series of other, roughly contemporary graves. In one way, we thus know a bit more about the structures of Tambouria (i.e. type and date) but the more we pursue questions of their relation to other activities and elements of the landscape, the dimmer becomes the clear image of their social significance. The initial idea that the graves at Tambouria were related to the settlements around the Paleokastro may still be valid, but the apparently interregional character of some of the structurating practices involved (Late Roman burials on the other side of water in relation to an acropolis) also suggests that their constructors (and the dead) constitute a series that perhaps did not even live in the area.

It was originally an outspoken attempt to register not only the properties of the grave, but also the possible relations to prominent features of the landscape, as well as taking time off to evoke 'phenomenological associations' (i.e. smoking cigarettes while imbibing the environment). This does not necessarily imply that we fall into the grip of a phenomenology of meaning; the recorded observations do not aspire to have any emic content. A surface survey, intensive or extensive, can never provide a full picture of all the activities and uses of places in a given area. Landscapes change; buildings, routes, paths change as well, and so do the different apprehensions of the elements of the landscape by different individuals. It is nonetheless interesting to study the place of the feature in this way, discussing the choice of location and what parameters imputed a given, particular place before others. Such 'subjective' data are not as straightforward as the radiocarbon figures but are not necessarily less interesting.

Archaeologists are generally eager to place their objects firmly within a chronological frame. Indeed, dating and categorising objects are sometimes described as be the main goal which provides a basis for organising old and new data into some sort of system or general narrative. By dating our objects and constructions, we can relate and compare contemporary data, in order to 'expand' or 'mend' the fragmentary evidence into 'whole pots' of knowledge. Perhaps this emphasis on chronology and firm dates provides a sense of security that we are actually doing real science? Is archaeology a science in that respect? It is interesting to note here that Lacan (and Freud also, for that matter), despite their outspoken aims, never really considered psychoanalysis as a science (Miller 1996:26). In a similar sense, Foucault was well aware that he never wrote anything but fictions (1980:193). The same objections apply to much archaeology, which ought to have consequences for how we

pursue our analysis and construct our models and narratives. As an exemplification, let us consider the following quotation from Saul Kripke on the existence of unicorns:

> ...even if archaeologists and geologists were to discover tomorrow some fossils conclusively showing the existence of animals in the past satisfying everything we know about unicorns from the myth of the unicorn, that would not show that there were unicorns (Kripke 1980:24).

Kripke's reasoning may seem disturbing at first glance; indeed many archaeologists would be contemptuous if they discovered data that supported a theory of, let us say, a typical kind of social organisation. What Kripke is saying is that, if data are discovered that fit the description of unicorns, they may suggest that there once were horses with horns but that they were not necessarily the same as the mythical figures of unicorns. There is a difference, and it is this difference that I am seeking to highlight here. The very same reasoning applies to our construction of models and fictions, as discussed in Chapter 2. If we find data that fit well with our idea of, let us say, the general structure of chiefdoms, this does not imply that the past social formation in question was a chiefdom in the same sense. Our models of contemporary, small-scale or western, industrialised, social formations are at best food for thought, but not ready-made, universal, social models.

The same is also true for objects such as those in the previous discussion of bronze items like 'razors'. It also applies to composed objects, such as 'graves', 'boats' or 'cities'. When we excavate a prehistoric pit containing artefacts and human remains, we have not necessarily found a grave, as it is symbolised in a personal or contemporary order. It is a pit with artefacts and human remains that need to be analysed as the social practice of another symbolic order and ideology. The social aspects of Lacan's psychoanalytic theory implies that the constitution of the symbolic, the imaginary and the Real are an outcome of the constitutional processes in which we become social subjects. These realms are thus likely to be changeable over time and space rather than solid entities, slowly varying in relation to actual, structurating practices, structurating positivities (or fibres and threads) and material circumstances. Because of the glitches and gaps (symptoms) of these continuous processes there is a need for some sort of reductive, intelligible scheme to make the world intelligible: Zizek employs the term *ideology* for this 'web of alterations'. It cannot, however, be a question of only a single ideology, but rather of a varying number of competing *ideologies*, of

which some are 'subaltern' and others temporally more successful. Ideologies can be partly intentional, as in the vulgar sense of the term, but are more likely to be uncontrollable 'creatures' that 'rescue' us from the horrors of the Real. The question of change in the matrix of associative chains of signification (meaning/sense) is awkward. There may be brutal shifts, as in the epistemic changes advocated by Foucault, or changes of nodal points (master signifiers), as stressed by Zizek, but also in less abrupt ways, as discussed in Chapter 2. However, they are most likely to be slow-changing and inert processes, rarely apprehensible during the life spans of single individuals.

By this, I am not suggesting that changes in ideologies or structurating positivities happens above the heads of the individuals. Quite the contrary, the microarchaeological framework allows several different ways in which social practices and even social positivities can change rather than being reproduced. For instance, we may consider small, unnoticeable changes in structurating practice, perhaps caused by misunderstandings or inability to do things the 'right' way, that may cumulatively lead to social change. Changes in material circumstances (ecology or human constructions etc) can also potentially initiate changes in practice, as well as changes in ways of handling materialities (e.g., technical innovation, building techniques, house and settlement layout). Social change may also occur because structurally conscious subjects that occupy powerful subject positions may want it. Finally, structurating practices may change because of unintentional effects from other structurating practices. The list can be continued.

The past is thus not the 'culturally other'; the past is just different, and it is different in many different ways. This is why we cannot *understand* social practices beyond nodal points. Artefacts and archaeological data may only have contemporary meanings; on that matter, Hodder and other hermeneutics are possibly correct. But, as I have tried to show by the discussions of psychoanalytic and social theory (there is not really a difference between them), social practice, that is, *collective* practice, is by definition structurated in various ways, and from that it follows that traces of practice are not randomly constituted. Materialities and practice are very much related to the changing constitution of contemporary symbolic orders and ideologies, but there are yet *patterns* for us to retrieve and discuss. Here is one important point on which microarchaeology diverges from 'traditional' archaeology. Instead of finding parallel phenomena, searching for origins and relating a given practice to a contemporary, cultural context, microarchaeological analysis discusses relations between local social practice and *then*, so to speak, tries to drag the threads out of the ball of fibres.

But is it really true, does the temporal depth prevent us from understanding past practices? After all, there are some things that we have in common with many dead and other, differently constituted individuals. The remoteness of past social practice is quite apparent in the study of the Neolithic burial site of Ajvide. For instance, how are we to relate to the phenomena of grave no. 6, in which the skull has been removed and the teeth have been carefully re-ordered in their original places? The suggestions of ancestor cult or skull cult do not really tell us very much more about such an event. This practice is, at least to me, as incomprehensible as the quotation from the 16th century physician Pomme in Chapter 2. Is it an important event? Is it perhaps an example of subaltern practice? Or is it actually a freak instance, in which the general, social significance of teeth and jaws is articulated in the extreme? Either way, I hesitate to try to make sense of such a practice (although I find the seemingly overall social significance of teeth at Ajvide intriguing).

The general analysis of the Ajvide site and the patterns found raise other, perhaps more apprehensible questions. One thing that can be learned is that a detailed, particular study seems to extend the degree of complexity. They often reveal social heterogeneity also in small locales during short periods of time. What are we supposed to do with these awful abjects (symptoms) that do not fit into our general narratives? We can ignore (repress) them as insignificant anomalies or embrace them and be ready to change our assumptions about a particular time or place, to acknowledge local variability over homogeneous systems. This is a main advantage of the microarchaeological approach to a general problem of archaeology. The interest in the particular and the local in favour of grand narratives has only begun to meet with greater interest among archaeologists (except for highlighting the spectacular). This text has not provided more than a first attempt in this quest. Microarchaeology is not provable by a series of successful case-studies. We can never firmly establish clear relations between different practices and materialities of the past, but that problem of verification is not one that is unique to microarchaeology. Lacan often emphasized that he did not invent anything new; he merely advocated 'a return to Freud'. It was a reaction to the 'misreadings' of ego-psychology that neglected the basic elements of Freudian theory (Lacan 1988b:10; 1996:60ff). Microarchaeology is in the same sense 'a return to the past', a return to the objects and the material traces of practice beyond hermeneutic relativism and stiff, contemporary models and fictions.

Appendix A: Radiocarbon and TL datings

Grave	RE-corr	unCorr	Mar (%)
1a(h)	4120+/-85	4320+/-85	-
1b(h)	4030+/-60	4230+/-60	-
2(h)	4035+/-75	4235+/-75	62
2(s)	4150+/-96	4450+/-65	100
2(hh)	4150+/-65	4150+/-65	0
2(c)	4111+/-44		
6(h)	3935+/-60	4135+/-60	-
13(h)	4185+/-65	4385+/-65	-
19(h)	3815+/-95	4015+/-95	66
28(h)	4150+/-85	4350+/-85	65
29(h)	4035+/-65	4235+/-65	80
29(s)	4205+/-99	4505+/-70	100
29(sus)	4195+/-70	4195+/-70	0
29(c)	4128+/-43		
30(h)	3895+/-65	4095+/-65	86
36(h)	4015+/-75	4215+/-75	-
41(h)	4160+/-80	4360+/-80	-
53(h)	4000+/-80	4200+/-80	-
13 TL	2550+/-250		
14 TL	1810+/-200		

Table 1. Calibrated carbon dates of human and animal bones; reservoir effect correlated (RE-corr), and without reservoir effect (Corr) and TL datings of ceramics from burials (years in BP). Abbreviations: h=homo, hh=hedgehog, s=seal, sus= pig, c=combined of all three datings. Data taken from Lindqvist & Possnert 1997a:74, Possnert 2002:171f; Burenhult 1997f:xviii-xix; Lindqvist & Possnert 1997b:55f and Österholm 1989:93, 123.

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Notes

Chapter two:

[1] Some prefer to discuss 'corporeal materialism' or 'situated knowledge' instead of embodiment (Haraway 1989; Butler 1990b; Braidotti 1991) as a means of singling out a more political dimension of the social body.

[2] Phenotypic or genetic differences may certainly imply a different world-view in a phenomenological sense. For instance, Chodorow has argued that the experience of

pregnancy but also the small differences in sexual physiognomy, entail a different experience of the world (e.g. Chodorow 1978; cf. Featherstone & Turner 1991:20). Chodorow is certainly right in criticising the modern concept of the self as mainly based on male experience, but to suggest that female experience in general is different is less valid (1978:25; cf. Irigaray 1994:83).

[3] In line with Zizek, I do not read Lacan as a structuralist thinker but emphasise the constructionist aspects of his work (cf. Zizek 1989:153, 183). I will also leave out as much as possible of the indigestible terminology and mathematical models that frequently occur in his texts.

[4] On the relations between the Imaginary, the Symbolic and the Real, see Miller in Lacan 1996:22 and the translator's note in Lacan 1977b:279f. For an extended discussion, see e.g., Zizek 1992; Feldstein *et al.* 1996:39-237.

[5] The term Other is here used according to the Lacanian notion of *le grand Autre* (the big other), that is, discourse or 'the way of doing things'. It should not be mixed with the notion of the cultural other as used by, e.g., Todorov (1982).

[6] These three concepts of Lacan are not to be confused with (or to replace) Freud's topographies (unconscious, pre-conscious and conscious or Id, Ego and super-Ego) but are better conceived as abstract and analytical dimensions.

[7] The concept of serial routes is akin to Hägerstrand's 'projects'. We also find similar ideas in David Seamon's notions of body ballets, place ballets and time-space routines. A body ballet is "a set of integrated gestures and movements which sustain a particular task; a time-space routine is a set of habitual bodily behaviors which extends through a considerable portion of time". The place-ballet is "a fusion of many time-space routines and body-ballets in terms of place" (1980:158f).

[8] There may be some confusion regarding the seemingly similar content between structurating practices and serial phenomena or serial categories. Serial phenomena are *effects* of the mutual practices of a serial category; these serial practices, however, do not need to be structurating.

[9] Göteborgs-posten 19 december 2002, p. 30.

[10] The first term of Giddens' concepts of *structurating properties* and *structurating principles* is defined as "Structurated features of social systems, especially institutionalised features, stretching across time and space" (Giddens 1984:377). The latter concept are "Principles of organization of societal totalities; factors involved in the overall institutional alignment of a society or type of society" (Giddens 1984:376).

[11] This notion of a third and fourth level of hermeneutics was later reconsidered by Shanks and included in the double hermeneutics of Giddens. On comparing historical and ethnographical interpretations, cf. Lévi-Strauss (1963:17).

[12] For different definitions of society and social systems, see, e.g., Linton 1936:253; Malinowski 1939; Parsons 1951:5-6; Giddens 1979:66; Barth 1992:29; Descola 1992:124 and Luhmann 1995. Alternative suggestions of terminology are few. One was provided by George Dalton, who unsuccessfully tried to establish *Genus* as a term for 'socio-political cultural units' (Dalton 1981:39).

Chapter three:

[13] In the original: Ich kenne den Tod, ich bin ein alter Angestellter von ihm, man überschätzt ihn, glauben Sie mir! Ich kann Ihnen sagen, es ist fast gar nichts damit. Denn was unter Umständen an Schindereien vorhergeht, das kann manja nicht gut zum Tode rechnen, es ist eine springlebendige Angelegenheit und kann zum Leben und zur Genesung führen. Aber vom Tode wüßte Ihnen keiner, der wiederkäme, was Rechtes zu erzählen, denn man erlebt ihn nicht. Wir kommen aus dem Dunkel und

gehen ins Dunkel, dazwischen liegen Erlebnisse, aber Anfang und Ende, Geburt und Tod, werden von uns nicht erlebt, sie haben keinen subjektiven Charakter, sie fallen als Vorgänge ganz ins Gebiet des Objektiven, so ist es damit..

[14] There are, of course, many later elaborations and advances of the themes discussed by Kroeber and Ucko but yet surprisingly little efforts to deal with the problems they pose. On the contrary, the narratives of the history of burial archaeology are frequently very similar. They normally follow a similar line, departing from the work of Hertz (1960) Binford (1971) and Saxe (1970), followed by Tainter (1977), Pader (1982), Alekshin 1983 and O'Shea (1984). For the reader who is not acquainted with these works, there are good summaries and discussions in Chapman, Kinnes & Randsborg (eds.) 1981; O'Shea 1984; Anderson Beck (ed.) 1995; Parker-Pearson 1999.

[15] Houby-Nielsen (1995:145) argues that it is primarily the status of the *family* of the deceased that is manifested in the grave. Houby-Nielsen thus assumes a universal presence of family like systems and that the next of kin have a significant position in the burial practices. This cannot be presumed for all prehistoric social formations, although it is common in many contemporary cultures.

[16] Morris (1987:95f, ch. 6) argued that the variability indicated that only a few (the 'Agathoi') received a proper burial, whereas others are not (the 'Kakoi'). There are, however, some problems in the chronology of the burials. Papadopoulos (1993) argues that Morris, as in the mentioned case of Tilley in his Fjälkinge analysis, mixed chronological variables with the social (cf. Morris 1998).

[17] There are certain problems related to discussions of age. Age and sex determinations are in general vague based on osteological data, but are even more uncertain for individuals over 50 years of age (Crown & Fish 1996:807) and for children (Manchester 1989:11). Moreover, the general age-groups employed by physical anthropology are not necessarily well fitted for the actually lived experience of age.

[18] Burenhult (1997e:173; 2002:32) interprets the radiocarbon dates to cover a period between 2750 BC and 2300 BC, while Lindquist and Possnert (1997b:74), calculating with the reservoir effect, suggest a period of use between 2905-2072 BC (68% probability) or 3062-1928 BC (95%).

[19] An additional spatial aspect to be considered is the internal relations between the places of interments in different zones of the grave (head, waist, feet, etc. (e.g. Pader 1982; Malmer 2002:93).

[20] Österholm's (1989:92) preliminary data from 1989 suggested a relatively low stature (159 cm for women and 160 cm for men). The present data fit better in relation to the average stature of individuals at other Middle Neolitic sites of Gotland: 169 cm for men and 175 for women (Sjövold 1974:199f). The numbers can be related to other general estimates, e.g., Mesolithic Skateholm: 161/153 cm, Danish Neolithic megaliths: 164/154 cm (Ahlström 2001:325). But see Bennike (1985: 480).

[21] There may actually be 12 individuals that have been radiocarbon dated, but it is not clear from the published data in which individual the dating termed "Ajvide 1/1994" is taken from (see Possnert 2002:171f).

[22] The number of buried individuals is 79 including the cenotaphs. The life expectancy at Ajvide is c. 30 years. Minimum and maximum figures have been calculated for a 1000-year and a 500-year period of use respectively. The formula is based on a stable population with equal birth and death rates.

[23] The dark colour of the burial pits is, however, not found only in the long ones but is also present in some of the shorter and in pits, which contradicts the boat hypothesis.

[24] It is, of course, also likely that the 'undertakers' of series C actually wanted to relate to the older graves by deliberately intersect them.

[25] There is still a small probability within the range of the C14 data (90% probability) that all the burials are more or less contemporary.

Chapter four:

[26] Kirk, G. S. (ed) 1954. *Heraclitus. The Cosmic Fragments,* Cambridge: Cambridge University Press, pp. 280-3.

[27] Ingold points to the relations between the landscape and the different forms of activities performed within it: "Just as the landscape is an array of related features, so – by analogy – the taskscape is an array of related activities" (Ingold 1993:158). The environment is thus revealed to the perceiver through an active process of engagement, rather than constructed through an ordering of sensations passively received.

[28] The concepts of place and locale are awkward and relational abstractions, but useful as general terms. The concept of *place* is a subjective (emic) objectivation of a particular space in space (Tilley 1994:11). Places are seldom just any place but an area loaded with ethics, emotions, social significance, etc. The concept of locales, on the other hand, refers to the *use* of space as a *setting* of interaction (e.g. a room, street corner, city, etc., Giddens 1984:118; cf. Rapoport 1990:12; Tilley 1994:19; cf. Foucault 1986 on heterotopias).

[29] Kurtz & Boardman 1971, 164, 312; see e.g., Poulsen 1994:23; Schallin 1996:170 (Helladic); Jameson, Runnels & van Andel 1994:425 (Classical); Hägg *et al* 1980 (Hellenistic); Coleman 1986:152; Forsell 1996:326 (Roman/Late Roman).

[30] The burials of Asea are described in greater detail in Fahlander 2002.

[31] Feature 2b (UA-15484) is dated to cal. AD 530-650 (63.8%), AMS.

[32] In 2001, a small concentration of tiles, indicating the presence of burials, was discovered on the slopes where the *stele* is claimed to have been found. The area was not walked during the survey, owing to the dense vegetation.

[33] Another common feature of the burials of S42 and S22-23 is the close proximity to wells, which may be another factor in the structurating matrix.

[34] Drakopolous (1997:301) argues that Classical and Roman Asea was situated elsewhere. Building on Pausanias, he identifies Roman Asea with the site at Agios Georgos, 20 *statis* from Paleokastron. Holmberg found no intermediate material between the Helladic and Hellenist periods, but artefacts from all periods were found in the fields around the acropolis (Forsén & Forsén 2002).

[35] Bone samples have been taken from 9 graves and may be used for future DNA analysis to establish the individual's spatial origins.

[36] Burials were generally not allowed within the city walls for religious and practical reasons (Toynbee 1971:48; Kyle 1998:169, but see Bodel 1986).

Chapter five:

[37] The daily Morning News, 10 May 2000. Cf. Miller, Mark Crispin, 2001. *The Bush Dyslexicon,* London: Bantam Books. p.310.

Bibliography

Adorno, Theodor W. 1970. *Ästhetische Theorie.* Gesammelte Schriften, Bd 7 (Adorno och Tiedemann eds.). Frankfurt am Main: Suhrkampf.

Ahlström, T. 2001. Det döda kollektivet. Skelettmaterialet från Rössbergagånggriften, *Falbygdens gånggrifter. Undersökningar 1985-1998,* (Eds: Persson, Per & Sjögren, Karl-Göran), Serie GOTARC. 34 Göteborg: Institutionen för arkeologi.

Aijmer, Göran (ms). Landscape and mindscape in Southern China. The management of death in a mountain community.

Ajvide homepage. (latest update unknown). *Stenåldersboplatsen Ajvide, Gotland University*. [Electronic] Available: <http://www.hgo.se/ajvide>. [2003-04-05].

Alcock, S. E., Cherry, J.F. & Davis, J.L. 1994. Intensive Survey, Agricultural Practice and the Classical Landscape of Greece, in Morris, I. (ed.), *Classical Greece. Ancient Histories and Modern Archaeologies*, Cambridge: Cambridge University Press, pp.137-170.

Alcock, Susan 1993. *Graecia capta. The landscapes of Roman Greece.* Cambridge: Cambridge University Press.

Alcorn Jr, Marshall W. 1994. The subject of discourse: Reading Lacan through (and beyond) poststructuralist context, *Lacanian theory of discourse: subject, structure, and society,* New York: New York Univ. Press, cop. , ed: Bracher, Mark et al, pp.19-45.

Alekshin, V. A. 1983. Burial customs as an archaeological source, *Current anthropology vol 24, no 2,* pp.137-50.

Andrén, Anders 1997. *Mellan ting och text. En introduktion till de historiska arkeologierna.* Stockholm/Stehag: Brutus Östlings Bokförlag Symposion.

Antonaccio, Carla M. 1992, Terraces, Tombs, and the Early Argive Heraion, *Hesperia 61,* pp. 85-105.

Antonaccoi, Carla M. 1994. Contesting the past: Hero cult, tomb cult, and epic in early Greece, *American Journal of Archaeology 98,* pp. 389-410.

Archer, Margaret 1982. Morphogenesis versus structuration: on combining structure and action. *British Journal of Sociology Vol 33, No 4.* pp. 455- 483.

Artelius, Tore 2000. *Bortglömda föreställningar. Begravningsritual och begravningsplats i halländsk yngre järnålder.* Gotarc series B: 15/ RAÄ Ark. und. 36, Göteborg.

Augé, Marc 1995. *Non-places: introduction to an anthropology of supermodernity*, London: Verso.

Backe Forsberg, Y. 1978, Tiles from Graves B5 and B8, in Hägg, I., Hägg, R. & Backe Forsberg, Y. eds., *Excavations in the Barbouna Area at Asine 2: Finds From the Levendis Sector, 197072,* pp121-27.

Bailey, Douglass W. 1990. The Living House: Signifying Continuity, In: *The social archaeology of houses,* Ed. Ross Samson, pp.19-48.

Barker, Graeme 1991. Approaches to archaeological survey, *Roman landscapes: archaeological survey in the Mediterranean region,* edited by Graeme Barker and John lloyd, London: British School at Rome. pp.1-9.

Barrett, John 1994. *Fragments from Antiquity: an archaeology of social life in Britain, 2900-1200 BC.* Oxford: Blackwell.

Barrett, John 2001. Agency, the duality of structure, and the problem of the archaeological record. In: Hodder, I. (ed). *Archaeological Theory Today.* pp.141-164, Cambridge: Polity Press.

Barth, Fredrik 1992. Towards greater naturalism in conceptualizing societies. In: Kuper, A. (ed). *Conceptualizing Society,* London: Routledge, pp. 17-33.

Barth, Fredrik 2002. Towards a richer description and analysis of cultural phenomena, *Anthropology beyond culture,* Eds: King, Barbara J. & Fox, Richard G., Oxford: Berg, pp.23-36.

Bauman, Zygmunt 1992. *Death, immortality and other life strategies,* Cambridge: Polity Press.

Baumeister, Roy F. 1986. *ldentity. Cultural Change and the Struggle for Self*, New York: Oxford University Press.

Beck, Lane Anderson (ed) 1995. *Regional approaches to mortuary analysis,* New York: Plenum, cop.

Bender, Barbara (ed) 1993. *Landscape: Politics and Perspectives*, Oxford: Berg.

Benjamin, Walter 1955. *Gesammelte schriften I,* Frankfurt:thesis VI

Bennike, Pia 1985. *Palaeopathology of Danish skeletons. A comparative study of demography, disease and injury,* Copenhagen: Coronet Books.

Bérard, V. 1892. Tégée et la Tégéatide, *Bulletin de Correspondance Hellénique 16,* pp. 536-540.

Berger, Peter & Luckmann, Thomas 1966. *The Social Construction of Reality. A Treatise in the Sociology of Knowledge*, Harmondsworth: Penguin Books Ltd.

Berggren, Karin 2000. The knowledge-*able* agent? On the paradoxes of power, *Philosophy and Archaeological Practice. Perspectives for the 21st Century,* Eds: Cornelius Holtorf & Håkan Karlsson, Göteborg: Bricoleur Press, pp.39-46.

Bhabha, Homi K. 1994. *The Location of Culture.* London: Routledge.

Bhaskar, Roy 1979. *Philosophy and the human Vol. 1: The possibility of naturalism,* Brighton: Harvester Press.

Bigwood, Carol 1991. Renaturalizing the Body (with the Help of Merleau-Ponty), *Hypatia, vol. 6, no. 3 (Fall 1991),* pp.54-73.

Billington, Rosamund 1991. *Culture and society. A sociology of culture,* London: MacMillan.

Binford, Lewis 1968. Methodological considerations of the archaeological use of ethnographic data, *Man the hunter,* Eds: I. DeVore & R. B. Lee, Chicago: Aldine Publishing company, pp.268-273.

Binford, Lewis 1971. Mortuary practices. Their study and potential. In: Brown, James A. (ed). *Approches to the social dimensions of mortuary practices.* (Memoirs of the Society for American Archaeology no 25), *American Antiquity vol 36, No 3, Part 2,* pp. 6-29.

Binford, Lewis 1978. *Nunamiut Ethnoarchaeology.* New York: Academic Press.

Binford, Lewis 1982. The archaeology of place, *Journal of Anthropological Archaeology Vol 1, No 1,* pp.5-31.

Bintliff, John L. 1991a. The Annales School and archaeology, *The Annales School and archaeology,* Ed: Bintliff, John L., Leicester: Leicester University Press,

Bintliff, John L. 1991b. The Roman Countryside in Central Greece: Observations and Theories from the Boeotia Survey, in Barker, G. & Lloyd, J., *Roman Landscapes. Archaeological Survey in the Mediterranean Region,* London: British School at Rome, pp. 122-132.

Bintliff, John L. 2000. The concepts of 'site' and offsite' archaeology in surface artefact survey, *Non-destructive techniques applied to landscape archaeology,* Eds: Pasquinucci, Marinella & Trément, Frédéric, Oxford: Oxbow, pp. 200-15.

Bloch, Maurice 1982. death, Women and Power, *Death and the regeneration of life,* Eds: Parry, Jonathan P. & Bloch, Maurice, Cambridge: Cambridge University Press, pp. 211-30.

Bourdieu, Pierre 1977. *Outline of a theory of practice,* Cambridge: Cambridge Univ. Press.

Bourdieu, Pierre 1990. *The Logic of Practice,* Cambridge: Cambridge Univ. Press.

Bradley, Richard 1989. Darkness and light in the design of megalithic tombs, *Oxford journal of archaeology 8(1),* pp. 251-8

Bradley, Richard 1991. The pattern of change in British prehistory, *Chiefdoms: power, economy and ideology,* Ed: Earle, Timothy, Cambridge: Cambridge University Press, pp. 44-70.

Bradley, Richard 1993. *Altering the Earth.* Edinburgh: Society of Antiquaries of Scotland, Monograph Series no 8.

Bradley, Richard 1995. Trial and error in the study of mortuary practices – exploring the regional dimension. In: Anderson Beck, L. (ed). *Regional Approaches to Mortuary Analysis,* New York: Plenum Press, pp. v-ix.

Bradley, Richard 1998. *The significance of monuments: on the shaping of human experience in Neolithic and Bronze Age Europe,* London: Routledge.

Bradley, Richard 2000. *An archaeology of natural places*, New York: Routledge.

Bradley, Richard 2002. *The past in prehistoric societies,* London: Routledge.

Braidotti, Rosi 1991. *Patterns of Dissonance. A Study of Women in Contemporary Philosophy*, Cambridge: Polity Press.

Branigan, Keith 1998. The Nearness of You: Proximity and Distance in EarIy Minoan Funerary Landscapes, Ed: K. Branigan, *Cemetery and society in the Aegean Bronze Age,* Sheffield: Academic Press, pp.13-26.

Braudel, Fernand, 1979. *Civilisation matérielle, économie et capitalisme, Xve-XVIIIe siécle,* vols 1-3. Paris: Armand Colin.

Brück, Joanna 1998. In the footsteps of the ancestors: a review of Christopher Tilley's A Phenomenology of Landscape: Places, paths and monuments, *Archaeological review from Cambridge 15*, pp. 23-36.

Burenhult, Göran 1997a. Säljägare och svinherdar på Ajvide, *Ajvide och den moderna arkeologin,* ed: Göran Burenhult, Stockholm: Natur och kultur, pp.15-21.

Burenhult, Göran 1997b. Ajvide och den moderna arkeologin, *Ajvide och den moderna arkeologin,* ed: Göran Burenhult, Stockholm: Natur och kultur, pp.24-40.

Burenhult, Göran 1997c. Stenålder och digital arkeologi, *Ajvide och den moderna arkeologin,* ed: Göran Burenhult, Stockholm: Natur och kultur, pp. 41-51.

Burenhult, Göran 1997d. Gravarnas vittnesbörd, *Ajvide och den moderna arkeologin, ed: Göran Burenhult,* Stockholm: Natur och kultur, pp.52-70.

Burenhult, Göran 1997e. Stenålderns Ajvide – liv och död under 4000 år, *Ajvide och den moderna arkeologin,* ed: Göran Burenhult, Stockholm: Natur och kultur, pp172-186.

Burenhult, Göran 1997f. Introduction, *Remote sensing: applied techniques for the study of cultural resources and the localization, identification and documentation of sub-surface prehistoric remains in Swedish archaeology, Vol. 1, Osteo-anthropological, economic, environmental and technical analyses,* Ed: G. Burenhult, Tjörnarp, pp. ix-xxi.

Burenhult, Göran 2002. The grave-field at Ajvide, *Remote sensing: applied techniques for the study of cultural resources and the localization, identification and documentation of sub-surface prehistoric remains in Swedish archaeology. Vol. 2, Archaeological investigations, remote sensing case studies and osteo-anthropological studies,* ed: Göran Burenhult, Stockholm: Natur och kultur, pp.31-167.

Burkitt, Ian 1999. *Bodies of thought,* London: Sage.

Burnham, Barry C. & Kingsbury, John (eds) 1979. Introduction, In: *Space, hierarchy and society: interdisciplinary studies in social area analysis,* [third Cambridge interdisciplinary conference, from 10th to 12th March 1978], Oxford, British archaeological reports, International series, 59, pp.1-9.

Butler, Judith 1990a. Gender trouble, feminist theory, and psychoanalytic discourse, In: *Feminism/postmodernism,* Ed: Nicholson, Linda J., New York: Routledge, pp.324-40.

Butler, Judith 1990b. *Gender Trouble, feminism and the subversion of identity,* New York: Routledge.

Butler, Judith 1993. *Bodies that matter: on the discursive limits of "sex",* New York: Routledge.

Butler, Judith 1997. *The physic life of power. Theories in subjection,* Stanford: Stanford University Press.

Butzer, Karl W. 1982. *Archaeology as human ecology: method and theory for a contextual approach,* Cambridge: Cambridge University Press.

Börjesson, Bengt 1986. *Se människan: tio föreläsningar om Jean-Paul Sartres bok Varat och intet,* Stockholm: Rabén & Sjögren.

Callmer, C. 1943. *Studien zur Geschichte Arkadiens,* Lund: Gleerupska.

Callon, M. and Latour, B. 1981. Unscrewing the big Leviathan: how actors macrostructure reality and how sociologists help them to do so, *Advances in*

social theory and methodology. Toward an integration of micro- and macro-sociologies, Knorr- Cetina, K and Cicourel, A. V. (eds), Boston, London & Henley: Routledge & Kegan Paul, pp. 277-303.

Carr, Christopher & Jill E. Neitzel (eds.) 1995. *Style, Society and Person. Archaeological and Ethnological Perspectives.* New York & London: Plenum.

Carr, Christopher 1995. Mortuary Practices: Their Social, Philosophical-Religious, Circumstantial, and Physical Determinants, *Journal of Archaeological method and theory, Vol 2, No 2,* pp.105-200.

Castoriadis, Cornelius 1995. *Filosofi, politik, autonomi. Texter i urval,* Transl: Mats Olin, Stockholm/Stehag: Bruno Östlings bokförlag Symposion.

Cavanagh, W. & Mee, C. 1998. *A Private Place: Death in Prehistoric Greece* SIMA 124, Jonsered: Paul Åströms Förlag.

Cavanagh, W. et al. 1996. *The Laconia Survey. Continuity and Change in a Greek Rural Landscape* II: *Archaeological Data* BSA Suppl. 27.

Cavanagh, W. G. & Mee, C. 1990: The location of Mycenaean chamber tombs in the Argolid, in Hägg, R. & Nordquist, G. (eds.), *Celebrations of Death and Divinity in the Bronze Age Argolid, Proceedings of the Sixth International Symposium at the Swedish Institute at Athens, 11-13 June 1988 (SkrAth 4°, 40),* Stockholm, pp. 55-64.

Chapman, John 2000a. *Tension at funerals. Micro-tradition analysis in Later Hungarian Prehistory,* Budapest: Archaeolingua publications.

Chapman, John 2000b. *Fragmentation in archaeology: people, places, and broken objects in the prehistory of south-eastern Europe,* London: Routledge.

Chapman, R. 1987. Mortuary practices: society, theory building and archaeology, In: *Death, decay and reconstruction: approaches to archaeology and forensic science,* eds: Boddington, A., Garland, A. N. & Janaway, R. C., Manchester: Manchester University Press, cop.

Chapman, R. Kinnes, I. & Randsborg, K. (eds) 1981. *The archaeology of death,* Cambridge: Cambridge University Press.

Cherry, J. 1994. Regional survey in the Aegean: the new wave (and after), In *Beyond the site: regional studies in the Aegean area,* Eds: P. Kardulias, London: University Press of America, pp. 91-112.

Cherry, J.F., Davis, J.L. & Mantzourani, E. *The Nemea Valley Archaeological Project. Archaeological Survey.* [Internet Edition], Available at: <http://river.blg.uc.edu/nvap/> [2002-11-24].

Cherry, J.F., Davis, J.L., Mantzourani, E. 1991. *Landscape Archaeology as Long-term History. Northern Keos in the Cycladic Iislands,* Monumenta Archaeologica 16, Los Angeles: UCLA Institute of Archaeology.

Chodorow, Nancy 1978. *The reproduction of mothering: psychoanalysis and the sociology of gender, Berkeley* Univ. of Calif. P.

Cleary, Simon Esmonde 2000. Putting the dead in their place: burial location in Roman Britain, *Burial, society and context in the Roman world,* Eds: Pearce, John, Millett, Martin & Struck, Manuela, Oxford: Oxbow, pp.127-42.

Coleman, J. E. 1986. *Excavations at Pylos in Elis* (Hesperia Suppl. 21), Princeton (N.J.): American School of Classical Studies at Athens.

Collins, Randall 1981. Micro-translation as a theory-building strategy. In: Knorr-Cetina & Cicourel (eds). *Advances in Social Theory and Methodology. Toward an integration of micro- and macro-sociologies,* Boston & London, pp. 81-108.

Conein, Bernard 1998. La Notion de routine: probleme de definition, *Sociologie du Travail, 1998, 40, 4, Dec,* pp. 479-489.

Conkey, M & Hastorf, C. (eds) 1990. *The uses of style on archaeology,* Cambridge: Cambridge University Press.

Cornell, Per & Fahlander, Fredrik 2000. The subject of social epistemology, *Philosophy and Archaeological Practice. Perspectives for the 21st Century,* Eds: Cornelius Holtorf & Håkan Karlsson, Göteborg: Bricoleur Press, pp. 215-7.

Cornell, Per & Fahlander, Fredrik 2002a. *Social praktik och stumma monument. Introduktion till mikroarkeologi.* Gotarc serie C no 46, Göteborg: Institutionen för arkeologi.

Cornell, Per & Fahlander, Fredrik 2002b. Microarchaeology, Materiality and Social Practice, *Current Swedish Archaeology, vol 10, 2002 pp21-38.*

Cornell, Per & Medina, Maria-Clara 2001. El cuerpo como espacio social: notas sobre cadaveres públicos y privados. *Lo publico y lo privado: género en America Latina,* Göteborg: Red Haina/Instituto Iberoamericano, Universidad de Gotemburgo, pp. 193-211.

Cornell, Per 2000. Post-structuralism and 'Archaeology': Michel Foucault and Jacques Derrida. In: Holtorf, C. & Karlsson, K. (eds). *Philosophy and Archaeological Practice. Perspectives for the 21st Century,* Göteborg: Bricoleur Press, pp.173-184.

Cornell, Per 2001. La externalización de la memoria, la arqueología y el subalterno. *Anales, NS 3,* Göteborg: Instituto Iberoamericano, Göteborgs universitet.

Cosgrove, Dennis 1984. *Social Formation and symbolic landscape,* London & Sydney: Croom Helm.

Crawford, O. G. S. 1927. Barrows, *Antiquity, March 1927,* pp.419-434.

Criado Boado, Felipe & Villoch Vazques, Victoria 2000. Monumentalizing landscape: from present perception to the past meaning of Galician megalithicism (North-West Iberian Peninsula), *European Journal of Archaeology vol 3(2),* pp. 188-216.

Crown, P. L. & Fish, S. K. 1996. Gender and status in the Hohokam Pere-classic to classic transition, *Am. Anthropologist 98(4),* pp. 803-17.

Dalton, George 1981. Anthropological models in archaeological perspective, *Patterns of the past,* Eds: I. Hodder, G. Isaac & N. Hammond, Cambridge: Cambridge University Press, pp.17-48.

Davidson, Gladys R. 1952. *Corinth: results of excavations conducted by the American school of classical studies at Athens. Vol. 12, The minor objects,* Cambridge (Mass.): American school of classical studies at Athens.

Davis, Kathy 1997. 'Embody-ing Theory: Beyond Modernist and Postmodernist Readings of the Body', *Embodied Practices. Feminist perspectives on the body,* edited by Kathy Davis, London: Sage, pp.1-23.

Deleuze, G. & Guattari, F. 1988. *A thousand plateaus: capitalism and schizophrenia,* London: Athlone.

Descola, Philippe 1992. Societies of nature and the nature of society, *Conceptualizing society,* Ed: Adam Kuper, London & New York: Routledge, pp.17-33.

Dick-Bissonnette, Linda E. 1998. Gender and Authority among the Yokoch, Mono, and Miwok of Central California, *Journal of Anthropological Research, 1998, 54, 1, spring,* pp. 49-72.

Donley-Reid, Linda W. 1990. A structuring structure: the Swahili house, *Domestic Archaitecture and the use of space. An interdisciplinary cross-cultural study*, Ed: Susan Kent, Cambridge: Cambridge University Press, pp.114-126.

Drakopoulos, E. 1997. Asea in Arkadia. A Case of Site Abandonment and Population Movement, *L'Antiquité Classique 66,* pp.301-309.

Dugas, C. 1921. Le sanctuaire d'Aléa Athéna a Tégée, *Bulletin Correspondence Hellenique 45*, pp.335-435.

Durkheim, Émile, 1895[1985]. *The rules of sociological method,* Edited & transl by: Kenneth Thompson, London & New York: Routledge.

Earle, Timothy 1987. Chiefdoms in archaeological and ethnohistorical perspectives. *Ann Rew. of Anthropology 16*, pp.279-308.

Earle, Timothy 1991. The evolution of chiefdoms, In: *Chiefdoms: power, economy, and ideology,* (Ed: Earle, Timothy), Cambridge: Cambridge University Press.

Eberhardt, Isabelle 1990. *Écrits sur le sable, vol II,* Eds: M-O Delacour & J-R Huleu, Paris: Bernard Grasset.

Ekholm, Kajsa, 1981. On the structure and dynamics of global systems, *The anthropology of pre-capitalist societies,* Eds: J. S. Joel & J. R. Llobera, London: MacMillan Press Ltd, pp.241- 261.

Elias, Norbert 1998. *On civilization, power, and knowledge*, Eds: Stephen Mennell & Johan Goudsblom, Chicago: Univ. of Chicago Press.

Elkana, Yehuda 1981. Sciences and cultures: anthropological and historical studies of the sciences, *Sciences and cultures. Sociology of the sciences vol 5*, Ed. by Everett Mendelsohn and Yehuda Elkana, Dordrecht: Reidel.

Fahlander, Fredrik (ms). Archaeology and anthropology – brothers in arms? On analogies in 21st century archaeology, *Material culture and other things,* Eds: F. Fahlander & T. Östigård, *in press.* [Available at: <http://hem.fyristorg.com/archaeology/MA/analogi.htm>

Fahlander, Fredrik 2001. *Archaeology as Science Fiction. A microarchaeology of the unknown,* Gotarc Serie C:43, Göteborg: Göteborgs universitet.

Fahlander, Fredrik 2002. Mortuary Data: Structural Relations Between Burials, Settlements and the Landscape. In: Forsén, J. & Forsén, B. (eds). *The Asea Valley Survey. An Arcadian Mountain Valley from the Palaeolithic Period until Modern Times*, (SkrAth-4°, LI), Jonsered: Paul Åströms förlag. Pp. 581-594.

Fairclough, Graham 1992. Meaningful constructions - spatial and functional analysis of medieval buildings, *Antiquity, no 66*, pp. 348-366.

Featherstone, Mike, Hepworth, Mike & Turner, Bryan S. 1991. *The body: social process and cultural theory,* edited by Mike Featherstone, Mike Hepworth & Bryan S. Turner, London: Sage.

Feldstein, R., Fink, B. & Jaanus, M. 1996. *Reading seminars I and II. Lacans return to Freud*, Eds: R. Feldstein, B. Fink, M. Jaanus, New York: State university of New York press.

Forsell, Renée 1996. The Roman period, In: Wells, B. (ed.), *The Berbati-Limnes Archaeological Survey 1988-1990* (SkrAth-4°, 44), Stockholm, pp.285-343.

Forsell, Renée 2001. *The Argolid under Roman rule (31 BC - AD 600),* Lund: Lund university.

Forsén, B. & Karieri, A. 2002. The Roman – Early modern periods – conclusions, *The Asea Valley Survey. An Arcadian Mountain Valley from the Palaeolithic Period until Modern Times*, (SkrAth-4°, LI), Forsén, J. & Forsén, B. (eds), Jonsered: Paul Åströms förlag. pp.491-540.

Forsén, Björn 2002. The road network of the valley, *The Asea Valley Survey. An Arcadian Mountain Valley from the Palaeolithic Period until Modern Times*, (SkrAth-4°, LI), Forsén, J. & Forsén, B. (eds), Jonsered: Paul Åströms förlag, pp.79-103.

Forsén, J. & Forsén, B. (Eds) 2002. *The Asea Valley Survey. An Arcadian Mountain Valley from the Palaeolithic Period until Modern Times,* (SkrAth-4°, LI), Jonsered: Paul Åströms förlag.

Forsén, J. & Forsén, B. 1997. The *polis* of Asea - A Case-Study of How Archaeology Can Expand Our Knowledge of the History of a *polis*, *Papers from the Copenhagen Polis Centre* 4 (Historia Einzelschriften 117), Stuttgart, pp.163-176.

Forsén, J., Forsén B. & Østby, E. 1999. The Sanctuary of Agios Elias - Its Significance and Relations to Surrounding Sanctuaries and Settlement Sites. Some Preliminary Reflections, in: T.H. Nielsen and J. Roy (eds), *Defining Ancient Arcadia* (CPCActs 6), pp.169-91.

Forsén, J., Forsén, B. and Karlsson, Lars 2002. The walls of Asea, *Opuscula Atheniensia 27,* pp. 83-104.

Forsén, J., Forsén, B., & Lavento, M. 1996. The Asea valley survey - a preliminary report of the 1994 season. *Opuscula Atheniensia XXI:5.* Göteborg, pp.73-97.

Foucault, Michel 1972. *The Archaeology of Knowledge,* Trans. A. Sheridan, London: Tavistock.

Foucault, Michel 1979. *Discipline and Punish. The birth of the prison.* Harmondsworth: Penguin.

Foucault, Michel 1980. *Power, Knowledge: selected interviews and other writings 1972-1977,* Ed. and transl. by Colin Gordon, New York: Pantheon cop.

Foucault, Michel 1981. *The History of Sexuality I. An introduction,* Harmondsworth: Penguin.

Foucault, Michel 1986. Of other spaces, *Diacritics: a review of contemporary criticism,* pp.22-27.

Foucault, Michel 1989. *Madness and civilization: a history of insanity in the age of reason,* London: Routledge.

Foucault, Michel 1990. *History of Sexuality III: The Care of the Self,* London: Random House.

Foucault, Michel 1994, *The Birth of the Clinic. An archaeology of medical perception,* transl. A.M. Sheridan Smith, New York: Vintage books

Foxhall, Lin 1996. Feeling the earth move: cultivation techniques on steep slopes in classical antiquity, Human landscapes in classical antiquity: environment and culture, Eds: J. Salmon & G. Shipley, London: Routledge, pp. 44-65.

Freeman, L. G. 1968. A theoretical framework for interpreting archaeological materials, *Man the hunter,* Eds: I. DeVore & R. B. Lee, Chicago: Aldine Publishing company, pp. 262- 267.

Fried, M. 1967. *The evolution of political society,* New York: Random house.

Friedman, Jonathan 1994. *Cultural Identity and Global Process,* London: SAGE Publ.

Fuchs, Werner 1969. *Todesbilder in der modernen Gesellschaft,* Frankfurt am Main: Suhrkamp Verlag.

Gansum, Terje, 2002. Fra jord till handling, *Plats och praxis: studier av nordisk förkristen ritual,* eds: Jennbert, Kristina, Andrén, Anders & Raudvere, Catharina, Lund: Nordic Academic Press, pp. 249-286.

Gansum, Terje, Jerpsen, Gro B. & Keller, Christian 1997. A*rkeologisk landskapsanalyse med visuelle metoder,* AMS- Varia 28, Stavanger: Arkeologiskt Museum i Stavanger.

Garfinkel, H. 1967. *Studies in ethnomethodology,* Englewood Cliffs, N.J.: Prentice-Hall.

Gates, Charles 1983. *From cremation to inhumation: Burial Practices at Ialysos and Kameiros During the Mid-Archaic Period, ca. 625-525 B.C.,* Occasional papers 11, Los Angeles: University of California.

Geertz, Clifford 1973. *The interpretation of cultures,* New York: Basic Books lnc.

Geertz, Clifford 2000. *Available light: Anthropological reflections on philosophical topics,* Princeton (N.J.): Princeton Univ. Press.

Gellner, Ernest 1985. *Relativism and the social sciences,* Cambridge: Cambridge University Press.

Gero, Joan 2000. Troubled Travels in Agency and Feminism, *Agency in Archaeology,* Eds: Marcia-Anne Dobres & John E. Robb, London & New York: Routledge, pp.34-39.

Giddens, Anthony 1979. *Central problems in social theory: action, structure and contradiction in social analysis,* London: MacMillan.

Giddens, Anthony 1984. *The Constitution of Society,* Cambridge: Polity Press.

Giddens, Anthony 1985. Time, space and regionalisation, *Social relations and spatial structures,* Ed: J. Urry, London: MacMillan, pp. 265-287.

Giddens, Anthony 1993 [1976]. *New rules of sociological method. A positive critique of interpretative sociologies,* Cambridge: Polity Press.

Gilchrist, Roberta 1999. *Gender and archaeology: contesting the past,* London: Routledge.

Goffman, Erving 1967. *Interaction Ritual.* New York: Doubleday.

Goffman, Erving 1971. *The Presentation of Self in Everyday Life.* Harmondsworth: Penguin books.

Goffman, Erving 1974. *Frame Analysis*. New York.

Goldstein, Lynne 1981. One-dimesional archaeology and multidimensional people: spatial organization and mortuary analysis, In: *The archaeology of death,* Eds: R. Chapman, I. Kinnes & K. Randsborg, Cambridge: Cambridge University Press, pp. 53-69.

Goodenough, William H. 1961. Comment on cultural evolution. *Daidalus, 90,* pp. 521-528.

Gosden, Chris 1994. *Social Being and Time.* Oxford: Blackwell.

Gottdiener, Mark 1995. *Postmodern semiotics: material culture and the forms of postmodern life,* Cambridge (Mass.): Blackwell.

Graves-Brown, P.M (ed) 2000. *Matter, Materiality, and Modern Culture,* London: Routledge.

Grimm, Linda 2000. Apprentice flintnapping, In: *Children and material culture,* Ed: Sofaer Deverenski, Joanna, London: Routledge, pp.53-71.

Götherström, Anders 2001. *Acquired or inherited prestige?: molecular studies of family structures and local horses in Central Svealand during the Early Medieval period,* Stockholm: Archaeological Research Laboratory.

Habermas, Jurgen 1972. *Knowledge and human interests,* Boston: Beacon Press.

Hahn, M. 1996. The Early Byzantine to Modern periods, In Wells, B. (ed.), *The Berbati-Limnes Archaeological Survey 1998-1990* (SkrAth 4º, XLIV), Stockholm, pp. 345-451.

Hall, R. L. 1976. Ghosts, Water Barriers, Corn and Sacred Enclosures in the Eastern Woodlands, *American Antiquity 41,* pp.360-364.

Haraway, Donna J. 1989. *Primate visions: gender, race and nature in the world of modern science,* London: Routledge.

Haraway, Donna J. 1991. *Simians, Cyborgs, and Women: the reinvention of nature.* London: Free Association Books.

Harris, Edward C. 1979. *Principles of Archaeological Stratigraphy.* London: Academic Press.

Harris, Marvin 1990. *Emics and ethics: the insider/outsider debate,* Ed: T. N. Headland, K. L. Pike & M. Harris, London: Sage.

Hedeager, Lotte 1990. *Danmarks jernalder: mellem stamme og stat,* Århus: Aarhus Universitetsforlag.

Heimann, Curry 2002. Neolitisering i Västvärmland, *In Situ 2000-2001,* pp.27-55.

Hertz, Robert 1960. *Death and the Right Hand,* (trans. R. & C Needham), Glencoe, Illinois: The Free Press

Higate, Paul 1998. The body resists. Everyday clerking and unmilitary practice, *The Body in Everyday Life,* Eds: Sarah Nettleton and Jonathan Watson, London & New York: Routledge.

Hobsbawn, E 1979. An historian's comments, In: *Space, hierarchy and society : interdisciplinary studies in social area analysis,* Eds: Burnham, Barry C. & Kingsbury, John, Oxford: British archaeological reports. International series 59, pp247-52.

Hodder, Ian & Orton, Clive 1976. *Spatial analysis in archaeology,* Cambridge: Cambridge University Press.

Hodder, Ian 1982. *Symbols in action,* Cambridge: Cambridge University Press.

Hodder, Ian 1984. Burials, houses, women and men in the European Neolithic, In D. Miller & C. Tilley (Eds), *Ideology, Power and prehistory,* Cambridge: Cambridge University Press, pp. 51-68.

Hodder, Ian 1987. The contextual analysis of contextual meanings, In I. Hodder (ed) *The archaeology of contextual meanings*, Cambridge: Cambridge University Press, pp1-10.

Hodder, Ian 1990. *The domestication of Europe: structure and contingency in Neolithic societies,* Oxford: Basil Blackwell,

Hodder, Ian 1991. *Reading the past,* (2:nd edition), Cambridge: Cambridge University Press.

Hodder, Ian 1992. *Theory and practice in archaeology,* New York: Routledge.

Hodder, Ian 1994. Architecture and meaning: the example of Neolithic houses and tombs, In: *Architecture and order: approaches to social space,* Eds: Parker Pearson, Michael & Richards, Colin, New York: Routledge, cop., pp.73-86.

Hodder, Ian 1999. *The Archaeological Process: An Introduction,* Oxford: Blackwell Publishers.

Holmberg, E.J. 1941. *A Doric Temple near Asea in Arcadia,* Göteborg: Göteborgs Högskolas Årsskrifter 48.

Holmberg, Erik J. 1944. *The Swedish Excavations at Asea in Arcadia.* Leipzig: ACTA Instituti Romani Regni Sueciae XI.

Horai, S (et al). 1989. DNA amplification from ancient human skeletal remains and their sequence analysis. *Proceedings of the Japan Academy, Series B - Physical and Biological Sciences. Vol 65,* pp. 229-233.

Horden, Peregrine & Purcell, Nicholas 2000. *The corrupting sea: a study of Mediterranean history,* Oxford: Blackwell,

Hornborg, Alf 1997. Landskap som 'text'. Några funderingar kring ett stycke svensk skärgård, *Moderna landskap. Identifikation och tradition i vardagen,* Eds: Saltzman, Katarina & Svensson, Birgitta, Stockholm: Natur och kultur, pp.207-19.

Houby-Nielsen, Sanne 1995. 'burial language' in archaic and classical Kerameikos *Proceedings of the Danish Institute at Athens 1,* pp129-91.

Houby-Nielsen, Sanne 2000. Child burials in ancient Athens, *Children and material culture,* Ed: Sofaer Deverenski, Joanna, ort: förlag, pp.151-66.

Huntington, Richard & Metcalf, Peter 1991. *Celebrations of death: the anthropology of mortuary ritual (2nd ed),* Cambridge: Cambridge University Press.

Hurt, T. D. & Rakita, G. F. M. (eds) 2001. *Style and function: conceptual issues in evolutionary archaeology,* Westport (Conn.): Bergin & Garvey.

Hårdh, Birgitta & Roslund, Curt 1991. Passage graves and the passage of the moon, Regions and reflections. In honour of Märta Strömberg, Eds: K. Jennbert, L. Larsson, R. Petré & B. Wyszomirska-Werbart, Lund: Almqvist & Wiksell Int., pp.35-43.

Hägerstrand, Torsten 1970. What about people in regional science?, *Papers of the regional science association no 23,* pp.7-21.

Hägerstrand, Torsten 1985. Time-geography: Focus on the Corporeality of Man, In: *Society and Environment. The Science and Praxis,* The United Nations University, pp.193-216.

Hägg, R., Hägg, I. & Fossey, J.M. 1980. *The Hellenistic Nekropolis and Later Structures on the Middle Slopes, 197377* Excavations in the Barbouna area at Asine 4, Uppsala: Uppsala universitet.

Härke, Heinrich 2002. Interdisciplinarity and the archaeological study of death, *Mortality,Vol.7,No.3,* pp340-1

Iacovou, Maria 2001. Survey archaeology and its scientific potentials: not a panacea but a step in the right direction, In: *Quo vadis archaeologia?: whither European archaeology in the 21st century?,* Ed: Zbigniew Kobylinski, Warszawa: Institute of Archaeology and Ethnology, pp. 136-148.

Ingold, Tim 1992. Culture and the perception of the environment, *Bush base: forest farm Culture, environment and development,* Edited by Elisabeth Croll and David Parkin, London & New York: Routledge, pp. 39-56.

Ingold, Tim 1993. The Temporality of the Landscape. *World Archaeology 25:2*, Cambridge: Routledge, pp. 152-174.

Ingold, Tim 1996. Hunting and Gathering as Ways of Perceiving the Environment, Redefining Nature. *Ecology. Culture and Domestication,* Edited by Roy Ellen and Katsuyoshi Fukui, Oxford: Berg.

Irigaray, Luce 1994. *Könsskillnadens etik och andra texter,* övers. Christina Angelfors, Stockholm: Symposion AB.

Jameson, M. H., Runnels, C. N. & van Andel, T. H. 1994. *A Greek Countryside: The Southern Argolid from Prehistory to the Present Day*, Stanford: Stanford Univ. Press.

Janzon, Gunborg 1974. *Gotlands mellanneolitiska gravar,* ACTA Universitatis Stockholmiensis, no 6, Stockholm: Almqvist & Wiksell, pp.1-140.

Jenks, Chris 1993. *Culture,* London & New York: Routledge

Jensen, Claus & Høilund Nielsen, Karen (eds) 1997. Introduction - towards a Danish burial archaeology?, *Burial and society: the chronological and social analysis of archaeological burial data,* Aarhus: Aarhus Universitetsforlag, pp. 9-15.

Johansson, N. 1996. *Burials and Society: a study of social differentiation at the site of El Pichao, north-western Argentina, and in cemeteries dated to the Spanish native period.* Gotarc, Series B: 5, Göteborg: Göteborg university.

Johnsen, B. & Welinder, S. (eds) 1995. *Arkeologi om barn,* Uppsala: Societas archaeologica Upsaliensis.

Johnston, Robert 1998. Approaches to the perception of landscape. Philosophy, theory, methodology, *Archaeological Dialogues July 1998 Vol. 5 nr. 1,* pp.54-68.

Jones, Andrew 2002. *Archaeological theory and scientific practice,* Cambridge: Cambridge University Press.

Joyce, R. A. 2000. Girling the girl and boying the boy: the production of adulthood in ancient mesoamerica, *World Archaeology vol 31(3),* pp.473-83.

Kaelas, Lili 1976. *Pitted ware culture: the acculturation of a food-gathering group?,* Brugge: De Tempel, Reprinted from *Dissertationes Archaeologicae Gandenses,* Vol. XVI, pp. 130-141.

Kaliff, Anders. 1992. *Brandgravsskick och föreställningsvärld. En Religions-arkeologisk diskussion.* Occasional Papers in Archaeology 4, Societas Archaeologica Upsaliensis, Uppsala: Uppsala university.

Kirk, G. S. (ed) 1954. *Heraclitus. The cosmic fragments,* Cambridge: Cambridge University Press.

Kirk, Trevor 1991. Structure, Agency, and Power Relations, *Processual and postprocessual Archaeologies. Multiple Ways of Knowing the Past,* Ed: R. W. Preucel, University at Carbondale Occasional Paper No. 10, Ort: förlag, pp.108-125.

Klindt-Jensen, O. 1975. *A History of Scandinavian Archaeology,* London: Thames & Hudson.

Komter, Aafke 1998. Dingen als spiegel van relaties, *Sociologische Gids, 1998, 45, 4, July-Aug,* pp. 234-246.

Kripke, Saul A. 1980. *Naming and necessity,* Oxford: Blackwell.

Kristiansen, Kristian 1985. PostDeposional Formation Processes and the Archaeological Record — a Danish Perspective", in *Archaeological Formation Processes: The Representativity of Archaeological Remains from Danish Prehistory,* Ed: K. Kristiansen, København: Nationalmuseum, pp. 7-11.

Kristiansen, Kristian 2002. The tale of the sword - swords and swordfighters in Bronze Age Europe, *Oxford Journal of Archaeology. Vol 21 No. 4,* pp.319-332.

Kroeber, Alfred L. 1927. Disposal of the Dead, *American Anthropologist 29,* pp.308-315.

Kroeber, Alfred L. 1952. *The nature of culture,* Chicago: University of Chicago press.

Kurtz, D.C. & Boardman, J. 1971. *Greek Burial customs,* London: Thames & Hudson.

Kyle, Donald G.1998. *Spectacles of death in Ancient Rome,* London: Routledge

Lacan, Jacques 1972[1949]. Spegelstadiet som utformare av jagets funktion, *Psykologi i teori och praktik nr 4,* Trans: Erik Graffman, pp.38-46.

Lacan, Jacques 1977a[1966]. *Écrits. A selection,* Trans: Alan Sheridan, Ort: Tavistock Publications.

Lacan, Jacques 1977b[1973]. *The four fundamental concepts of psycho-analysis,* ed: Jacques-Alain Miller, transl: Alan sheridan, London: Penguin Group.

Lacan, Jacques 1988a. *The seminars of Jacques Lacan, Book I,* Ed: Jaques-Alain Millner, New York: W. W. Norton & Co.

Lacan, Jacques 1988b. *The seminar of Jacques Lacan. Book II,* Ed: Jaques-Alain Millner, Ort: Tavistock Publications.

Lacan, Jacques 1995[1951]. Några reflektioner över jaget, *Divan no 3,* Trans: Jurgen Reeder, pp. 38-46.

Lacan, Jacques 1996[1966]. *Écrits. Spegelstadiet och andra skrifter i urval av Iréne Matthis,* Transl: Iréne Matthis et al, Stockholm: Natur och Kultur.

Laclau, Ernesto, 1989. Preface, In: Slavoj Zizek's *The sublime object of ideology,* transl. Jon Barnes., London: Verso, pp. ix-xv.

Lagerlöf, Agneta 1991. *Gravfältsundersökningar och gravarkeologi,* rapport från Riksantikvarieämbetets seminarium om gravmaterialet som källa för kunskap om människans livsvillkor, religiösa och sociala värderingar: 26-27 oktober 1988, Red: Agneta Lagerlöf, Stockholm: RAÄ.

Lamo de Espinosa, Emilio 2002. Lengua, nation y estado, *Claves de razón práctica (2002),* pp.14-22.

LaMotta, V. M. & Schiffer, M. B. 2001. Behavioural archaeology: Toward a new synthesis, In I. Hodder (ed) *Archaeological theory today,* Cambridge: Polity Press, pp. 14-64.

Larsson, M., Olsson, E. & Biwall, A. (eds) 1997. *Regionalt och interregionalt: stenåldersundersökningar i Syd- och Mellansverige,* Stockholm: Avd. för arkeologiska undersökningar, RAÄ.

Latour, Bruno 1991. Technology is Society Made Durable. *The Sociological Review Monograph, 1991, 38,* pp.103-131.

Latour, Bruno 1992. Where are the Missing Masses? The Sociology of a Few Mundane Artefacts. *Shaping Technology/Building Society. Studies in Sociotechnical Change* (Bijker & Law eds.), Cambridge (Mass.): The MIT Press, pp. 225-258.

Latour, Bruno 1998. *Artefaktens återkomst: ett möte mellan organisationsteori och tingens sociologi,* trans: Elisabeth Wennerholm, Stockholm: Nerenius & Santérus

Leach, Edmund 1961. Rethinking anthropology, *Rethinking Anthropology,* Ed: E. R. Leach, London: Athlone Press, pp.1-29

Leach, Edmund 1979. Discussion, In: *Space, hierarchy and society: interdisciplinary studies in social area analysis,* [third Cambridge interdisciplinary conference, from 10th to 12th March 1978], Burnham, Barry C. & Kingsbury, John (eds), Oxford: British archaeological reports. International series 59, pp.119-24.

Leach, Edmund 1989. Tribal ethnography: past, present, future. In: Tonkin, Donald & Chapman (eds). *History and Ethnicit,* London & New York: Routledge, pp. 34-47.

Lekberg, Per 2002. *Yxors liv: människors landskap: en studie av kulturlandskap och samhälle i Mellansveriges senneolitikum,* Uppsala: Department of Archaeology and Ancient History.

Lesser, Alexander 1961. Social fields and the evolution of society, *Southwestern journal of anthropology no17,* pp. 40-48.

Levi, Giovanni 1991. On Microhistory. *New Perspectives on Historical Writing* (Burke, P. ed.). Cambridge: Polity Press, pp. 93-113.

Levi, Giovanni 1998. The origins of the modern state and the microhistorical perspective, In: *Mikrogeschichte - Makrogeschichte: komplementär oder inkommensurabel?*, eds: Gribaudi, Maurizio, Levi, Giovanni, Tilly, Charles & Schlumbohm, Jürgen, Göttingen: Wallstein-Verl., pp.53-82.

Levi-Strauss, Claude 1963. *Structural Anthropology,* Anchor Books: New York.

Lévi-Strauss, Claude 1966. *The Savage Mind,* London: University of Chicago Press.

Lillehammer, Grete 2000. The world of Children, *Children and material culture,* Ed: Sofaer Deverenski, Joanna, pp.17-26.

Lindquist, Sune 1926. *Vendelkulturens ålder och ursprung,* Stockholm: Akademiens Förlag.

Lindqvist, Christian & Possnert, Göran 1997a. Om reservoareffektproblemet, *Ajvide och den moderna arkeologin,* ed: Göran Burenhult, Stockholm: Natur och kultur, pp. 73-74.

Lindqvist, Christian & Possnert, Göran 1997b. The subsistence economy and diet at Jacobs/Ajvide and Stora Förvar, Eksta parish and other prehistoric dwelling and burial sites on Gotland in long-term perspective, *Remote sensing: applied techniques for the study of cultural resources and the localization, identification and documentation of sub-surface prehistoric remains in Swedish archaeology*, Vol. 1, Osteo-anthropological, economic, environmental and technical analyses, Ed: G. Burenhult, Tjörnarp, pp.29-90.

Linton, Ralph 1936. *The Study of Man. An introduction.* New York: Appleton-Century-Crofts Inc.

Luhmann, Niklas 1995. *Social systems,* Transl. J. Bednarz, with D. Baecker, Stanford: Stanford University Press.

Lynch, Kevin 1960. The image of the city, Cambridge [Mass.]: M.I.T. Press & Harvard Univ. Press,

Löfgren, Orvar 1997. Mellanrum, vita fläckar och svarta hål i storstadens utkant, *Moderna landskap. Identifikation och tradition i vardagen,* Eds: Saltzman, Katarina & Svensson, Birgitta, Stockholm: Natur och kultur, pp.45-69.

MacKenzie, Maureen 1991. *Androgynous Objects. String Bags and Gender in Central New Guinea.* Philadelphia: Harwood Academic Publishers.

Macnaghten, P. & Urry, J. 1998. *Contested Natures,* London: Sage.

Madsen, T. & Jensen, H. J. 1982. Settlement and Land Use in Early Neolithic Denmark: A Regional Study of Change, *Analecta Praehistorica Liedensia* 15, pp. 63-86.

Malinowski, Bronislav 1939. The group and the individual in functional analysis, *American Journal of Sociology, vol 44,* pp. 938-64. Also in: Bohannan, P., and Glazer, M. 1988. *High Points in Anthropology,* New York: McGraw-Hill, pp. 275-293.

Malmer, Mats P. 1975. *Stridsyxekulturen i Sverige och Norge,* Lund: Liber.

Malmer, Mats P. 2002. *The Neolithic of South Sweden. TRB, GRK, and STR.* Stockholm: Kungliga Vitterhets-, Historie- och Antikvitets Akademien.

Manchester, Keith 1989. Burial Archaeology: the state of the art past, present and future, *Burial Archaeology Current Research, Methods and Developments,* Eds: C. A. Roberts, F. Lee & J. Bintliff, BAR 211, pp. 5-14.

Mann, Thomas 1991. *Der Zauberberg,* Frankfurt am Main: Fischer verlag

Meskell, Lynn 1999. *Archaeologies of SociaI Life. Age, Sex, Class et cetera in Ancient Egypt,* Ort: Blackwell.

Miller, Jacques-Alain 1996. Jacques Lacan, *Écrits. Spegelstadiet och andra skrifter i urval av Iréne Matthis,* Stockholm: Natur och Kultur.

Molnar Appelblom 1997. Arbete, förslitningar och könsroller – om osteobiografins möjligheter att tolka forntida livssituationer, *Ajvide och den moderna arkeologin,* ed: Göran Burenhult, Stockholm: Natur och kultur, pp. 85-94.

Montelius, O. 1874. *Sveriges Forntid. vol I: Stenåldern,* Stockholm: P. A. Nordströms förlag.

Montelius, Oscar 1884. Den förhistoriska fornforskarens metod och material, *Antiquarisk Tidskrift för Sverige Del 8 Nr 3,* Stockholm.

Montelius, Oscar 1909. De Mykeniska kupolgrafvarna och de nordiska gånggrifterna, *Fornvännen 4,* pp.161-175.

Moore, Henrietta 1994. *A passion for difference: essays in anthropology and gender,* Cambridge: Polity press.

Moraïtis, Nikolaou D. 1832. *Ηστορια τισ Τεγεασ,* Athens.

Morris, Ian 1987. *Burial and Ancient Society. The rise of the Greek city-state.* Cambridge: Cambridge University Press.

Morris, Ian 1988. Tomb Cult and the 'Greek Renaissance': The Past in the Present in the 8th Century BC, *Antiquity 62,* pp750-761.

Morris, Ian 1992. *Death Ritual and Social Structure in Classical Antiquity,* Cambridge: Cambridge University Press.

Morris, Ian 1998. Burial and society after ten years, In: *Nécropoles et pouvoir: idéologies, pratiques et interprétations: actes du colloque Théories de la nécropole antique,* Lyon 21-25 janvier 1995, Eds: Sophie Marchegay, Marie-Thérèse Le Dinahet & Jean-François Salles, Lyon: Maison de l'Orient méditerranéen, pp. 21-36.

Morris, Ian 2000. *Archaeology as cultural history. Words and thing in Iron Age Greece,* Oxford: Blackwell Publishers

Mouzelis, N. 1989. Restructuring Structuration Theory, *Sociological Review 37,* pp.617-35.

Muller, J. 2002. Zur Belegungsabfolge des Gräberfeldes von Trebur: Argumente der typologieunabhängigen Datierungen, *Prähistorischen Zeitschrift 77, 2002,* pp148-158.

Nihlen, John 1927. *Gotlands stenåldersboplatser,* Stockholm: Victor Petterson

O'Shea, John M. 1984. *Mortuary Variability. An Archaeological Investigation.* Orlando: Academic Press.

O'Shea, John M. 1996. Villagers of the Maros: a Portrait of an Early Bronze Age Society, (Review Feature), Cambridge *archeological journal vol8, no1,* London: Plenum Press, pp. 95-111.

Okely, J. 1979. An anthropological contribution to the history and archaeology of an ethnic group, In: *Space, hierarchy and society: interdisciplinary studies in social area analysis:* [third Cambridge interdisciplinary conference, from 10th to 12th

March 1978], Oxford: British archaeological reports. International series 59, pp. 81-92.

Olsen, Bjørnar J. 1997. *Fra ting til tekst. Teoretiske perspektiv i arkeologisk forskning,* Oslo: Universitetsforlaget.

Osbourne, Robin 1996. *Greece in the making 1200-479 BC,* London: Routledge.

Pader, Ellen-Jane 1982. *Symbolism, Social Relations and the Interpretation of Mortuary Remains.* Oxford: British archaeological reports. International series 130.

Papadopoulous, J. 1993. To kill a cemetery: the Athenian Kerameikos and the Early Iron Age in the Aegean, *Journal of Mediterranean Archaeology 6,* pp.175-206.

Parker Pearson, Michael & Richards, Colin 1994. *Architecture and order: approaches to social space,* London & New York: Routledge.

Parker Pearson, Michael 1982. Mortuary practices, society and ideology: an ethnoarchaeological study. In: Hodder, I. (Ed). *Symbolic and Structural Archaeology,* Cambridge: Cambridge University Press, pp. 99-113.

Parker Pearson, Michael 1993. The Powerful Dead: Archaeological Relationships between the Living and the Dead, *Cambridge Archaeological journal 3:2,* pp. 203-229.

Parker Pearson, Michael 1999. *The Archaeology of Death and Burial*, College Station: Texas A&M University Press.

Parsons, Talcott 1951. *The Social System,* London: The Free Press.

Pearce, Susan M. 1994. Thinking about Things, *Interpreting Objects and Collections,* Ed: S. Pearce, London: Routledge, pp. 125-132.

Peet, Richard 1998. *Modern geographical thought,* Oxford: Blackwell Publishers

Persson A.W. 1942. *New tombs at Dendra*, Lund.

Persson, Per & Sjögren, Karl-Göran 2001. *Falbygdens gånggrifter. Undersökningar 1985-1998,* (Eds: Persson, Per & Sjögren, Karl-Göran), Serie GOTARC. 34 Göteborg: Institutionen för arkeologi.

Pikolas, Y. A. 1988. *Η Νοτια Μεγαλοπολιτικη χωρα.* Αθηναι· Η Μεγαλη Βιβλιοθηκη.

Plog, Stephen 1980. *Stylistic Variation in Prehistoric Ceramics. Design analysis in the American Southwest.* Cambridge: Cambridge University Press.

Possnert Göran 2002. Stable and radiometric carbon results from Ajvide, *Remote sensing: applied techniques for the study of cultural resources and the localization, identification and documentation of sub-surface prehistoric remains in Swedish archaeology.* Vol. 2, Archaeological investigations, remote sensing case studies and osteo-anthropological studies, Ed: Göran Burenhult, Stockholm: Natur och kultur, pp. 169-72.

Poulsen, E. 1994. *Asine II: Results of the Excavations East of the Acropolis 1970-1974, Fasc. 6. The Protogeometric Periods 2: The Postgeometric Settlement Material and Tombs from the Hellenistic Period* (SkrAth 4°, 24:6.2), Stockholm.

Preda, Alex 1999. The Turn to Things: Arguments for a Sociological Theory of Things, *The Sociological Quarterly, 1999, 40, 2, spring,* pp.347-366.

Purcell, Nicholas 1995. The Roman villa and the landscape of production, *Urban society in Roman Italy,* Eds: Cornell, Tim & Lomas, Kathryn, London: UCL Press, pp.151-79.

Rapoport, Amos 1990. Systems of activities and systems of settings, *Domestic Architecture and the use of space. An interdisciplinary cross-cultural study,* Ed: Susan Kent, Cambridge: Cambridge University Press, pp.9-33.

Rathje, William & Cullen, Murphy 1992. *Rubbish!: the archaeology of garbage.* New York: Harper Collins.

Rathje, William 1974. The Garbage Project: A new way of looking at the problems of archaeology. *Archaeology 27,* pp. 236-41.

Rathje, William 1981. A Manifesto for Modern Material Culture Studies. *In Modern Material Culture: The Archaeology of Us,* Eds: Richard A. Gould and Michael B. Schiffer, New York: Academic Press, pp. 51-56.

Renfrew, Colin & Wagstaff, J. Malcolm 1982. *An island polity: the archaeology of exploitation in Melos,* Cambridge: Cambridge University Press.

Renfrew, Colin 1984. *Approaches to Social Archaeology.* Edinburgh: Edinburgh University Press.

Reybrouck, David van 2000. Beyond Ethnoarchaeology? – A critical history on the role of ethnographic analogy in contextual and post-processual archaeology. *Vergleichen als archäologischen Methode. Analogien in den Archäologien* (Gramsch ed.), BAR Int. series 825, Oxford: Archaeolpress, pp. 39-51.

Richards, Colin 1993. Monumental choreography. Architecture and spatial organization in Late Neolithic Orkney, in: *Interpretative archaeology,* Ed: C. Tilley, Oxford: Berg, pp.143-78.

Richards, Colin 1996. Henges and Water. Towards an Elemental Understanding of Monumentality and Landscape in Late Neolithic Britain, *Journal of Material Culture 1,* pp.313-336.

Ricoeur, Paul 1976. *Interpretation theory: discourse and the surplus of meaning,* Fort Worth: The Texas Christian University press.

Riggins. Harold 1994. *The Socialness of Things: essays on the socio-semiotics of objects,* Ed: H. Riggins, Berlin: Mouton de Gruyter,

Ritzer, George 1992. *Classical Sociological Theory,* New York: The Mcgraw-hill.

Ritzer, George 1997. *Postmodern social theory,* New York: The Mcgraw-hill.

Romaios, Konstantinos A. 1954. Ανασκαφικι ερένα κοντα τιν Αναλιπσισ. *Praktika,* Athens.

Rose, Gillian 1993. *Feminism and geography: the limits of geographical knowledge,* Cambridge: Polity Press.

Rosenberg, Tiina 2002. *Queerfeministisk agenda,* Stockholm: Atlas.

Rudebeck, Elisabeth 2002. Vägen som rituell arena, *Plats och praxis: studier av nordisk förkristen ritual,* (eds) Jennbert, K., Andrén, A & Raudvere, C), Lund: Nordic Academic Press, pp. 299-342.

Sackett, James. R. 1977. The Meaning of Style in Archaeology: A General Model, *American Antiquity, vol 42, no 3,* pp.369-80.

Sackett, James. R. 1982. Approaches to style in lithic archaeology, *Journal of Anthropological Archaeology l*, pp.59-112.

Salomonsson, Karin 1998. *Fattigdomens besvärjelser: visionära ideal och vardagliga realiteter i socialt arbete,* Lund: Historiska Media.

Saltzman, Katarina & Svensson, Birgitta 1997. Inledning, *Moderna landskap. Identifikation och tradition i vardagen,* Eds:Saltzman, Katarina & Svensson, Birgitta, Stockholm: Natur och kultur, pp. 9-20.

Sartre, Jean-Paul 1988[1946]. *Existentialismen är en humanism,* Stockholm: Aldus/Bonnier

Sartre, Jean-Paul 1991. *Critique of dialectical reason*, London: Verso,

Saxe, Arthur 1970. *Social Dimensions of Mortuary Practices.* Ann Arbor: Univ Microfilms.

Schallin, Ann-Louise 1996. The Late Helladic Period, in: Wells, B. (ed), *The Berbati-Limnes Archaeological Survey 1988-1990* (SkrAth 4°, 44), Motala, pp123-176.

Schiffer, Michael B. 1999. *The Material Life of Human Beings: artifacts, behavior, and communication.* London: Routledge.

Schneider, Helmuth & Cancik, Hubert 1996. *Der neue Pauly: Enzyklopädie der Antike,Vol 8,* (eds) Schneider, Helmuth & Cancik, Hubert, Stuttgart: Metzler.

Seamon, David 1980. Body-Subject, Time-Space Routines, and Place-Ballets, *The human experience of space and place,* Eds: Anne Buttimer and David Seamon, London: Croom Helm Ltd, pp.148-165.

Segerberg, Ann 1999. *Bälinge mossar: kustbor i Uppland under yngre stenåldern,* Lund: Liber.

Semenov, S. A. 1964. *Prehistoric Technology. An experimental study of the oldest tools and artefacts from traces of manufacture and wear.* Transl: M. W. Thompson, Bath: Adams & Dart.

Sestieri, Anna Maria Bietti 1992. *The Iron Age community of Osteria dell'Osa: a study of socio-political development in central Tyrrhenian Italy,* Cambridge: Cambridge University Press.

Shanks, M. & Tilley, C. 1987a. *Social Theory and Archaeology.* Cambridge: Cambridge University Press.

Shanks, M. & Tilley, C. 1987b. *Re-Constructing Archaeology. Theory and Practice,* Cambridge: Cambridge University Press.

Shennan, Susan. 1975. The social organisation at Branc, *Antiquity No49,* pp.279-286.

Simonsen, Povl & Munch, Gerd Stamsø (eds) 1973. *Bonde - veidemann, bofast - ikke bofast i nordisk forhistorie,* foredrag og diskusjoner fra XIII. nordiske arkeologmøte i Tromsø 1970, Tromsø: Universitetsforl.

Sjöholm, Cecilia 1996. Inledning: Zizeks teoretiska projekt, *Njutandets förvandlingar: sex essäer om kvinnan, kulturen och makten*, Stockholm: Natur & Kultur, pp.7-22.

Sjövold, Torstein 1974. Some aspects of physical anthropology on Gotland during Middle Neolithic times, *Gotlands mellanneolitiska gravar,* (Janzon G.)

ACTA Universitatis Stockholmiensis, no 6, Stockholm: Almqvist & Wiksell, pp.176-203.

Snodgrass, Anthony M. 1998. Rural burial in the world of cities, In: *Nécropoles et pouvoir: idéologies, pratiques et interprétations: actes du colloque Théories de la nécropole antique, Lyon 21-25 janvier 1995,* Eds: Sophie Marchegay, Marie-Thérèse Le Dinahet et Jean-François Salles, Lyon: Maison de l'Orient méditerranéen, pp.37-42.

Socialism Today, issue no 66, June 2002. [Electronic], Available at: <http://www.socialismtoday.org/66/monarchy.html>, [2003-04-17].

Sofaer Deverenski, J. 2000. Material culture shock: confronting expectations in the material culture of children, In: *Children and material culture,* Ed: J. Sofaer Deverenski, London: Routledge, pp. 3-16.

Soja, Edward W. 1996. *Thirdspace. Journeys to Los Angeles and other Real-and-Imagined Places,* Cambridge: Blackwell Publishers Inc.

Sourvinou-Inwood, Christiane 1993. Early sanctuaries, the eight century and ritual space. Fragments of a discourse, *Greek sanctuaries. New approaches,* (Eds: Nanno Marinatos och Robin Hägg), London: Routledge.

Spaulding, A. C. 1977. On Growth and Form in Archaeology: Multivariate Analysis. *Journal of Anthropological Research, Vol 33, No 1,* pp.1-15.

Spector, Janet 1993. *What that awl means: Feminist Archaeology at a Wahpeton Dakota Village*, St. Paul: Minnesota Historical Society Press.

Spivak, Gayatri Chakravorty 1988. Can the subaltern speak?, *Marxism and the interpretation of culture,* Eds: Nelson, Cary & Grossberg, Lawrence, Houndmills: Macmillan Education, pp.271-313.

Spivak, Gayatri Chakravorty 1999. *A Critique of Postcolonial Reason. Towards a history of the vanishing present.* Cambridge (Mass.): Harvard University Press.

Stig Sørensen, Marie Lousie 2000. *Gender archaeology*, Malden, Mass.: Polity Press.

Stinchcombe, Arthur 1986. *Stratification and organization: selected papers,* Cambridge: Cambridge Univ.

Stone, Ann & Stoneking, Mark 1993. Ancient DNA from a pre-Colombian Amerindian population, *American Journal of physical anthropology 92,* pp.463-471.

Stoodley, Nick 2000. From the cradle to the grave: age organization in the early Anglo-Saxon burial rite, *World Archaeology vol 31, No 3*, pp.456-72.

Strassburg, Jimmy 1998. Let the 'Axe' go! Mapping the meaningful spectrum of the 'Thin-Butted Flint Axe, *The Kaleidoscopic past: proceeding of the 5th Nordic TAG Conference, Göteborg, 2-5 april 1997,* Eds: A-C. Andersson et al, *Gotarc* serie C no.16, Göteborg: Dept. of archaeology, pp. 156-169.

Strassburg, Jimmy 2000. *Shamanic shadows: one hundred generations of undead subversion in southern Scandinavia, 7000-4000 BC.* Stockholm: Stockholm studies in archaeology. 20.

Strinnholm, Anders 2001. *Bland säljägare och fårfarmare: struktur och förändring i Västsveriges mellanneolitikum,* Uppsala: Wikströms

Summerskill, Ben 2002. Spitfires, hymns - but no Elton at the funeral, [Electronic], *The Observer, Sunday April 7, 2002,* Available at: <http://www.observer.co.uk/Print/0,3858,4388982,00.html>, [2003-04-17].

Sunesson, Sune 1992. Inledning, In: *Vansinnets historia under den klassiska epoken,* Lund: arkiv, pp. i-xx.

Taffinder, Jacqueline 1998. *The allure of the exotic: the social use of non-local raw materials during the Stone Age in Sweden,* Uppsala: Univ.: Dept. of Archaeology and Ancient History.

Tainter, Joseph 1977. Modeling change in prehistoric social systems, *For theory building in archaeology: essays on faunal remains, aquatic resources, spatial analysis, and systemic modeling,* ed: Binford, Lewis, New York: Academic P., cop., pp.327-49.

Tarlow, Sarah 1999. *Bereavement and commemoration: an archaeology of mortality,* Oxford: Blackwell.

Thomas, Julian 1989. Technologies of the Self and the Constitution of the Subject, *Archaeological Review from Cambridge 8:1,* pp.101-7.

Thomas, Julian 1996. A précis of Time, Culture and Identity, *Archaeological dialogues 1996- 1,* pp. 6-21.

Thomas, Julian 2001. Archaeologies of place and landscape, *Archaeological theory today,* Ed: Hodder, Ian, Cambridge: Polity press, pp. 165-186.

Thompson, John B. 1984. *Studies in the Theory of Ideology,* Cambridge: Cambridge University Press.

Thrift, N. 1985. Bear and mouse or bear and tree? Anthony Giddens reconstitution of social theory, *Sociology 19,* pp. 609-23.

Tilley, Christopher & Thomas, Julian 1993. The Axe and the Torso. *Interpretative archaeology,* (C. Tilley ed.), Oxford: Berg

Tilley, Christopher 1993. Introduction: interpretation and a poetics of the past, In C. Tilley (ed) *Interpretative archaeology,* Oxford: Berg, pp.1-27.

Tilley, Christopher 1994. *A Phenomenology of Landscape,* Oxford: Berg

Tilley, Christopher 1996a. The powers of rocks: topography and monument construction on Bodmin Moor, *World Archaeology Vol 28(2),* London & New York: Routledge, pp.161-176.

Tilley, Christopher 1996b. *An Ethnography of the Neolithic: early prehistoric societies in southern Scandinavia.* Cambridge: Cambridge Univ. Press

Tilley, Christopher 1999. *Metaphor and Material Culture,* Oxford: Blackwell.

Tilly, Louise A. 1981. Conclusion, In: *Class conflict and collective action,* Eds: L.A. Tilly & C. Tilly, London: Sage, pp.233-44.

Todorov, Tzvetan 1982. *La conquête de l'Amérique. La question de l'autre.* Paris: Seuil.

Toynbee, Jocelyn Mary Catherine 1971. *Death and burial in the Roman world,* London: Thames and Hudson

Treherne, Paul 1995. The warrior's beauty: the masculine body and self identity in Bronze Age Europe, *Journal of European Archaeology 3:1,* pp. 105-44.

Trinkhaus, Katryn M. 1995. Mortuary behavior, labor organization and social rank, *Regional approaches to mortuary analysis,* Ed: Lane Anderson Beck, New York: Plenum, cop., pp. 53-75.

Turner, B. 1996. *The body and society: explorations in social theory,* London: Sage.

Tylor, Edward 1871. *Primitive culture: researches into the development of mythology, philosophy, religion, art and custom* (2 vols), London: Murray.

Ucko, Peter J. 1969. Etnography and the Archaeological Interpretation of Mortuary Remains, *World Archaeology 1,* pp. 262-90.

Urry, John 1985. Social relations, space and time, *Social relations and spatial structure,* Eds: J. Urry & D. Gregory, London: Macmillan, pp.20-48.

Wallerstein, Immanuel 1974-89. *The modern world-system I-III,* New York: Academic Press.

Wallerstein, Immanuel 1990. Culture is the World-system: A reply to Boyne, *Theory, Culture and Society, vol 9,* London: Sage, pp. 63-65.

Vansina, J. 1965. *Oral tradition. A study in historical methodology*, Harmondsworth: Penguin

Wason, Paul K. 1994. *The archaeology of rank,* Cambridge: Cambridge Univ. Press.

Weissner, Polly 1983. Style and Social Information in Kalahari Projectile Points. *American Antiquity 48,* pp. 253-276.

Weissner, Polly 1985. Style or Isochrestic Variation? A reply to Sackett, *American Antiquity 50,* pp. 160-165.

Weissner, Polly 1989. Style and Changing Relations Between the Individual and Society, *The Meanings of Things* (Hodder ed.), London: Unwin & Hyman.

Wells, Berit (ed) 1996. *The Berbati-Limnes Archaeological Survey 1988-1990* SkrAth 4°, 44, Motala.

Wells, Berit 1990. Death at Dendra. On Mortuary Practices in a Mycenaean Community, in: Hägg, R. & Nordquist, G. (eds.), *Celebrations of Death and Divinity in the Bronze Age Argolid,* Proceedings of the Sixth International Symposium at the Swedish Institute at Athens, 11-13 June 1988 (SkrAth 4°, 40), Stockholm, pp.125-140.

Welton, Donn 1998. Introduction: Situating the Body, *Body and flesh: a philosophical reader,* edited by Donn Welton, Oxford: Blackwell Publishers lnc., pp.1-8.

Vermeule, Emily Townsend 1979. *Aspects of death in early Greek art and Poetry,* Berkeley: Univ. of Calif. Press

Whelan, Mary K. 1995. Gender and Historical Archaeology: Eastern Dakota Patterns in the 19th Century. *Historical Archaeology, Volume 25,* pp.17-32.

Whyte, William H. jr. 1954. The web of mouth, *Fortune, nov 1954,* p.p140-143 and 204-212.

Wilson, Peter 1988. *The domestication of the human species,* New Haven: Yale University Press.

Wittgenstein, Ludwig 1953. *Philosophical Investigations,* Oxford: Blackwell.

Wobst, Martin H. 1977. Stylistic Behavior and Information Exchange. *For the Director: Research essays in honour of James B. Griffin,* (C.L. Cleland ed.), Museum of Anthropology Papers in Anthropology, no. 61, Ann Arbor: University of Michigan, pp.317-42.

Voyatzis, Mary E. 1990. *The Early Sanctuary of Athena Alea at Tegea and Other Archaic Sanctuaries in Arcadia* (SIMA-PB 97), Göteborg.

Wright, Rita P. 2002. Archaeology and culture: sites of power and process, *Anthropology beyond culture,* Eds: B. J. King & R. Fox, Oxford: Berg, pp.147-68.

Yates, Timothy 1990. Jacques Derrida: There is nothing outside the text, *Reading Material Culture,* Ed: C. Tilley, Oxford: Basil Blackwell, pp. 207-80.

Zizek, Slavoj 1999. *The ticklish subject: an essay in political ontology,* New York, N.Y.: Verso

Zizek, Slavoj 1989. *The Sublime Object of Ideology.* London & New York: Verso.

Zizek, Slavoj 1991. *Looking awry. An introduction to Jacques Lacan through popular culture,* Cambridge: MIT Press.

Zizek, Slavoj 1992. *Enjoy Your Symptom! Jacques Lacan in Hollywood and out.* New York & London: Routledge.

Zizek, Slavoj 1994. Introduction: The spectre of ideology, *Mapping ideology,* Ed: S. Zizek, London: Verso, pp.1-33.

Zizek, Slavoj 1996. *Njutandets förvandlingar: sex essäer om kvinnan, kulturen och makten,* Stockholm: Natur & Kultur.

Zizek, Slavoj. 2001. Il n'y a pas de rapport religieux, *Lacanian Ink 18 (spring),* pp. 80-105.

Zubrow, Ezra 1994. Knowledge representation and archaeology: a cognitive example using GIS. In: Renfrew, Colin & Zubrow, Ezra (eds). *The Ancient Mind. Elements of cognitive archaeology.* Cambridge: Cambridge Univ. Press.

Åkerlund, Agneta 1996. *Human responses to shore displacement: living by the sea in Eastern Middle Sweden during the Stone Age*, Lund: Liber.

Ødegård, Knut., Bakke, Jørgen., Risan, Thomas., Bøe Sollund, May-Liss & Stabbetorp, Odd Egil (ms). The Norwegian Arcadia Survey 1999-2001; Some remarks on Survey Methodology, To be published in: *Norwegian Archaeological Review* 2003.

Ørskov, Willie 1966. *Aflæsning av objekter og andre essays.* København: Borgen.

Østby, E., Luce, J-M., Nordquist, G.C., Tarditi, C. & Voyatzis, M.E. 1994. The Sanctuary of Athena Alea at Tegea: First Preliminary Report, *Opuscula Atheniensia 20,* pp. 693-702.

Österholm, Inger 1989. *Bosättningsmönstret på Gotland under stenåldern: en analys av fysisk miljö, ekonomi och social struktur,* Stockholm: Stockholms Univ.

Österholm, Inger 1997. Käutaltaret på Ajvide, *Ajvide och den moderna arkeologin,* ed: Göran Burenhult, Stockholm: Natur och kultur, pp. 75-84.

Österholm, Inger 2002a. The Jakobs/Ajvide site at Eksta, *Remote sensing: applied techniques for the study of cultural resources and the localization, identification and documentation of sub-surface prehistoric remains in Swedish archaeology.* Vol. 2, Archaeological investigations, remote sensing case studies and osteo-

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