Benjamin W. Roberts
Marc Vander Linden *Editors*

Investigating Archaeological Cultures

Material Culture, Variability, and Transmission

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ISBN 978-1-4419-6969-9 e-ISBN 978-1-4419-6970-5
DOI 10.1007/978-1-4419-6970-5
Springer New York Dordrecht Heidelberg London

Library of Congress Control Number: 2011929939

Printed on acid-free paper

Springer is part of Springer Science+Business Media (www.springer.com)

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Chapter 1
Investigating Archaeological Cultures: Material Culture, Variability, and Transmission

Benjamin W. Roberts and Marc Vander Linden

Homo sapiens are about pattern recognition. Both a gift and a trap

William Gibson, Pattern Recognition (2003, 22)

Introduction

The concept of an archaeological culture rarely features in any surveys of the literature of modern archaeology, especially in the Anglo-American world. When it does appear, "cultures" are treated as an anachronism – a remnant of an archaic and long-dismissed stage of the discipline. Kent Flannery's *Parable of the Golden Marshalltown* provides an exemplary formulation of the unfashionable status of the archaeological culture, when the Old Timer archaeologist was sacked by his own department for his continued but apparently outdated belief in this concept (Flannery 1982). Both introductory textbooks (e.g. Johnson 1999; Hodder and Hutson 2003; Renfrew and Bahn 2008) and theoretical compilations (e.g. Preucel and Hodder 1996; Hodder 2001; Van Pool and Van Pool 2003; Funari et al. 2005; Meskell and Preucel 2006) communicate the same message: the concept of archaeological cultures is deeply flawed and, as a consequence, should no longer be applied or even discussed.

The purpose of this volume is to re-ignite the debate concerning the analysis of archaeological cultures. The reason is that archaeological cultures continue to be employed by prehistorians throughout the world. They are used in order to make sense of potentially coherent assemblages of artefacts, from the Lower Palaeolithic to the onset of reflective literacy. This continuing practical reliance upon a theoretically moribund concept occurs even though the majority of archaeological cultures were

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B.W. Roberts and M. Vander Linden (eds.), *Investigating Archaeological Cultures: Material Culture, Variability, and Transmission*, DOI 10.1007/978-1-4419-6970-5_1,

defined during the first half of the twentieth century. The basic classification of archaeological data into broad spatially and temporally coherent blocks does not only encompass archaeological cultures but also includes other categories, such as "civilisations" (e.g. Demoule 2008), "traditions" (Osborne 2008), "groups", "horizons" (e.g. Phillips and Willey 1953; Willey and Phillips 1958), "techno-complexes" (e.g. Bar-Yosef and Zilhão 2006) and "style zones" (Cunliffe 2005). Whatever they are called, heuristic devices such as archaeological cultures appear to be embedded in the intellectual fabric of the archaeological discipline and provide one of the main characters in prehistoric narratives (Pluciennik 1999). This implies that for many archaeologists, the "culture" concept retains a validity that is independent of the extensive critiques, albeit only as an unwelcome but necessary methodological tool.

Despite the lingering "reality" of archaeological cultures, what this term might represent to modern archaeologists has not been reassessed, even in traditions where they remain widely in use. Such a consideration is often seen as either a retrograde step by more theoretically inclined archaeologists or as more unnecessary theorising by more empirically orientated colleagues. Nevertheless, the resilience of archaeological cultures implies that either a new device for grouping archaeological data needs to be found, or we must explore how patterns in the archaeological record designated as archaeological cultures are perceived, classified and accepted. It is therefore essential that applications of the archaeological cultures are re-examined. After all, culture history has had a far longer gestation period, and remains far more influential and widely used as an archaeological paradigm, than either processualism or post-processualism. In a way, archaeological cultures are the archaeological concept *par excellence*.

The Rise and Fall of Archaeological Cultures

The definition of an archaeological culture remains Childe's iconic formulation of "certain types of remains – pots, implements, ornaments, burial sites, house forms, constantly recurring together" (Childe 1929, v–vi; see Harris 1994). The primary inspiration for this scheme which incorporates both time and space was the ideas and approach advocated in Kossinna's *Siedlungsärcheologie Methode* (settlement method) (Kossinna 1911, 1926; see Veit 1989). Kossina held that "Sharply defined culture areas correspond unquestionably with the areas of particular peoples or tribes" (Kossinna 1911, 3; Veit 1989, 37, Veit's translation). Though frequently heralded as the founding fathers of the archaeological culture concept, neither their ideas nor their approach were original. The independent identification of cultural sequences based on the careful excavation of sites in the Southwestern North America (Kidder 1924) led to a florescence of approaches that analysed the interrelationships between archaeological assemblages (e.g. Gladwin and Gladwin 1934; McKern 1939; see Lyman et al. 1997; Chap. 4). This classification of the archaeological record as a mosaic of "cultures representing peoples" reflected the late nineteenth century's growing fascination with contemporary and historical ethnic and national identities. It was from this cultural substrate that cultural histories grew, leading archaeologists and anthropologists of the period throughout northern and central Europe to refer to distinctive assemblages as “cultures”. They consequently influenced subsequent generations of archaeologists on either side of the Atlantic (Trigger 2006, 232–235). The culture history approach, and especially the European version, provided a theoretical and methodological framework for the emerging national archaeologies throughout Asia, Africa, Central and South America and Oceania (Trigger 1990; Evans 1996; Murray 2001; Politis 2003; Trigger 2006, 261–278).

Critics of archaeological cultures emphasise that particular types of material culture do not equal groups or societies, thereby denying the validity of archaeological cultures as historical actors (e.g. Clarke 1968; Renfrew 1977; Hodder 1978a, b). The complexities bound up in ideas of ethnicity and identity strongly undermine any straightforward labelling of peoples from archaeological cultures, especially based upon single types of ceramics, houses or burials (e.g. Wolf 1984; Shennan 1989a; Jones 1997; Díaz-Andreu et al. 2005; Insoll 2007). The consequence is that any simple equation of data from other disciplines, such as linguistics or genetics, with an archaeological culture, in lieu of a prehistoric population, should be read with extreme caution (see MacEachern 2000; Thornton and Schurr 2004a).

Critics of the culture history approach further stress that mapping archaeological cultures yields misleading representations of spatial variation in the archaeological record which can be created through a wide variety of different natural and human factors, rather than simply as a result of the appearance of new peoples or external ideas (Taylor 1948; Binford 1965; Hodder and Orton 1976; Schiffer 1976). Archaeological cultures serve to mask variation in the material record by creating coherent entities where changes are highlighted only at spatial and temporal boundaries, a point already recognised and admitted by its leading practitioner (see Childe 1956, 124). Further doubts regarding the validity of archaeological cultures are based in the nationalist origins of many culture histories (see contributions in Kohl and Fawcett 1995; Díaz-Andreu and Champion 1996; Graves-Brown et al. 1996; Meskell 1998; Kohl and Pérez Gollan 2002; Kane 2003). All in all, the last 50 years of analytical onslaughts by theoretically inclined archaeologists have seen archaeological cultures rejected, deconstructed and subsequently ignored.

Pattern of the Past

The identification of archaeological cultures constitutes the recognition (empirically more than systematically) of interconnections in material culture through space and time whose implications are not well understood. It is assumed that studying these interconnections can provide one of the richest sources of information for reconstructing the past, especially for those periods where there is little or no written record. In this perspective, archaeological cultures are also needed given the genuine challenge raised by the rapid growth in the scale and complexity of relevant and accessible data, whether through the vast increase in the number of excavations due to developer-led archaeology throughout the world (Murray 2001), in America (e.g. Green and Doershuk 1998), Britain and Ireland (e.g. Bradley 2007) or Japan (Tsude 1995), or due to the application of new scientific techniques (Collins 2006). An alternative temptation in the current intellectual climate is to narrow the range and focus of research in order to maintain the increasingly contextual (and frequently micro-scale) archaeological practice advocated by new theoretical paradigms, such as "agency" (see Dobres and Robb 2000, 2005; Gardner 2004).

The problem with these new approaches lies not in their ability to shift to smaller ethnographic-inspired scales, but to continue addressing the larger scales. Seeking to unravel the potential multiple meanings of material culture through ever-more detailed analyses is entirely possible, provided that there is no need to move beyond the scale of the locale or its place in a thematic discussion. This could lead to a potential cul-de-sac that means involuntarily learning more and more about less and less. New data would rarely be applied to re-examining broader interconnections, such as those articulated by archaeological cultures. It is perhaps not surprising that there is a parallel development in anthropology:

> One of the worrying consequences of exponential growth in the volume of research and publication during the latter part of this century is that we know more and more about less and less. It is hard enough for any scholar to keep abreast of developments within a relatively narrow field, let alone to follow what is going on in even closely related specialisms. What is lost in the process is an awareness of the interconnectedness of phenomena, of their positioning within wider fields of relationships. Knowledge is fragmented, its objects treated in isolation from the contexts in which they occur (Ingold 1994, xx).

The application of radiocarbon dating shattered the carefully constructed edifices of cultural interrelationships. This can be seen most clearly when comparing the chronological framework of Stuart Piggott's *Ancient Europe* (Piggott 1965) to that of Colin Renfrew's *Before Civilization* (Renfrew 1973) less than a decade later. Yet radiocarbon dating did not provide a replacement model for understanding material culture through time and space, especially one that is able to shift from the micro- to the macro-scale and remain empirically grounded. Instead, this lacuna was not filled by any methodological innovation developed from the archaeological record, but by the borrowing of various broad intellectual templates. These included those of the Annales school (e.g. Bintliff 1991; Bernard-Knapp 1992), World Systems theory (e.g. Kohl 1978; 1989; Sherratt 1993; Algaze 1993), Peer Polity Interaction (e.g. Renfrew and Cherry 1986) and Interaction Spheres (Caldwell 1964; Wright 2002).

This absence of methodological replacement explains why archaeological cultures still form the basic blocks from which to create larger syntheses. Indeed, they provide a widely understood descriptive shorthand for scholars discussing broader patterns of material culture, whatever their theoretical stripes, whether in Europe (see Renfrew 1987; Hodder 1990; Whittle 1996), the Middle East (Algaze 1993; Breniquet 2006), South America (Bruhns 1994), Africa (de Maret 1990), Asia (Chang 1987; Liu 2005) or elsewhere. There is no ultimate definition of what an archaeological culture is that is either shared by the various contributors, or put forward by the authors of this introduction. The reasons for this lack of definition are twofold. First, it is the outcome of the theoretical openness for which we have opted in the constitution of this volume. Second, we do not believe that the advantages of any approach lie in its apparent intellectual totality and closeness, but rather in the multiplicity of the perspectives it creates.

Modern Uses and Abuses of Archaeological Cultures

The global acceptance of the archaeological culture concept was due to its ability to provide a clear and empirically based framework within which to place new data from excavations and surveys (e.g. Klejn 1982; Adams and Adams 1991, 214–232). The concept was widely employed in the early development of global archaeology by the leading practitioners, with a consequence that it became institutionalised through the earliest publications and teaching (Ucko 1995). It is not that the complexities of correlating ancient peoples with archaeological cultures remain unrecognised. Indeed, the problems inherent to "equating pots with people" are clearly outlined in reviews of national and regional archaeologies across the world (e.g. Trigger 1990; Hodder 1991; Ucko 1995; Koryakova 2002; Politis 2003). Neither can it be argued that modern theoretical perspectives have either failed to penetrate beyond the Anglo-American sphere or been rejected elsewhere (e.g. Eggert 2001; Biehl et al. 2002; Funari et al. 2005; Demoule et al. 2005). There is simply a widespread belief that archaeological cultures enable patterns of similarities and differences in the archaeological record to be identified and discussed, and no other framework has supplanted them in this regard.

However, it would be a mistake to assume archaeological cultures to be uniformly applied or perceived, in spite of their reputation as an empirically orientated and atheoretical approach (e.g. compare Chaps. 2, 3 and 4). Particular circumstances can lead to peculiar consequences. For example, scholarly rivalry can lead to a high inflation rate in the identification of archaeological cultures, as evidenced by the (frankly terrifying) number of Bronze Age cultures across the Eurasian steppes (Kohl 2007, 16–17). Alternatively, a massive accumulation of archaeological data can lead to the formulation of seemingly endless cultural sub-groups, as has occurred within the pottery of the Jomon culture in Japan (Habu 2004). The main variations in the global use of archaeological cultures can generally be ascribed to how closely related the archaeological assemblage is to an ancient or modern population group defined by other factors, such as linguistics or genetics, and the influence of past and contemporary politics on their interpretation.

The assigning of an archaeological culture to a specific people or group, whether historically documented or not, tends to relate to the appearance, distinctiveness and distribution of the archaeological culture. The relatively sudden appearance of new settlements, burial practices or material assemblages indicates to many scholars the arrival of a migrating people, subsequently labelled as a coherent entity (e.g. Neustupný 1982; Rouse 1986; Anthony 1990; Cameron 1995; Chapman and Hamerow 1997;

Burmeister 2000; Lightfoot 2008). There is also a greater tendency to attribute ethnic labels to archaeological cultures when there is a strong proto-historical and historical record for the continuous movement of peoples throughout a region, such as the Eurasian steppes stretching from eastern Europe to western China (e.g. Chapman 1997; Koryakova and Epimakhov 2007).

To some extent, the integration of archaeological cultures with genetic and linguistic data is predicated upon the same lines, and is driven primarily by specialists outside of archaeology who wish to explore the relationships between genes, culture and language (Sergent 1995; Chikhi et al. 2002; Oppenheimer 2006). The patterns revealed in the analysis of linguistic or genetic data are unable to be independently dated beyond broad ranges of probability inferred from general transmission processes (Dixon 1997; Richards 2003). The consequence is that some linguists and geneticists have sought patterns in the archaeological record that resemble the dating, distribution and direction of particular traits and groups of "languages" or genes, such as the equation of painted Neolithic pottery and ceramic figurines with Y-chromosome lineages across the Levant, Anatolia and parts of eastern and central Europe (King and Underhill 2002).

For these specialists, archaeological cultures would seem to provide sufficient evidence for shared underlying behaviour as well as a relatively accurate map of a population's presence in space and time. Archaeological culture is thus analysed and discussed as a single entity – the material remnant of an exclusive group defined by their ethnicity, language and culture – and frequently with a minimal consideration of the complexities and problems of the archaeological data (MacEachern 2000; Thornton and Schurr 2004b). The interdisciplinary use of archaeological cultures, whether taken at face value or from a more nuanced perspective, is concentrated in periods and regions where major transitions are thought to have occurred, such as the arrival of new populations, language families or ways of life. As demonstrated by research exploring the proposed global dispersal of languages and peoples through the spread of agriculture (see papers in Bellwood and Renfrew 2002; see Chap. 16), there is immense potential in such collaborative projects; yet, there remains a very real need to be cautious in making assumptions concerning archaeological cultures.

The political atmosphere within which archaeologists practice can strongly effect their methods and interpretations. The adoption of archaeological cultures is frequently associated with the growth of nationalism and its search for antecedents in the distant past, especially if such "predecessors" seem to demonstrate the desired longevity, exclusivity or creativity of the resident population or politically dominant group (e.g. Kohl and Fawcett 1995; Díaz-Andreu and Champion 1996; Graves-Brown et al. 1996; Meskell 1998; Kane 2003). However, to view archaeological cultures as only promoting the modern nation-state is to miss the varied and changing perceptions of these entities whose interpretation is also shaped by other forms of identity politics. For example, the European Union has been actively seeking to use widespread archaeological phenomena, such as Bronze Age or the Iron Age "Celtic" culture, in order to overcome nationalist perspectives on cultural heritage (e.g. Moscati 1991; Demakopoulou et al. 1999) – an approach that has been subsequently criticised (e.g. Fitzpatrick 1996; Collis 2003; Kristiansen 2008 and replies). In contrast, archaeological cultures throughout the former Soviet Union have been used to enhance claims over disputed territories between individual ethnic groups, including those currently lacking a representative nation-state (Kohl 1993; Dolukhanov 1995).

Yet, it is within this region that, due to the influence of Marxism rather than nationalism, pan-cultural frameworks, encompassing numerous archaeological cultures, have been most extensively developed. These broader units include "cultural intercommunity", whereby spatial stability in successive archaeological cultures is achieved due to the influence of ecological conditions; the "family of cultures", whereby the interrelationships of archaeological cultures through time allow for a genealogical model to be formulated; or the "cultural world", which denotes a structural unity throughout archaeological cultures as a consequence of social development in comparable social, political and economic conditions (Koryakova 2002; Koryakova and Epimakhov 2007, 18–21; see also in a related Anglo-American perspective Kohl 2008). There are also broad technologically orientated concepts concerning material production and consumption, such as "metallurgical provinces" (Chernykh 1992). The influence of Marxist thought on the interpretation of archaeological cultures is also manifest in China in the *quxi leixing* model, where archaeological cultures developed in parallel in different regions (Falkenhausen 1995; Wang 1997).

Even when the identity of a nation-state is concerned, the situation is rarely straightforward with regards to archaeological cultures. In Japan, culture history has not, in recent decades, emphasised ethnicity. As a result, it has been argued that more ethnicity-orientated debate is required in order to challenge the ideas of an eternal and coherent Japanese identity (Hudson 2006). In contrast, India has recently seen a rejection of archaeological debates on ethnicity, a move being fuelled by a resurgent nationalism (Chakrabarti 2003). In Poland, the strength of an ethnicity-oriented culture-historical approach has meant that, despite an awareness of the multiple problems of such interpretations, it is hard to supplant their role in analysis and discussion (Wyszomirska-Werbart and Barford 1996). In Germany, the repulsive political legacy of Kossinna's (1911, 1926) proposals of racially superior Northern European cultures has led to several generations of scholars who eschew debates on ethnicity (Veit 1989) yet remain enthusiastic adherents to the archaeological culture concept. Therefore, political agendas and patronage are extremely consequential to the encouragement or diminution of ethnic interpretations of archaeological cultures. If archaeological cultures are accepted as coherent "actors" in the past, then they will have a political dimension which may well have been instrumental to their success (Shennan 1989a; Pluciennik 1999). It is necessary to be aware of these political dimensions and the limits in ascribing identity within the archaeological culture concept (see Shennan 1989a; Jones 1997), but it is argued that these drawbacks are not sufficient to dismiss the concept as a potentially useful analytical tool for addressing interconnections. Nor can it be convincingly argued that archaeological cultures are so institutionalised in narratives of the past that it is impossible to remove them. Concepts, such as "race" and "hyper-diffusion", were widely employed archaeological concepts with both theoretical and practical efficacy before they were discredited and discarded (Wolf 1994; Orser 2003; Feder 2007). Thus it seems that archaeological cultures have not been cast aside as intellectual refuse because, in certain ways, they remain useful to scholars of the past. If they retain a use, whether it is implicitly or explicitly acknowledged, then archaeological cultures must be shown (rather than assumed) to possess validity with regards to the analysis of the archaeological record. Only if their validity in these contexts can be demonstrated can archaeological cultures be regarded as analytical units rather than as anachronisms. Doing so requires understanding the mechanisms that were involved in the reproduction of objects and practices whose apparent similarities attract the designation of archaeological culture.

Archaeological Redemption

The long-term persistence over time and space of archaeological cultures is related to the fact that they represent patterns in the archaeological record whose significance, if any, remains obscure to archaeologists. Although a general history of spatial patterning in archaeology is outside the scope of this introduction, we consider that there is much to gain by approaching archaeological cultures from this particular angle. For this purpose, we only refer to the Anglo-American development of the concept, as this particular tradition has undoubtedly spent the most energy in creating, defending and then criticising archaeological cultures. Within these debates, it is possible to trace the problem of variation in material culture being addressed using archaeological cultures, then subsequently using artefacts and finally using people. We argue that despite achieving a greater understanding of the many mechanisms at play in shaping material variation at the level of the individual and the object, we still lack the ability to address the existence of broader units, such as archaeological cultures.

Despite being based, at least in theory, on the integration of several congruent types of data, archaeological cultures were often defined on the basis of a single category of evidence, most especially lithics for early prehistory and ceramics for later prehistory. This methodological weakness, coupled with the excess of ethnic or migrationist explanations, was instrumental in the birth of processual archaeology. In order to achieve his ambitious goal of making archaeology a proper scientific discipline, Binford (1965, 1972) focused most of his attention upon material patterning, arguing that patterns observed in the archaeological record had to be interpreted in behavioural (rather than cultural) terms. On the other side of the Atlantic, Clarke (1968) pursued a similarly scientific vision of archaeology, but he explored the constitution of archaeological cultures in a systematic way, crafting a rigorous hierarchy of concepts and accompanying nomenclature.

In a related attempt at objectifying material patterning, Hodder and Orton (1976) opposed the continued use of random association groups, proposing instead that non-random association groups should be the focus of archaeologists interested in spatial patterning. This last opposition eventually provided the starting point for Hodder's (1982) seminal *Symbols in Action*, which explicitly grew out of his disillusionment with the spatial patterning of archaeological cultures. His original purpose was to understand, through ethnoarchaeological fieldwork, the potential factors responsible for the non-random association groups that he had defined in his earlier work. However, rather than elevating spatial patterning and archaeological cultures to a new level of analytical sophistication, this work led to a more anthropological tone – a "contextual" archaeology (later branded "post-processual" archaeology). This new approach concentrated on the "wholeness" of cultures through the interconnectedness of material culture. Hodder wrote:

> Each aspect of the material culture data, whether burial, settlement pattern, wall design or refuse pit distribution, can be interpreted in terms of common underlying schemes. These structures of meaning permeate all aspects of archaeological evidence. […] This is not to say that the patterns in different types of data are always direct mirror images of each other. […] The structures behind the patterning in one type of data must be interpreted by reference to other structures in other categories of information (Hodder 1982, 212).

The dissatisfaction with the concept of archaeological cultures, and especially the type of spatial patterning it encapsulated, led to the dismissal of this concept. Furthermore, it contributed to the growing realisation that spatial patterning did not necessarily have to be approached at the scale of the entire assemblage, but could be tackled at the scale of the artefact itself. This approach is probably best observed in the well-known debate on style (Wiessner 1983, 1984, 1985, 1990; Sackett 1982, 1985; Conkey and Hastorf 1990; see also Plog 1983; Hegmon 1992 for reviews). An implicit consensus within the debate was reached with the realisation that style in artefacts should be modelled using several non-exclusive factors, such as the expression of individual identity, collective cultural norms, or elaboration in terms of functional fitness.

Due to the consequential role played by ethnoarchaeology in the debate on the constitution of style, theories were frequently accompanied by discussions concerning the role of people in the construction and use of material culture. The desire to identify the actions of the individual eventually supplanted the desire to contextualise artefacts, resulting in the development of studies on cultural transmission (e.g. Shennan 1989b; Stark 1998; Bettinger and Eerkens 1999) and agency (e.g. Barrett 1994; Dobres and Robb 2000; Dornan 2002) in the mid-late 1990s. This research explored the processes at play in shaping style, or any other forms of material patterning, at the level of the corresponding human agents (see Schiffer and Skibo 1997; Stark 1998; Dietler and Herbich 1998; Dobres and Robb 2000, 2005; Eerkens and Lipo 2007; O'Brien 2008; Stark et al. 2008; VanPool 2008). These current debates over spatial patterning are not substantially different to the old-fashioned question of what constitutes an archaeological culture.

The connection between modern theory and the established concept of archaeological cultures is probably most evident in (neo-) Darwinian or Evolutionary archaeology (e.g. Eerkens and Lipo 2007; O'Brien 2008; O'Brien and Shennan 2009). While agency theory and ethnoarchaeology stress the variety and social context of cultural transmission, Darwinian archaeologists reduce this potential variation to the sole concepts of vertical (from parents to offspring), horizontal (between members of similar or different groups) and oblique (from any member of the older generation to the younger generation) transmission (Cavalli-Sforza and Feldman 1981; Shennan 1989b; Eerkens and Lipo 2005, 2007). It is considered that these concepts provide the clearest insights into archaeological patterning. For instance, in analysing Early Neolithic ceramic style in central Europe (ceramics traditionally classified in the Linearbandkeramik Culture), it was argued that the stylistic coherence of this pottery cannot be explained in neutral terms. Instead, they must rather be understood as a strong bias by the craftsmen against novelty, at least during the initial stages of settlement (Shennan and Wilkinson 2001). Similar results have been reached on the cereal assemblages of this archaeological culture (Colledge et al. 2005; Conolly et al. 2008). This purported bias against novelty, and the concomitant preference for "locked-in" inheritance systems and vertical transmission, is thus observed in at least two different components of the archaeological record. Despite the conceptual gulf between culture-historical and Darwinian archaeologies, there is, however, an intriguing convergence in the results of these two studies and the traditional definition of an archaeological culture. This could suggest that the Linearbandkeramik culture was a form of prehistoric reality (but see Chap. 9). Furthermore, and regardless of one's position towards the (neo-) Darwinian approaches, the methodological rigour found in these studies has to be acknowledged, as it constitutes an attractive alternative to the instinctive culture-historical empiricism.

The Structure of This Volume

Ideas of cultural transmission have had a long gestation, but only recent years have seen a multiplicity of reviews and edited volumes, often with a specific, theoretical perspective being thoroughly explored (Eerkens and Lipo 2007; O'Brien 2008; Stark et al. 2008). The present book takes a different stance by seeking to address cultural transmission from many different theoretical backgrounds, requesting that the contributors explore cultural transmission through the lens of archaeological cultures (or vice versa). The contributions span Europe, Africa, Asia, North and South America, and range in time throughout all periods of prehistory. The decision to concentrate exclusively on prehistory is not due to a lack of awareness of comparable debates within proto-historic and historic periods (Izzet 2007; Hakenbeck 2007; O'Brien and Lyman 2009) or a desire to ignore them. Rather, it is due to the recognition that prehistoric archaeology originally inspired the archaeological culture concept, and that an understanding of what archaeological cultures represent and how they could be approached analytically is required more urgently where there is no recourse to written texts.

Alongside this theoretical openness, we have also opted for a strong empiricism. Archaeological cultures were born out of the increasing need to classify and interpret a growing mass of data, and, now more than ever, the same challenge is upon us. The emphasis of the papers is thus as much methodological as it is theoretical, with many applying their approach to substantial archaeological datasets. Few contributors use social anthropology or ethnography in order to ground their concept of culture, as it remains under debate within these disciplines as well (e.g. Clifford 1988; Brumann 1999; Poutignat and Streiff-Fenart 1999). This is perhaps an advantage. Neither social anthropology nor ethnography has to analyse the sheer scale of datasets found in archaeology. Analysing the archaeological record on its own terms enables a more honest appreciation of the limitations of archaeology, especially when seeking to integrate different forms of archaeological and non-archaeological data. The versatility of archaeological cultures allows analysis at different scales within the same framework, and therefore shows how different scales can shed light on each other.

The contributions in this volume (beyond the first historiographical section) have been grouped into a series of sections which mirror the progressive and integrative capabilities of the archaeological culture concept. Papers in the second section concentrate on a single material or artefact category, either through necessity (as in the Palaeolithic, where flint provides the main surviving data source) or by choice when investigating the transmission of a given technology. Papers in the third section share as a starting point the assumption of the existence of archaeological cultures, and subsequently refute or expand upon their existence in order to address material expressions of identity. The fourth section comprises papers which analyse data in order to model archaeological cultures, and which explore the integration of archaeological cultures with other disciplines, such as linguistics and/or genetics. In terms of spatial scale, each section follows a scalar organisation, with papers ranging between the regional, supra-regional and even pan-continental.

Historiographies

The variety of approaches present in this volume illustrate that there is currently no consensus concerning either cultural transmission or archaeological cultures. This situation is perhaps not so surprising given the varied definitions and development of culture history and archaeological cultures in different regions and periods. Striking divergences in the nature and role of archaeological cultures can be observed even between neighbouring countries, such as Britain and France. Contemporary archaeologists in Britain reject the existence of virtually all archaeological cultures within national boundaries, yet reference them across the Channel where they are a widely accepted classificatory tool – albeit one that has been neutered (Chap. 2). The archaeological research traditions in each country also demonstrate how a scholarly emphasis on different periods strongly influences the approaches to archaeological cultures. By tracing the attitudes of specific senior figures about archaeological cultures, it is possible to explain how archaeological cultures arrived at their current status in both countries.

These intellectual biographies are especially important for exploring the *Kulturkreislehre* or “theory of cultural circles” which not only continues to influence the structure and interpretation of the archaeological record in Central Europe (Chap. 3) but also played a formative role in North American anthropology and archaeology (e.g. Kluckhohn 1936). While there are undoubtedly overlapping themes, the *Kulturkreislehre* is a distinct approach from the better known *Siedlungsarchäologie Methode* (Kossinna 1911, 1926), in that it seeks to emphasise the multiplicity of culture histories rather than their homogeneity. In North America, a similar debate centred upon the role of innovation in culture change. Here, the work of Kroeber (1923, 1935, 1940), who sought to define cultural innovation and the spread of cultural traits using quantitative methods, is fundamental (Chap. 4). By framing the debate on cultural units and cultural transmission for the subsequent decades, Kroeber created a tradition of rigorous analysis concerning cultural variation and change that can be traced up to the recent computer simulations of cultural transmission (e.g. Mesoudi and O'Brien 2008a, b).

Cultural Technology

The exclusive reliance upon a single category of data in defining an archaeological culture has often been heralded as one of the main failures of this approach. However, the modern investigation of a single technology, with its emphasis on the production, use, distribution and deposition of objects in relation to social behaviour, provides a dataset with the ability to reshape or understanding of the broader archaeological record. The delineation of archaeological cultures in the Lower Palaeolithic of Britain represents a far greater challenge than in later periods due to increased surviving evidence, chronological resolution and the issue of multiple human species potentially being responsible for the same assemblages (Chap. 5). The analysis of the flint assemblages illustrates that variation can demonstrate not only structural elements through vast time depths, but also adaptations.

Many of these issues are addressed in a far larger macro-scale perspective by Rabett (Chap. 6), who explores how the systematic investigation of a given techno-complex can reveal different behavioural trajectories amidst adaptations to new environments. The appearance of a new technology within a region is still heralded as a consequential event that potentially reshapes the societies involved. The earliest presence of copper in Eurasia provokes a shift in the scholarly perception from stone- to metal-using cultures, regardless of the quantities of objects involved or the scale of their impact (Chap. 7). The relationship between the earliest metal-using cultures and archaeological cultures reveals that metal is simply one material that is reshaped according to the expectations and desires of the communities involved. Such comparisons demonstrate the influence of cultural norms, even in the face of a potentially disruptive technology. In a similar vein, the rates and mechanisms of the dispersal of technical innovations in ceramics can provide the foundations for a broader discussion on the role of cultural innovation (Chap. 8). In a long-term investigation of ceramic technology in Iran, Petrie focuses upon the interplay between the transmission of knowledge and the practices of craftspeople, the structure of the communities in which they work, and the geographical location of production sites with respect to trade networks.

Culture Histories

Doubtless, the era has passed where archaeological cultures could be regarded as historical actors and conveniently labelled as a particular ethnic group. However, the issue of “finding identities” in the archaeological record remains present. In exploring the variation within the burial rites of a single (apparently uniform) culture, different local practices are demonstrated and the idea of a broader identity for the Linearbandkeramik culture in Central and Western Europe is rejected (Chap. 9). Drawing on Bourdieu’s (1977) notion of *habitus* and the community traditions emanating out of past remembrance, the authors stress the need to anchor archaeological cultures, such as these earliest farming communities, in the local scale to prevent variations from being ignored.

In a short, provocative essay drawing upon a recent reformulation of European Bronze Age societies (Kristiansen and Larsson 2005), the materialisation of institutions through specific object types is addressed (Chap. 10). This approach allows interpretation of elite identities in the Northern European Bronze Age to be discussed. This issue of identity is also explicitly tackled through wide-ranging ethnoarchaeological research in Sub-Saharan Africa, demonstrating the limits of equating technology, in this case making pots, with languages and ethnicity (Chap. 11). Instead, the argument is made for concentrating on the context and process of knowledge acquisition, such as apprenticeships, and the craftspeople’s conditions of adoption and practice.

Modelling Cultures

The analysis and explicit modelling of interrelationships between multiple archaeological cultures and/or forms of evidence also provide invaluable insights into the underlying mechanisms of cultural transmission. The issue of irregularity in the adoption of an innovation is one that is found throughout the human past. The role of population density and movement in the process of innovation has been discussed before (e.g. Shennan 2000), though it can now be placed theoretically within an ecological perspective – meta-population ecologies (Chap. 12). It is through this lens that the success or failure of wider cultural transmission can be explained, rather than through assumptions regarding the cognitive capacities required and/or materialised by each new technology. Similarly, the phylogenetic method is applied to lithic assemblages of the Late Glacial of Southern Scandinavia (Chap. 13). This work builds upon individual actions in order to redefine top-down interpretations of technological change and archaeological cultures.

A very different bottom-up approach can be seen in the creation of maps of individual cultural traits relating to the Neolithic of the Near East (Chap. 14). The mapping of prehistoric cultures has tended to be the result of analysis rather than the actual analytical process. Here, the authors overlay maps derived for each separate trait and, on this basis, to explore the possibilities of cultural boundaries and cultural territories. While they do not use the word, their approach to archaeological cultures is undoubtedly a polythetic one – a perspective that runs throughout the analysis of the earliest Neolithic in northwest Europe (Chap. 15). Vander Linden aims to show that the available evidence neither supports a homogeneous vision of the Neolithisation process in this region, nor should it be viewed as a series of locales. Concentrating on the archaeological cultures concerned in the introduction of the Neolithic in northwest Europe, he suggests that there are a series of recurrent mechanisms at play in the constitution of this cultural mosaic.

The importance of addressing archaeological cultures as potential units of analysis can be seen in the multidisciplinary contributions in this volume. Archaeological, genetic and linguistic evidence relating to the origins and migrations of Austronesian-speaking peoples enable not only a review of the potentials and problems of integrating these forms of data, but also shed further light on human population flows (Chap. 16). The authors' suggestion that there is a high correlation between genes, culture and language during periods of large-scale movement has great relevance to our discussion of archaeological cultures vis-à-vis identities in the past. A different perspective on interdisciplinary research is offered by the final paper in this volume, in which linguistic patterns provided the inspiration to review the archaeology of Middle Horizon Peru (Chap. 17). Macro-processes are identified by considering the chronology, geography and causation of the evidence, showing two cultural expansions in the archaeological record that coincide with two major linguistic changes.

Conclusion

There is a wide range of areas, theoretical orientations and topics throughout the volume, although it could never hope to be exhaustive within this context. For instance, the role of the environment in patterns of cultural transmission is only briefly touched upon by Hopkinson, Petrie and Vander Linden (Chap. 8, 12 and 15) Within this perspective, the use of Geographic Information Systems (e.g. Wheatley and Gillings 2002; Conolly and Lake 2006) as well as mathematical and computer modelling (see Kohler and vander Leeuw 2007; Mesoudi and O'Brien 2008a, b) could be used to disentangle random and non-random patterning in relation to landscape features, among other issues. In a related vein, quantitative approaches, with the noticeable exception of Riede's contribution, are less evident here than might be expected or desired (see O'Brien 2008). From a more theoretical point of view, questions of style, boundaries and identity are natural extensions of the archaeological culture concept which, although present in various fashions in several papers, would probably merit further theoretical elaboration. It goes without saying that any scholar will have their own list of topics and issues deemed of crucial importance.

Archaeological cultures came out of the need to connect together different elements of the archaeological record. We feel that the diversity of approaches represented in this volume demonstrates that, beyond theoretical self-imposed labels, archaeological cultures can still operate in a similar way. This is not simply because of their strong empirical content, but more fundamentally by gathering together scholars with diverse interests around that same old question of spatial patterning. We do not – and refuse to – claim that archaeological cultures are the new *big thing* for archaeology: we happily leave this fashionable task to other colleagues. We do, however, suggest that because of their analytical requirement, in terms of extensive datasets and explicitly considering of questions of scale and patterning, archaeological cultures constitute an invaluable (if ill-defined) tool for the discipline. As the stubborn persistence of archaeological cultures through time and paradigms eloquently shows, archaeological cultures were instrumental in shaping the archaeology of the twentieth century and will surely remain as influential in the future.

Acknowledgements This volume derives originally from a conference session *What is an Archaeological Culture? Approaching cultural transmission and variation* held at the 13th European Association of Archaeologist meeting in Zadar, Croatia on 18–23 September 2007. We are very grateful to the speakers and those who attended the session. We are also very grateful to Cate Frieman and Chris Thornton for reading and commenting on this chapter. Any errors remain entirely our own.

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Chapter 2
A Tale of Two Countries: Contrasting Archaeological Culture History in British and French Archaeology

Marc Vander Linden and Benjamin W. Roberts

Introduction

The definition of an archaeological culture and its subsequent application throughout Europe during the first half of the twentieth century tends to be presented as a straightforward process. Scholars in each country simply adopted ideas advocated by Gustaf Kossinna (e.g. Kossinna 1911) and V. Gordon Childe (e.g. Childe 1925) to create a mosaic of archaeological cultures which continue to structure the archaeological record. When surveying the varying directions of archaeological theory in Europe, culture-history is portrayed as a traditional and deeply flawed approach that is unusually stubborn in refusing to be consigned to oblivion, despite the presence of newer and fresher processual and post-processual theories (e.g. papers in Hodder 1991; Ucko 1995; Biehl et al. 2002). Yet, the methodology underlying archaeological cultures and their interpretation varied considerably, and each country subsequently experienced very different trajectories in the development of culture histories. At one extreme, this locally contingent development has led to the majority of archaeologists in Britain to reject culture-historical traditions while in neighbouring France archaeological cultures are still regarded as an essential tool for the spatial and temporal classification of the archaeological record. In this frequently difficult and distant relationship (Scarre 1999), the British regard much of the archaeology done in France as having been conducted within an outdated framework; yet, they predominantly derive their theoretical approaches from translations of theories by French sociologists and anthropologists. In order to explore the underlying reasons for this apparent archaeological paradox, it is necessary to compare the development of the culture-history perspective and archaeological culture structure within Britain and France, and to trace the changes and continuities in each nation.

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B.W. Roberts and M. Vander Linden (eds.), *Investigating Archaeological Cultures: Material Culture, Variability, and Transmission*, DOI 10.1007/978-1-4419-6970-5_2,

In reviewing the publications and personalities that shaped the direction of culture-history over the decades, there is a natural, though not exclusive, bias towards prehistory and prehistorians, and a particular emphasis on certain periods in each country – the Palaeolithic in France and the Neolithic, Bronze and Iron Ages in Britain. It would seem that the absence of the written record provided not only the necessary encouragement, but also the relative lack of interpretative restrictions to facilitate theoretical and methodological experimentation. The intellectual founders of culture-history during the second half of nineteenth century employed a social evolutionary perspective to identify stages of human development in the archaeological record. Beyond being indicative of social or technological complexity, object distributions were also occasionally thought to represent the migration of peoples, or the diffusion of new objects and practices. It was only in the early to mid-twentieth century that archaeological cultures were explicitly defined, stimulated by the need to analyse and compare ever-increasing assemblages. The identification of archaeological cultures with past peoples represented the pinnacle of culture-history, both intellectually and methodologically, especially in Britain. Yet, in France, while scholars analysed the spatial dimension of assemblages, their research remained entrenched in older and more chronologically orientated questions concerning the definition of apparent gaps or "transition horizons" in the archaeological record. The mid-late twentieth century and the early years of the twenty-first century have seen a series of substantial challenges to the established dominance of culture-history in each country that have resulted in very different outcomes. The application of radiocarbon dating shattered many of the relative chronologies that provided the temporal span of each archaeological culture. Furthermore, the assumptions underlying the development of culture histories and the identification and interpretation of archaeological cultures were challenged by new theoretical approaches in processualism and later post-processualism. In Britain, this development led to intense theoretical debates that eventually resulted in the apparent abandonment of archaeological cultures while in France the same process culminated in their restricted application as classificatory tools. However, as we shall demonstrate, not only is the demise of archaeological cultures exaggerated, but in neither country have the implications of the concept been addressed.

Culture-History Before Archaeological Cultures: Prehistory in Britain and France During the Nineteenth-Early Twentieth Centuries

The classification and interpretation of prehistoric archaeology in Britain and France during the nineteenth century largely reflected broader preoccupations with technological progress and social evolution. Despite early uses (e.g. Wilson 1851), the widespread adoption of the Scandinavian scheme, which divided prehistory into Stone, Bronze and Iron Ages, took place very gradually and amidst considerable resistance (Rowley-Conwy 2007). Explanations for the appearance of new technologies or object types tended to be interpreted as the result of the migration of past peoples (e.g. Latham and Franks 1856). The desire to identify stages of social and cultural development became more pronounced during the second half of the nineteenth century with the rise of evolutionary approaches in both countries (e.g. Lubbock 1865; de Mortillet 1883). Debates concerning the cultural evolution of humanity led to the development of definitions of culture, most famously Edward B. Tylor's construction of it being "that complex whole which includes knowledge, belief, art, morals, law, custom, and other capabilities and habits acquired by man as a member of society" (Tylor 1871, 1). However, the concept tended to be used as a singular, in the sense of a human universal that became increasingly complex as it was transmitted through time (Trigger 2006, 232–235).

The classification of material culture into evolutionary schemes found its purest expression in the work of Gabriel de Mortillet who, from his prominent institutional position, first at the Musée des Antiquités Nationales in Saint-Germain-en-Laye and then as professor of prehistoric anthropology at the Ecole d'Anthropologie de Paris, dominated Palaeolithic research in France (Richard 2008, 165–174), and was highly influential in Britain (O'Connor 2007, 115–125). He stressed the necessity of defining archaeological periods on the basis of corresponding artefact types (*fossiles-directeurs*), which he grouped into industries (*industries*) (Coye 1997, 136–146). Under the strong influence of transformist theory, according to which the laws of evolution are universal and apply to biological beings as well as to man-made tools, de Mortillet assigned each of his industries to a given place in a strict succession that did not allow any geographical or chronological overlap. In his scheme, the Acheuléen is followed by the Moustérien, the Solutréen and the Magdalénien (all terms which are still used today in Palaeolithic archaeology), while the entire Neolithic was subsumed under the Robenhausien which is, in contrast, no longer in use (de Mortillet 1872). This primacy of chronological classification ran parallel to the development of trench excavation and the recording of vertical stratigraphy was seen as a means of confirming the validity of the evolutionary schemes (Richard 2008, 173). Despite its adoption in both Britain and France (Coye 1997, 146–149; O'Connor 2007, 115–125; Richard 2008, 176), de Mortillet's approach did receive criticism – most pertinently from the Belgian geologist and prehistorian Edouard Dupont who proposed the possibility of distinct contemporary populations living in different regions rather than in different periods (Dupont 1874).

While prehistory in France was strongly shaped by the Palaeolithic at the expense of subsequent periods which were believed to occur after a gap in the archaeological record, in Britain there was a strong emphasis on the Neolithic, Bronze and Iron Ages. The idea of invasions and migrations shaping Britain's past was deeply rooted in the mentalities of scholars whose historical education had featured the conquest of the island by Romans, Saxons, Danes and Normans (Holmes 1907). Rather than praising racial purity as otherwise occurred within the underlying evolutionary paradigm of the period, British scholars believed that multiple invasions bought a form of "hybrid vigour" to the population that ensured and justified their supremacy in the world (Rouse 1972, 71–72). This openness to the influence of outsiders ensured that the publication of later prehistoric archaeological sites (e.g. Greenwell 1877) and corpuses (e.g. Evans 1881) led scholars to explore the relationship between the appearance of prehistoric artefacts and peoples. Migrating or invading peoples were identified on the basis of distribution maps of object types which themselves could be placed in relative chronologies. In the absence of any potential written sources, the identification of different types of Bronze Age pottery vessels in Britain as well as their potential equivalents on the continent provided the foundations for identifying a series of prehistoric invaders (Abercromby 1904, 1912). Similarly, the Late Iron Age cremation cemetery at Aylesford, Kent, was interpreted in light of the Belgic invaders into southeast Britain that had first been mentioned by Julius Caesar (Evans 1890). In France, although Alexandre Bertrand argued against the identification of megaliths and Celts, he nevertheless put forward a diffusionist scenario, megalithic builders spreading from northern Europe to southern Africa (Bertrand 1889; Coye 1997, 183–186). This movement of peoples in later prehistory was the main mechanism for social change, though crucially they were not always explicitly linked to defined archaeological assemblages (e.g. Myres 1911). In Britain, these trends led to increasing spatial analyses of archaeological data within defined periods allowing scholars to propose “cultures” (e.g. Peake 1922; Myres 1923a, b), define units, such as “cultures”, “industries” and “civilizations” (e.g. Burkitt 1921), and explore geographical methods for analysing cultural origins and boundaries (e.g. Crawford 1912, 1921). The impact in France was to reinforce the primary purpose of archaeological analysis remained the construction of more detailed artefact typologies (e.g. Déchelette 1908; see also Briard 1989 on the long-lasting influence of Déchelette on French Bronze and Iron Age research). Scholars active in Britain and France during the nineteenth to early twentieth century did not explicitly define archaeological cultures. They were nonetheless seeking to unravel a culture-history in the sense that the narratives drew on the archaeological record to follow the movements of peoples and objects.

Culture-Historical Syntheses in Britain During the Early Mid-Twentieth Century

The post-war generation of scholars in the early twentieth century did not seek to provide new definitions of culture, and neither could they claim to have invented the spatial analysis of archaeological finds or their interpretation of the patterns as past peoples. However, what they did do was develop a systematic theoretical and methodological approach towards the construction of a cultural, rather than an evolutionary, history of the past from the archaeological record. This was achieved by stressing the temporal and spatial coherence of archaeological assemblages over the analysis of individual object types. Yet, this was more than simply a re-styling of the historical narrative. By defining an archaeological culture so clearly as "certain types of remains – pots, implements, ornaments, burial sites, house forms, constantly recurring together" and emphasising that it could be assumed that this is "the material expression of what today would be called a people" (Childe 1929, v–vi), it provided an approach that could be easily applied to the dramatically expanding archaeological record. The impetus was, therefore, to analyse and catalogue archaeological features and artefacts, and then explore their relationship with other sites and cultures. This privileging of the material provided the stimulus both to those excavating sites and regional assemblages and those constructing national and continental syntheses. For the practitioners of the new culture-history, including those that would go on to develop approaches beyond it, such as Grahame Clark, "the Science of Archaeology might well be defined as the study of the past distribution of culture traits in space and time, and of the factors governing their distribution" (Clark 1933, 232).

These archaeological cultures represented past peoples whose collective dynamics could be interpreted from the changes in their material remains. The cause of any changes in the archaeological cultures of Britain lay invariably with continental invaders or traders bringing new objects or practices across the Channel, thus continuing the notion of migration or diffusion from the east as shaping the island's past. A flavour of this new history seen retrospectively through the eyes of the main pioneer, V. Gordon Childe saw its aims as "distilling from archaeological remains a preliterate substitute for the conventional, politico-military history, with cultures, instead of statesmen, as actors, and migrations in place of battles" (Childe 1958, 70). The immediate effect of the culture-history approach on British archaeology, and especially prehistory, was the creation of major syntheses which would only be replaced with the advent of radiocarbon dating several decades later. The veritable avalanche of fundamental publications saw the definition and division of the Upper Palaeolithic (Garrod 1926), Mesolithic (Clark 1932), Neolithic (Piggott 1931), Bronze Age (Childe 1930; Kendrick and Hawkes 1932; Piggott 1938) and Iron Age (Hawkes 1931) as well as seminal national and international syntheses (e.g. Childe 1925, 1940; Fox 1932; Childe and Burkitt 1932). In contrast, the subsequent two decades of culture-historical scholarship reflected a period of synthetic consolidation rather than intellectual innovation (e.g. Piggott 1954; Hawkes 1959). The main thread that ran throughout these publications was the identification and tracking of cultures and the postulation of immigrant communities responsible for change. Whether this was the Iron Age A, B or C peoples defined by Hawkes (1931, 1959) or the Early Bronze Age Wessex elites identified by Piggott (1938), they all came from the continent to Britain. Where the movement of peoples was not clear under the culture model, routes of cultural diffusion across Europe were cited to explain the appearance of seemingly novel objects, materials or practices, such as the building of megalithic monuments (Daniel 1958). This definition and systematic application of the culture-historical perspective represented a fundamental transformation in the understanding of the past and the approach in Britain.

Industries in France During the Early Mid-Twentieth Century

The early decades of the twentieth century in France witnessed the continuing concentration of scholarly energies and innovation in the Palaeolithic with a growing reaction against rigid approaches based solely on *fossiles-directeurs* and strictly chronologically and spatially sequential *industries* (Coye 1997, 253–254; O'Connor 2007, 203–38). Perhaps one of the most sophisticated proposals argued for the existence of a common component (*fonds commun*) shared by all lithic industries, regardless of their geography and chronology. It was therefore necessary to study the entire range of each lithic industry, and in particular the debitage, in order to ascertain the distinctive aspects in identifying a given assemblage (Vayson 1921, 1922). Such analyses, potentially carried using statistical tools (Vayson 1921, 346), would enable a classification of sites within a region which would then allow each to be placed in a relative chronology (Vayson 1922). Using numerous archaeological and ethnographic examples, it was also argued that there could not be any straightforward relationship between the morphology and function of artefacts, as well as any one-to-one assimilation of a particular type of tool with a given population or civilisation (Vayson 1922). Despite these pioneering insights, this approach did not prove to be entirely influential. Instead, it was the proposal of distinct, yet contemporary, lithic industries that won recognition throughout France and Britain (e.g. Breuil 1932). This new model of parallel Palaeolithic industrial cultures, though most clearly articulated by Breuil, also reflected broader discussions within the discipline (O'Connor 2007, 285–288). This conception of industries encouraged a vision of the Palaeolithic where each distinct industry was correlated to a distinctive population or human species (Coye 1997, 263–273) whose movements could be tracked through the landscape. This could be applied to bifacial and flake industries during the Lower Palaeolithic (e.g. Breuil 1930, 1932) or to the proposed material expressions of native Neanderthal populations, labelled the Périgordien industry, and invading tribes of Cro-Magnons, corresponding to the Aurignacian industry, in south-western French Upper Palaeolithic (Peyrony 1933).

This conception of industry had the same potential to address the geographical variability of archaeological assemblages in later periods as archaeological cultures had in Britain (Coye 1997, 254–255) yet relatively little progress was made (Schnapp 1981, 470; Demoule 1989). The new classification of the Neolithic into entities, such as Dommartinien, Gérolfinien and Vadémontien (Goury 1936; Desmaisons 1939) has not resisted the passage of time, in contrast to the still familiar classification employed for the Palaeolithic or the Mesolithic. Since the Neolithic was defined in reference to the Palaeolithic, rather than on basis of the sparse available data, most interpretative scenarios involved the mixing of residual Palaeolithic populations with various incoming groups, especially of Mediterranean origin (Coye 1997, 259). In a survey of Neolithic research (Octobon 1927), a series of damaging factors was listed, most noticeably the absence of stratigraphy, leading to the damning commentary that "one gathers the Neolithic, one excavates the Palaeolithic" (Octobon 1927, 253, personal translation). It is only with the publication of the stratigraphy of the Italian site of Arene Candide in 1946 (Brea 1946) that a robust classification of the Neolithic in the western Mediterranean basin was recognised (Arnal and Bénazet 1951).

The over-reliance on pottery as a chronological marker in later prehistory was hampered by the reluctance of French scholars to conduct ceramic, rather than lithic, research (the over-reliance on lithics being sometimes identified as one of the key reasons underlying the delay of French Neolithic research: e.g. Riquet 1959, 365). Despite earlier work (e.g. Fourdrignier 1905; see Coye 2001), the criticisms by Joseph Déchelette on the multiplicity and heterogeneity of the ceramic typological criteria as employed by German scholars doing comparable research proved to be highly influential (Déchelette 1908). In the resulting absence of any clear classificatory scheme, most contributions were thus either short notices describing surface finds (e.g. Octobon 1928), or regional gazetteers of sites and finds (e.g. de Pardieu 1937). Whether it was the relatively late translation of culture-historical publications by V. Gordon Childe (Childe 1949), it was foreign scholars who identified and interpreted later prehistoric archaeological cultures in France (e.g. Bosch-Gimpera and de Serra-Rafols 1926; Childe and Sandars 1950). This did not go unnoticed by French researchers who felt that the situation was "a permanent embarrassment for our foreign scholars. There have been several attempts to modify this state of affairs by doing the work themselves that should be ours. It is often difficult for them, and very humiliating for us" (Hatt 1954, 101, personal translation).

Cultural Critiques, New Chronologies and Theoretical Upheavals in Britain during the mid-late Twentieth Century

The assumptions underpinning culture-history were being questioned soon after their application, even by the main advocates of the approach, such as V. Gordon Childe, who questioned whether the idea of ethnicity should or could be central to prehistoric scholarship (Childe 1930, 240–247). The identification of invading or migrating populations was also a source of dissatisfaction to a later generation of scholars, such as Hodson (1964), whose careful reading of the archaeological evidence led him to question the framework proposed by Hawkes (1931, 1959). Just a few years later, the invasionist hypothesis that had governed British prehistory was dealt a fatal blow in an incisive and broad ranging paper (Clark 1966), but by this stage there were other challenges to the carefully constructed framework of archaeological cultures.

The application of radiocarbon dating from the late 1950s introduced an independent dating scheme which entirely undermined relative typological and cultural schemes, whether through the dramatic extension back in time for the Neolithic (e.g. contrast Piggott 1954 with Clark and Godwin 1962), or the disconnection of the much-cited links between Wessex and Mycenae (Renfrew 1968). The stark demonstration of its temporal failure was matched by doubts arising regarding the assumptions of archaeological cultures as entities that could be straightforwardly equated with people (e.g. Clarke 1968; Ucko 1969; Renfrew 1973). Whether being criticised for an absence of a scientific methodology (Renfrew 1973), a reliance on external, rather than internal, dynamics (Clarke 1968), or the ambiguous nature of cultural boundaries (e.g. Renfrew 1978; Hodder 1978a, b) to explain the archaeological record, the culture-historical approach was unable to address its deficiencies.

The proposal of a processual archaeology to replace a culture-historical archaeology should not be seen as simply an attack on archaeological cultures as it is sometimes perceived. Indeed, David Clarke, one of the leaders of the theoretical upheaval in Britain, brought the concept of archaeological cultures to a new, still unsurpassed, level of complexity (Clarke 1968). He argued that archaeological cultures arose due to the necessity of interpreting spatial patterns in the archaeological record leading Colin Renfrew to suggest that Clarke's (1968) *Analytical archaeology* was "really a treatise on patterns in archaeology" (Renfrew 1969, 242). Strongly influenced by systems theory (e.g. von Bertalanffy 1950), Clarke adopted a systemic vision of culture, conceptualised as a combination of specialised subsystems, which conveyed information flows in a structured manner. In this view, archaeology becomes the explication of the relationship between the material culture subsystem and the rest of the higher level cultural system. This theoretical goal is implemented through a thorough examination of fundamental concepts, such as type, assemblage or culture, organised into a strict, but not exclusive, hierarchical order. According to this systemic approach, an archaeological culture is no more the closed box of the culture-historians, but itself a dynamic construct, a polythetic set defined in famous terms as "a group of entities such that each entity possesses a large number of attributes of the group, each attribute is shared by large numbers of entities and no single attribute is both sufficient and necessary to the group membership" (Clarke 1968, 36). This terminological effort constituted the first stage of an ambitious reclassification of the archaeological record, a necessary prelude to new, more rigorous and complex interpretations of material patterns. Clarke's vocabulary remains the sole legacy of *Analytical Archaeology*. Indeed, partly because of the opacity of his style and his untimely death in 1976, his systemic programme was never really followed up (Shennan 1989b).

Since Clarke, British theoretical archaeology took a new direction, where material culture patterning first played a founding role before gradually being more and more neglected. In the late 1970s, Ian Hodder published a series of seminal contributions based on his ethnoarchaeological fieldwork in Zambia and, most famously, the Baringo district in Kenya (Hodder 1979a, b, 1982a). In the latter area, Hodder observed that "there is great uniformity in material culture over the extensive areas occupied by each of these acephalous tribes, and there are relatively sharp boundaries between them" (Hodder 1979b, 447). Although this research could, therefore, have provided an empirical justification for putting archaeological cultures back on the theoretical agenda, Hodder took another interpretative direction. Considering the amount of interaction between individuals and the physical distance separating them could not explain this patterning alone, Hodder stressed that material culture was not the passive recipient of human behaviour but rather one of its active components: "Material culture can be used to express and reinforce aspects of social relationships that are related to economic and political strategies" (Hodder 1979b, 448). When elaborated as an explicit reaction to the ecological determinism which then personified processualism, this statement formed the foundations for his subsequent research as well as that of his students into what came to be known as post-processual archaeology (see contributions in Hodder 1982b). By stressing the active role of material culture, Hodder erased the link between material culture and culture that Clarke had attempted to define. Indeed, in the new framework, material culture now occupied the same ontological level as the culture of the anthropologists so that any material trait could potentially have and produce meanings which could be directly translated into human terms. Drawing extensively on the translations of post-structuralist French philosophers, sociologists and anthropologists, such as (Bourdieu 1972; translated 1977), (Foucault 1969; translated 1965) and (Merleau Ponty 1945; translated 1962), who were ignored by French archaeologists (see Cleuziou et al. 1999; Coudart 1999), this subsequent generation rejected any engagement with culture-history to explore the meanings of monuments, burials and landscapes (e.g. Parker-Pearson 1999; Tilley 1994). The intellectual drive towards an increasingly contextual archaeology has meant that the scale of analysis has been reduced to the site or locale within a broader region. Furthermore, the lack of emphasis on material culture has meant that questions relating to broader scale spatial patterning that inspired archaeological cultures could be ignored.

While archaeological cultures started their career in Britain as material expressions of past ethnic realities, they lost their *raison d'être* in the sheer complexity of unravelling issues of identity from the archaeological record (see Shennan 1989a; Jones 1997). Culture-history was rejected as an explanatory body by most archaeologists, but archaeological cultures are generally covertly retained, either with reference to continental material (e.g. Hodder 1990; Bradley 2007) or where they stretch beyond Britain (e.g. Needham 2005). Likewise, the validity of archaeological cultures would appear to underlie the broader discussion on cultural transmission from a Darwinian archaeological perspective (e.g. Shennan 2002; see Chapter 1).

Archaeological Cultures as Classificatory Tools in France During the Mid-Late Twentieth Century

The fundamental redefinition of French prehistory occurred during the decades of the mid-twentieth century and can be seen in the changing status of archaeological cultures, or rather, using the contemporary French vocabulary, *industries*, *civilisations*, or *faciès* (see Lenoir 1974; Gaucher 1989). French Palaeolithic research was dominated by two contrasting figures, André Leroi-Gourhan and François Bordes. While the influence of the first is still shaping most of the contemporary agenda and extends well beyond France, Bordes' typological approach is less influential today (Audouze 2002, 277; but see Moyer and Rolland 2002). Originally trained as a geologist, François Bordes is best known for developing a rigorous quantitative approach to Palaeolithic lithic assemblages. Bordes' methodology involved firstly the constitution of lists of tools specific to the period being investigated (*listes types*), as exemplified by Bordes himself for the Middle Palaeolithic (Bordes 1953) and by his wife for the Upper Palaeolithic (Sonneville-Bordes and Perrot 1954, 1955, 1956a, b). The frequency of these types by site was then counted and plotted on a cumulative graph. This provided a graphical representation of the variability of the studied assemblage. It must be noted that, despite having been a pioneer of experimental flint knapping (see Johnson 1968, and subsequent comments by Bordes), technology only played a minor secondary role in his classification of lithic industries (Julien 1993, 166–168). Working on these premises, Bordes identified four markedly different industries for the Middle Palaeolithic (e.g. Bordes 1961). He considered these to be the material expressions of distinct past human communities: "We tend to interpret these differences as reflecting cultural differences of human groups in possession of different traditions" (Bordes and de Sonneville-Bordes 1970, 64). This general interpretation was challenged by Lewis and Sally Binford, who used Bordes' own classification and new statistical approaches to suggest that assemblage variability was related to functional and adaptive factors (Binford and Binford 1966). Regardless of the actual outcome of this debate (e.g. Dibble 1991; Chapter 12), Bordes undeniably brought a new level of methodological rigour to the study of lithic industries.

As the Bordes were compiling their *listes types*, the ethnologist turned prehistorian André Leroi-Gourhan was taking French (prehistoric) archaeology into a new direction. It is impossible here to review his extensive oeuvre and impact (e.g. Stiegler 1994; Groenen 1996; Audouze 2002; Audouze and Schlanger 2004). One of the founding figures of cultural technology, Leroi-Gourhan was first and foremost interested in the interaction between man and technique (e.g. Leroi-Gourhan 1964), and introduced the concept of *chaîne opératoire* into archaeology (Audouze 2002). In order to implement his programme, he revolutionised French archaeological excavation techniques by introducing carefully conducted and recorded horizontal excavations, applied to either Palaeolithic (e.g. Pincevent: Leroi-Gourhan and Brézillon 1966) or Neolithic sites (e.g. hypogeum of the Mournouards: Leroi-Gourhan et al. 1962). It is this focus on high-quality excavations and associated techniques (e.g. intra-site variability, lithic technology and refitting etc.) that still characterises most of the French archaeological agenda, especially in Palaeolithic studies (Audouze 1999). However, it also led to less interest in the question of archaeological cultures which, despite Leroi-Gourhan's own personal and fluid perception of ethnicity, did not have any reward in his intellectual and philosophical framework (Demoule 1999, 196–197).

The mid-twentieth century also witnessed the floruit of archaeological cultures throughout French later prehistoric research. The following discussion, however, mostly focuses on the Neolithic, for which the concept of "archaeological cultures" appears to be most crucial. After decades of demise, the successful classification of the Neolithic period began due especially to the growing recognition of the important role of ceramic analysis (e.g. Arnal and Bénazet 1951) and the dismissal of some of the categories inherited from the late nineteenth century. These include the Campignien, a proposed hiatus horizon between the Mesolithic and Neolithic that still had influential adherents (Nougier 1954a, b), but was completely demolished by Bailloud and Mieg de Boofzheim in their seminal *Civilisations néolithiques de la France dans leur contexte européen* (Bailloud and Mieg de Boofzheim 1955). This last book is in many respects exemplary of this renewed French Neolithic research. In their introduction, Bailloud and Mieg de Boofzheim defined their methodology into three successive stages (Bailloud and Mieg de Boofzheim 1955, 3): firstly, "defining sufficiently stable and homogeneous cultural groups" on basis of the available documentation, especially, but not only through pottery ("ethnographic stage"); secondly, defining the geographical extension of these groups ("geographic stage"); thirdly, organising these groups into a coherent chronology ("historical stage"). They argued that these three steps together comprise the necessary requirements for any synthesis. The methodological influence of Childe's archaeological cultures is obvious, and indeed acknowledged (Bailloud and Mieg de Boofzheim 1955, 213), but these similarities should not be overstated. The discussion on the explanatory role of diffusion is reduced to the mere recognition of three major diffusionist streams (Near East, Continental and Mediterranean: Bailloud and Mieg de Boofzheim 1955, 6–10). Their volume is otherwise a thorough description, admittedly organised in archaeological cultures, of extensive data that was previously scattered with little order.

This classification of the French Neolithic continued in the 1970s and the 1980s (e.g. Guilaine 1980; Blanchet 1984). It is very much perceived as unfinished due to the self-proclaimed primacy of ongoing fieldwork, leading to the constant revision of cultural frameworks (Cleuziou et al. 1973; Audouze and Leroi-Gourhan 1981, 174–178; Schnapp 1981; Demoule 1989). This situation has now intensified under the pressure of the vast amount of data produced by development-led archaeology (Demoule 2005a). It is probably in this sense that many British archaeologists would consider the bulk of contemporary French research as culture-history. Such a dismissal is slightly unfair since, in the modern French tradition, archaeological cultures are first and foremost considered as a necessary classificatory tool, without prejudging of an interpretation which can be set in ecological or social terms, but very rarely, if ever, in terms of migrations (e.g. Demoule 2005b). This apparently paradoxical situation is best exemplified by the "culture" entry of the *Dictionnaire de la Préhistoire*: "Association of a given number of elements of the material culture of a population, those which are preserved and that [prehistorians] can recognise […]. It is necessary that the elements used allow each [archaeological culture] to be placed within relatively precise chronological limits. It is obvious that, by defining a culture on basis of partial and disparate elements, prehistorians must admit that they make whatever formed the unity and internal coherence of this potential culture disappear. Nobody knows which reality the defined entities could correspond to. Their creation is however necessary, for the description of chronological sequences and for the palethnological study. It is however important not to see this convenient classification as the direct expression of a past reality" (Leclerc and Tarrête 1994, personal translation). In this sense, archaeological cultures appear as a fundamental tool, although perhaps not the one best suited for the "palethnological" goals which characterise the practices of the archaeological discipline since Leroi-Gourhan.

There have been very few attempts within French archaeology to reconcile archaeological cultures and cultural technology. One notable exception must, however, be mentioned. In a lengthy paper published in the late 1980s, Pierre Pétrequin and his collaborators put forward a reinterpretation of the Late Neolithic (i.e. mid third millennium BC) Sâone-Rhône "civilisation", distributed in central eastern France and western Switzerland (Thévenot et al. 1976; Pétrequin et al. 1987/1988; see also Pétrequin 1993). The basis of their work is twofold: firstly, the existence of a series of waterlogged lake sites with perfect organic preservation which enabled the creation of a high-resolution dendrochronology (e.g. Pétrequin and Pétrequin 1988); secondly, informed by decades of ethnoarchaeological fieldwork first in western Africa and then in Papua New Guinea (e.g. Pétrequin and Pétrequin 1984, 1993), an ethnoarchaeological approach which presupposes that "each element of the material culture can possess its specific dynamics" (Pétrequin et al. 1987/1988, 4, personal translation) so that ceramics, lithics and ornaments (or any other relevant artefact) are independently studied. It is only after this initial analytical stage that Pétrequin and colleagues looked for "the essential links, parallels, complementaries or oppositions between the evolutionary dynamics of each category of artefacts, in order to try to suggest a more nuanced picture of the content of the Sâone-Rhône civilisation (Pétrequin et al. 1987/1988, 4, personal translation)". The influence of David Clarke's polythetic model here is obvious (see also Shennan 1989b, 833), although in their conclusion Pétrequin and colleagues distance themselves from this particular stance on archaeological cultures. Indeed, they stress that the high level of spatial and temporal variation in the various facets of the material culture they analysed stretches the conceptual limits of both archaeological and polythetic cultures (Pétrequin et al. 1987/1988, 73–77). They consider that the region under investigation was set at a crossroad of influences so that "the civilisation [culture] has progressively become an area of technological transfer, which is a form of originality as important as the concept of the culture-block or of the polythetic culture" (Pétrequin et al. 1987/1988, 77, personal translation). In their scenario, the flux of influences is not simply a typological construct, but the result of continuing small-scale movements of populations related to ever-changing ecological and demographic pressures (Pétrequin et al. 1998, 1987/1988; see also Shennan 2000).

Conclusion

The image of the chest of drawers is sometimes invoked by its critics to describe culture-history, with its apparent seamless succession of archaeological cultures. Unsurprisingly perhaps, the same scholars have a similar vision of the history of archaeology, with culture-history giving way to processualism, which is then followed by post-processualism (e.g. Jones 2008). Obviously, the historical reality is much more complex between these various paradigms. Culture-history in Britain always possessed a strong geographical and social component, O.G.S. Crawford being the first to coin the term "social archaeology" (Crawford 1921, 100). In contrast, French scholars were (and still are) more concerned with temporal and classificatory issues than identifying, for example, wandering tribes. These differences are partly explained by the general orientation of the archaeological research, with British archaeologists having paid more attention to later prehistory, while the Palaeolithic period was given primacy in France (see also Scarre 1999, 156). Culture-history was thus never a coherent tradition (see also Chaps 1, 3 and 4).

It is therefore necessary to disentangle the systematic association of culture-history and archaeological cultures. Archaeological cultures were a conceptual and methodological addition to the culture-history perspective with its over-reliance on diffusion and migration as explanations. Archaeological cultures and industries were initially devised to enable a more sophisticated level of analysis of the archaeological record, especially its spatial dimension. That they started as a foundation for an "event history" based on migrations is thus more informative of the intellectual context of their elaboration and early use, rather than of their intrinsic nature. This last point is demonstrated by the role played by archaeological cultures in French Neolithic research since its renewal in the mid-1950s. In this case, archaeological cultures have been stripped of their diffusionist dimension and, although remaining a challenging concept, still constitute an elementary stage of any work. It is tempting to conclude that the various problematic issues associated with archaeological cultures are related to their uncritical use or rejection by exclusive theoretical schools, ranging from diffusionnism to post-processualism. Rare studies, such as Clarke's systemics (Clarke 1968) or Pétrequin's ethnoarchaeological approach (Pétrequin et al. 1987/1988), demonstrate the great potential in considering archaeological cultures, not as a ready-made ill-suited tool for a given theoretical goal, but as an object of empirical investigation per se.

Acknowledgements We are very grateful to Cate Frieman, Jody Joy and Chris Scarre for reading and commenting on earlier drafts of this paper.

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Chapter 3
Thoughts in Circles: *Kulturkreislehre* as a Hidden Paradigm in Past and Present Archaeological Interpretations

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Introduction

Despite the common myth that archaeology in German-speaking countries is hostile to theory, recent papers on research history have demonstrated the contrary. Reflections on research history tend to focus primarily on Kossinna and the *Siedlungsarchäologische Methode* (settlement archaeology) explaining the concept, the political implications as well as the misuse during the Nazi regime (e.g. Härke 2000; Veit 1984; Werbart 1996; Kossack 1992). There were, however, other important schools of thought that shaped the way archaeologists interpreted the past. This paper is an attempt to highlight the theory of cultural circles, the *Kulturkreislehre*, as an important paradigm in the development of the discipline. The theory of cultural circles, a branch of the German cultural–historical school of thought in anthropology, dominated interpretations in ethnology as well as prehistory at the beginning of the twentieth century, especially in Central Europe. The Viennese school of prehistoric archaeology had a pioneering role in this development for a number of reasons. The University of Vienna created the first chair exclusively for prehistoric archaeology in the whole of Europe in 1892. The Natural History Museum in Vienna, which had opened its collections in 1889, combined collections on ethnology and archaeology in one house, and became a central point of archaeological research for the area of the Austro-Hungarian empire, which encompassed sites not only in today's Austria, but also in the Czech Republic, Slovakia, Hungary, Poland, Romania, Slovenia, Croatia, Bosnia, Serbia, Italy, Montenegro and the Ukraine. Research traditions were begun before World War I and continued through the formation of new nation states, often using German as a language that was understood in academic circles across borders.

Theory in German publications is rarely found as a separate chapter called "theory" at the beginning of a monograph. Theory is often implicit and written

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B.W. Roberts and M. Vander Linden (eds.), *Investigating Archaeological Cultures: Material Culture, Variability, and Transmission*, DOI 10.1007/978-1-4419-6970-5_3,

between lines, and reflection upon it can be doubted in many cases (Karl 2005). Nevertheless, theory existed during the course of writing about prehistory. In fact, without theory, there would be little to write about prehistory, since in the absence of written sources explanations of material culture are what we make of them – artefacts, after all, do not speak for themselves.

Different academic traditions shaped the past and current understanding of prehistory in Europe in different ways. Whereas in Northern Europe evolutionary models remained dominant, in Central Europe, especially in the area of the former Hapsburg Empire and southern Germany, remnants of the cultural circle theory still infiltrate ways of explaining the archaeological record (Sørensen and Rebay 2008a). To a certain extent, however, theories were not developed and formulated independently from the large quantities archaeological material being discovered. The nature of the archaeological record does in some cases suggest distinct groups which did things differently than their neighbours and used different material culture, and one may suspect that creating boundaries through different practices was a deliberate form of stating group identities. This paper might not fulfil all criteria of writing a straightforward historiography, but by bringing together broad traits and *mikrohistoire* (Kaeser 2006), it might help to understand the past of our discipline which is vital for today's interpretations; reflecting upon research history is, I believe, quality control for ongoing research.

Origins of Cultural Circles

In the mid nineteenth century, when prehistoric archaeology, ethnology and physical anthropology were still under the umbrella of anthropology in Central Europe, ideas of human evolution and cultural development were shaped and implemented into the academic discipline. Unilinear schemes of human development, however, proved unsatisfactory for explaining the diversity of human culture (Closs 1957, 1; Bernatzik 1962, 4), and the anti-evolutionary, cultural historical approach promised new insights. F. Ratzel (1844–1904), a geographer, explained similarities in material culture using migrations, despite the need to bridge huge distances (Hirschberg 1988, 392). His strongest interests lay in exploring the mutual relationships between space, nature and man, an approach he labelled *Anthropogeographie* (Human Geography). Ratzel explicitly opposed evolutionary theories (Köb 1996, 37), believing that early stages of mankind were provided with a number of inventions that could only spread through diffusion. Distinguishing between "*Kulturvölker*" (culture-peoples) and "*Naturvölker*" (nature-peoples), he assigned the latter minor mental capacities and stagnated levels of development. Ratzel used the term *Kulturkreis* to describe an area in which a culture was spread, as well as to describe the culture itself (Leser 1963, 9).

The term *Kulturkreis* was already in use in the German language by the mid nineteenth century, without being thoroughly defined. E. Meyer (1855–1930), for example, a historian and writer of the monumental "Geschichte des Altertums"

(History of Antiquity) (Meyer 1884) differentiated between cultures and circles of cultures: Meyer used "Kultur" to refer to a uniform cultural entity, and "Kulturkreis" to refer to a mixed culture. "*Wenn zwei ursprünglich unabhängige Kulturen in Berührung treten, sich gegenseitig beeinflussen und auf neutralem Gebiet mischen, immer neue Nationen in den Bereich ihrer Einwirkung ziehen, dann entstehen große, sich immer weiter ausdehnende, zu immer gesteigerterer Wechselwirkung der einzelnen Glieder auf einander fortschreitende Kulturkreise*". (If two originally independent cultures make contact, influence each other mutually and mix in a neutral area, dragging ever new nations into their domain of activity, then large and further expanding *Kulturkreise* are formed, in which the single components interact in ever more complex ways.) (1884, after Leser 1963, 20).

One of Ratzel's pupils, L. Frobenius (1873–1938), introduced the term *Kulturkreis* in ethnology in 1898. Frobenius saw culture as a living organism that is born, develops, ages and dies with humans being the "bearer of the culture" and being objects rather than subjects of a culture (Hirschberg 1988, 273). The *Kulturkreis* as a basic concept of cultural historical ethnology became popular and widely known through a lecture by F. Graebner and B. Ankermann in Berlin's Anthropological society on November 19, 1904 (Köb 1996, 276; Eggert 2001, 39). Fritz Graebner (1877–1934) was the first to define the *Kulturkreislehre* in more detail in his "*Methode der Ethnologie*" (Method of Ethnology) (Graebner 1911) as a historical method of ethnology. Graebner's "Kulturkreis" is a complex of cultural elements typical for a certain area covering religion, material culture, settlement patterns, forms of tools and weapons, burial customs, etc. Graebner's primary goal was to explore and establish relations between cultures, and furthermore, various ethnographically described cultures were to be integrated into a general historical development. The Kulturkreislehre aimed to write a "history of civilisation", covering all cultural expressions of all peoples, regardless of time and space (Graebner 1911, v). Prehistoric archaeology could provide a window into the remote past. The methodology of mapping the spatial distribution of types of finds became introduced to prehistory around the turn from the nineteenth to the twentieth century, and scholars like A. Lissauer, R. Beltz and A. Voss worked on establishing type maps for further research (Eggert 2001, 271).

From *Kulturkreis* to *Kulturkreislehre*: The Viennese School of Ethnography

The step from *Kulturkreis* as a descriptive entity to *Kulturkreislehre* as an explanatory theory was undertaken by the Viennese school of ethnology. Some of the urgent questions at the time were tracing the origins of religion, family or state (Hirschberg 1988, 272), which were believed to be found by launching a worldwide sequence of *Kulturkreise*. A universal history of mankind was to be established with cultural historical methods, using Graebner's method and the concept of *Kulturkreise* as well as Ratzel's migration theory as basic principles. Cultural elements, in particular material culture, were compared in terms of form, quantity and constancy, and on the basis of this comparison, relations between cultures were established. Groups of matching forms were called *Kulturkreise*, which would ideally be paralleled with language ("*Sprachkreise*") and brought into a historical sequence. Cultural elements were believed to have limited origins, and parallel inventions in different cultures were held to be unlikely. Consistency was assumed to be a main trait in all societies, and as a consequence, change was explained by cultural contact and fusion rather than development (Hirschberg 1988, 271). The spread of cultural elements was explained historically by cultural contact – "cultural elements rest solely on an historical connection" (Kluckhohn 1936).

The main proponent of the Viennese school of ethnology was Wilhelm Schmidt (1868–1954). Acknowledging some of his biography is essential for understanding the paradigms of the *Kulturkreislehre*. As a priest in the missionary order Societas Verbi Divini (SVD), Schmidt's research was rooted in Catholicism (Köb 1996, 58). Many of his close collaborators, such as Wilhelm Koppers (1886–1961), who became the first professor for ethnology in 1928 (Köb 1996, 51; Hirschberg 1988, 263) or his pupils, such as Martin Gusinde and Paul Schebesta, were members of the same catholic order (Köb 1996, 53). Schmidt taught in the Roman Catholic seminary St. Gabriel near Mödling, founded the journal "Anthropos" and became professor for anthropology in 1924. As an influential man in the church, he became director of the missionary and ethnological museum of the Vatican in Rome (Köb 1996, 51), but stayed in contact with the Institute in Vienna until 1938, when he emigrated to Switzerland. Schmidt had a strong interest in linguistics and religion, but was also interested in the material culture of all peoples. Without engaging in fieldwork himself, Schmidt synthesised data collected by his students.

Schmidt struggled to combine the results of scientific research and Catholic doctrines. The Catholic Church's central position was the belief in the creation of man by God, which naturally did not fit evolutionary theories. The church opposed Darwinism, but as a researcher Schmidt engaged with Darwinian thoughts and even accepted the evolution of species. He did not even rule out the possibility that man as well as primates descended from earlier, animal-like ancestors; at the same time, God's creation of Adam and Eve was untouchable as a dogma. One way out of the dilemma was to state that the first divine revelation ("*Uroffenbarung*") could only reach and be accepted by man who had developed sufficient mental capacities (Köb 1996, 68). Schmidt's scientific driving force was the desire to find scientific proof for the divine revelation and existence of God (Köb 1996, 48). Remaining solid in his belief and faithful to the church, his solution was trying to find the origin of the idea of God ("*Der Ursprung der Gottesidee*", 1912–1955). His idea was that especially in technologically primitive cultures, one god ("*Hochgott*") is worshiped as creator of the world and guarantor of a moral order (Hirschberg 1988, 424).

The descent of man remained a central question. Most representatives of anthropological disciplines in Vienna welcomed Jakob Kollmann's theory of 1902 that a short race of human gatherers, rather than primates, represented the origin of mankind (Fuchs 2003, 207). This theory was more easily integrated with the Bible, according to which Adam and Eve were the parents of all people. According to Schmidt, their direct descents, the "*Urgesellschaft*" lived in small monogamous families, holding both private and communal properties and worshiped their one creator. This form of social organisation was seen as original and natural (Fuchs 2003, 208). Through population growth, groups split off and populated the earth transforming and mixing. Some societies, however, remained in a state of culture that was very similar to the original, primary one (Köb 1996, 61–62). Schmidt saw the "*Urkultur*" represented in the Pygmies, Southeast Asian Negritos and Fuegians, who for him all belonged to one "race" (Köb 1996, 74). Graebner was sceptical to equate prehistory and contemporary peoples under the name of a "primary culture", but to Schmidt's theory, there were no chronological or spatial limits (Köb 1996, 80). From the original hunter gatherer culture ("*Urkultur*"), three parallel primary cultures ("*Primärkulturen*") emerged: advanced hunting, matriarchal farming and nomadic herding. Secondary cultures ("*Sekundärkulturen*") and eventually advanced civilisations result from mixtures of primary cultures. Hierarchical social structures, for example, were explained by an overlap of differentiated agricultural societies and cattle-breeding nomads (Hirschberg 1988, 272). Change was not seen in terms of a development from primitive to higher cultures (Wahle 1951, 108), but as a wide variety of forms and stages as well as degenerations. Christianity, of course, was an exception and was interpreted as restoring the natural order according to god's will.

The Catholic theological approach of the main proponents of the Viennese school did bias research, but had its benefits as well. Although the classification of "primary" and "secondary" cultures, "primitive" and "civilized" people implied racist inequalities, people of "primary" cultures were valued as being closer to creation and therefore closer to God. The national-Catholic reasoning of anti-Semitism and the view on the "Jewish question" were different as well, since anti-Semitism was rooted in religion rather than in a pseudo-biological, fictional "race".

The Political Context

Austria's political background needs to be reviewed to understand the short-lived success and rapid decline of the Viennese School of ethnology. In contrast to Germany, Austria–Hungary was a multi-ethnic monarchy, a difficult concept in the nineteenth century with emerging nationalist movements and nations struggling for recognition. After the First World War was lost, the monarchy was abolished and the much smaller First Republic of Austria suffered from severe political rifts. Political opinions and differences played an important role in private and academic life between the wars, and conceptual differences ("*Weltanschauung*") were huge and irreconcilable. The city of Vienna, as an industrialised capital, was ruled by a socialist majority, in contrast to most of the countryside, where patriotic, conservative Catholics held the power. There were a number of people who would have appreciated Austria's integration in the larger Germany. The political rift between the Catholic-conservatives and the sympathisers of Germany is probably mirrored by the split of the department for "*Anthropologie und Ethnographie*" into two separate entities in 1928. Tables were turned as chancellor Engelbert Dollfuß established an autocratic regime (Austro-Fascism) that was dominated by the conservative, Catholic and patriotic "*Vaterländische Front*" (Patriotic Front) in 1933. The Communist Party and the NSDAP were banned in 1933, the Social Democratic Party of Austria after the February Uprising of 1934. Austro-Fascism remained in place until March 12, 1938, when German troops occupied the country and the annexation of Austria to the Third Reich was declared. Ethnology thus escaped the extremely racist physical anthropology until 1939, when the clerics were replaced by sympathisers of the Nazi regime, such as Hermann Baumann (Hirschberg 1988, 56).

Kulturkreislehre and Prehistory in Vienna

Primarily because the *Kulturkreislehre* was essentially anti-evolutionist, the Viennese School of Ethnology aligned with the historical sciences rather than the natural sciences. The explicit historical focus and the less explicit rejection of biology (physical anthropology), functionalism and structuralism were bound to enable close collaboration with prehistory. In its application, the *Kulturkreislehre* is generally descriptive rather than explanatory and great effort was put into describing the detailed components of each *Kulturkreis* as well as defining its boundaries (Kossack 1992, 86). This methodology was very well suited for prehistoric material culture. Ideas from the *Kulturkreislehre* in ethnology were picked up explicitly by prehistoric scholars, such as Moritz Hoernes and Oswald Menghin, and implemented in a more subtle way by many others (Veit 1989, 41).

Moritz Hoernes (1852–1917) worked in the National History Museum of Vienna before he gained the first proper professorship for prehistoric archaeology at the University of Vienna in 1911. Influenced by Lubbock and Morgan, he argued in his basic systematics of prehistoric archaeology ("*Grundlinien einer Systematik der prähistorischen Archäologie*", 1893) that mankind before written records was above all a creation of nature. Prehistory could be divided into two parts: natural history, including the origin and descent of man, and a cultural history with the sequence of Palaeo- and Neolithic, Bronze, Hallstatt and La Tène periods (Hoernes 1893, 70). According to Hoernes, prehistory and ethnography aimed to broaden and complement the concept of mankind over the boundaries of history ("*den Begriff der Menschheit über die von der Geschichte in herkömmlicher Weise gezogenen Grenzen hinaus erweitern und ergänzen*") (Hoernes 1893, 69). One of his points was that neither language nor race, but "Bildung" (education and culture) made peoples (Hoernes 1893, 50). A primary cultural element ("*culturelles Urelement*"), he argued, is to be found all over the world inhabited by people (Hoernes 1893, 51). The origin of mankind, however, which he saw as the separation of man from the animal world (Hoernes 1893, 68), is a topic he left to physical anthropologists. Hoernes declared that "prehistoric stages of culture" did exist in contemporary times and were, therefore, valuable to complete the fractured archaeological sources (Hoernes 1926, 7).

He suggested approaching prehistoric archaeology systematically from three different angles: areas and peoples described as "*Grundfaktoren*" (basic factors); inventions, change and tradition known as "*Entwicklungsfaktoren*" (developing factors); and language, religion, family, tools, industry, trade, etc., which he interpreted as "*Einzelformen*" (individual forms). Among the description of the "*Einzelformen*" were well-known classic aspects of prehistoric archaeology, such as the form of houses, settlements, burials and fortifications. The gathering and description of these individual forms was of course suitable to reconstruct "*Kulturkreise*" (Fuchs 2003, 210). Hoernes' 1885, main publication was the 1898 monograph "*Die Urgeschichte der bildenden Kunst in Europa*" (Prehistory of Plastic Arts), but as early as 1895 he wrote the "*Untersuchungen über den Hallstätter Culturkreis*" (Studies on the Hallstatt Cultural Circle), which he subsequently divided into four groups in 1905 (Hoernes 1905). This division has remained influential until today.

Oswald Menghin (1888–1973), Hoernes' student and successor on the chair of prehistory in Vienna after Hoernes' death in 1918, was the most explicit and prominent prehistoric archaeologist using the theory of cultural circles. Otto Urban has recently put together and interpreted his curriculum (Urban 1997), which is indeed exciting and gives evidence of a strong, yet ambivalent character. Menghin was born in Meran, in a part of Tyrol that belonged to the Austrian Empire at the time and is now Italy. He seems to have been, first of all, a convinced Catholic, a patriot on the borderline of being nationalist, and he was, to a certain extent, a Germanophile, racist and anti-Semitic. It would not suffice to describe him as a Nazi. He was not even a member of the NSDAP until 1940, despite having applied for membership in 1938 (Urban 1997, 9). As Urban put it, he was "the man between the lines". Menghin engaged in the political discourse of the time. Rather than "trying to cloak religion and political beliefs in a scientific mantle" (Kohl and Pérez-Gollán 2002, 574), "Menghin was caught up in a worldview … that he believed in" (Trigger commenting on Kohl and Pérez-Gollán 2002, 582).

The political circumstances did indeed suit Menghin's career as he became Dean of the University of Vienna in 1935/1936. Engaging more and more in politics, he pursued and actively helped to carry out the annexation of Austria into Greater Germany as a member of the Seyß-Inquart cabinet in 1938. When Minister for Education for only a few weeks, he was responsible for the "cleansing" of the University of Vienna, hence responsible for the dismissal and replacement of Jewish and dissident professors and the severe restrictions for Jewish students (Urban 1997, 9). Menghin returned as professor to the Institute of Prehistory in Vienna in August 1938 and remained in his position until 1945. After the war, he was considered a war criminal and imprisoned, but on invitation of Argentina's government, Menghin emigrated to Buenos Aires in 1948 (Urban 1997, 9–10). Menghin's surprisingly successful second career in South American archaeology, which he pursued within the same theoretical framework, has recently been discussed by Kohl and Pérez-Gollán (2002).

Menghin defined prehistory as a historical discipline. His aim was to reconstruct *Kulturkreise* as in ethnology and to write a cultural history based upon archaeological evidence (Kohl and Pérez-Gollán 2002, 571). His major work was a synthesis of the Stone Age "*Weltgeschichte der Steinzeit*" (Menghin 1931). Adopting Schmidt's theory of a primary culture, he created a world system of cultural circles for prehistory, pointing out the links between culture, language and "race".

Race

At this point, it is worth pointing out that "race" was not a fixed category at the time. In fact, there was an ongoing debate on the nature of "race" in the 1920s. Was "race" a linguistic, a cultural–historical or a purely biological category? The Viennese School, and probably the majority of scholars at the time argued for the first definition while representatives of a more German-nationalist standpoint agued for the latter (Fuchs 2003, 278). Furthermore, there was a range of different connotations regarding the "purity" and "mixing" of races. Whereas some scholars insisted that impurity lead to degenerations, others saw the mixing of races as one of the few mechanisms of innovation and cultural change.

The latter position was certainly not typical among the anthropological schools of other countries at the time. Explanations may be found in the fact that Austria did not have overseas colonies, and therefore research on "races" was focused on ethnic differences within the Austro-Hungarian Empire. The Habsburg monarchy had understood itself as multi-ethnic, and Vienna was indeed the "melting-pot" at the turn of the century (Fuchs 2003, 152–158). The discussion on the nature of "race" had an impact on important questions in prehistory. For example, the debate about whether "Indo-Germans" came from Northern Germany/Scandinavia or from Asia was neatly divided along political lines. W. Koppers engaged in this debate on the grounds of ethnology (Koppers 1929, 1941; after Wahle 1951, 109), arguing for an origin in the east ("*Ostthese*"). Menghin preferred a European origin of the "Indo-Germans" and linked them cautiously with the spread of the Neolithic or Schnurkeramik (Menghin 1935, 79).

Menghin's 1933 opinion on race was somewhere in between these positions. In 1933, he published the book "*Geist und Blut*" (Spirit and Blood), which expressed his opinion that education and culture are more important than genetic inheritance, which, of course, impacts individuals as well (Fuchs 2003, 279; Urban 1997, 6). The book is by no means free of racist and anti-Semitic statements, but also did not quite fit the Nazi myth of Aryan racial superiority (Kohl and Pérez-Gollán 2002, 563). In the introduction to his 1935 article "*Die Ergebnisse der Urgeschichtlichen Kulturkreislehre*" (The Results of the Prehistoric Theory of Cultural Circles) Menghin paid tribute to Kossinna, whom he saw as the one who helped the breakthrough of the *Kulturkreislehre*. Menghin states that Kossinna's aim to trace back historical peoples into prehistoric times was correct (Menghin 1935, 72), although he criticised details of his work (Grünert 2002, 124). Menghin's mission, however, was more ambitious; he aimed at a global and universal prehistory. To reconstruct the sequence of cultural history ("*Ablauf des Kulturgeschehens*") and its relationship to language, races and peoples, the method of *Kulturkreislehre* provided the ultimate clues. It is worth noting, however, that prehistoric *Kulturkreise* did not always emerge out of the archaeological record, but the data was pressed to fit the evidence, categories were preconceived, rigid and led to circular conclusions (Kohl and Pérez-Gollán 2002, 574).

Menghin's system of archaeological cultural circles differentiated three Palaeolithic cultural circles: the "*Klingenkulturen*" (blade cultures), the "*Faustkeilkulturen*" (hand-axe cultures) and the "*Knochenkulturen*" (bone cultures) (Menghin 1935, 72). Each of the cultural circles is a compendium of various more or less sharply defined individual cultures. As in ethnology, similarities in cultural elements were not explained as incidental or due to similar developments. Even if they were geographically distant, they were believed to have originated from a common source, spread by diffusion and migration of peoples. Instead of the mechanical, evolutionary model of stages of development in human consciousness, Menghin understood culture in terms of a group-specific process (Kossack 1992, 86). For instance, early Neolithic cultures were divided into circles of cattle, pig- and breeders of riding animals. Various mixtures resulted in ever more complex systems of cultures. Earlier and later cultures were linked, according to Menghin, not only chronologically, but also organically and spatially (Menghin 1935, 76).

The system of prehistoric cultural circles worked in parallel with the results of the ethnological *Kulturkreislehre* in a straightforward way (Menghin 1935, 78). Menghin attempted to prove the existence of a "*Urkultur*" (primary culture) by stating that the global distribution of microlithic stone tools does in fact align with the global distribution of a pygmy primary race (Fuchs 2003, 220). Last, Menghin discussed races which he broadly grouped into white, yellow and black people, assigning outstanding artistic performance, totemism, hunting and stone blades to the white race, cattle-breeding and bone culture to the yellow race and the use of hand-axes to the black race. According to Menghin, race, language and culture corresponded well at the beginning of prehistory, but through the mixing of races things became more complicated. The guiding principle was still the equation that cultures represent peoples. Interestingly, Menghin defined national character ("*Volkstum*") as a somehow harmonised linguistic and racial equivalent of a mixture ("*ein irgendwie harmonisiertes sprachliches und rassisches Mischungsäquivalent*").

Menghin's worldviews run like a thread through his work. He accepted creation and the development of culture and races after the Great Flood as part of his religious views, and was convinced that only the white race was capable of maximising cultural progress, developing and making use of it (Menghin 1935, 80). Atheism, materialism and evolution were what Menghin fought against throughout his life (Kohl and Pérez-Gollán 2002, 564). Although Menghin integrated much of the ethnological *Kulturkreislehre* in his research, others remained more distant from this perspective. The relationship between ethnology and prehistory in Vienna was competitive and problematic, which even led to a complete rift in the late 1930s and 1940s. Different methodological approaches caused insurmountable problems. Prehistoric dating depended to some degree on evolutionary concepts, and according to the *Kulturkreislehre*, there were no early hominids, only modern humans.

Prehistory did not accept the Pygmies to be stuck in a primary stage of culture, but saw them as contemporary rather than prehistoric people (Köb 1996, 96). In contrast to the prehistorians, the ethnologists had a different conception of chronology, less absolute and unilinear, which made communication difficult. Schmidt stated that prehistory did not have any evidence of prelithic cultures, and could not comment on mental, social and religious culture at all, and through its limitation to material culture, prehistory's value for explaining the origins of human civilisation was limited (Schmidt and Koppers 1937, 271). No wonder the prehistorians in the Society for Anthropology[1] were not amused. W. Koppers, who was more diplomatic that Schmidt did not disregard prehistory, but made another dangerous assumption: prehistoric artefacts similar to the ones found in ethnological "primary cultures" suggest a "mental culture (language, family, religion) corresponding to their technology" (Koppers 1952, 31).

After 1945

After the Second World War, the academic landscape changed drastically again; German-nationalists and members of the NSDAP lost their jobs. Koppers was called back and regained his professorship in the Department of Ethnology. Despite the obvious errors of the *Kulturkreislehre*, the ethnological school in Vienna withdrew the concept only after Schmidt's death in 1954 and went on to take a different course. The historical focus remained – "Ethnohistorie", a historical view on ethnology with consideration of space and time, replaced the *Kulturkreislehre* (Wernhart 1994).

In the Department of Prehistory, Richard Pittioni (1906–1985) became Vienna's first post-war professor. The first years of post-war prehistory were dominated by restoring facilities, museums and the protection of monuments. Every association with nationalism was avoided and the focus of research went back to detailed investigation of typology and chronology – a reaction known as "*Kossinna-Syndrom*" in Germany (Smolla 1980). With Pittioni, the theories of the *Kulturkreislehre* were given up. Pittioni underlined that all sources and time are in direct relation to each other; historic situations are unique and a historic dynamic is imminent in all cultures (Pittioni 1954, 78–83).

Pittioni read "*Systematische Urgeschichte*" (Systematic Prehistory), but his main interest was local rather than global, which is reflected by his publication "*Urgeschichte des österreichischen Raumes*" (Prehistory of the Austrian Area) published in 1954 and colloquially referred to as the "*Urbibel*". The monograph is a systematic presentation of Austrian finds with little introduction and interpretation. After the description of each larger chronological entity, a chapter on

[1] The "Anthropologische Gesellschaft", housed in the Museum of Natural History, was probably more inclined to take a more biological, evolutionary view on anthropology than the University.

"*historischer Ablauf*" (The Course of History) followed. He proposed that prehistoric sources could be put together into closed complexes as "*Ausdruck von spezifischen soziologischen Gemeinschaften*" (expressions of specific and sociological communities), which represent "*Reste von gestaltenden Kräften …, die ihrerseits wieder nur von Individuen und Gemeinschaften geboren werden können*" (remains of shaping forces, that can only be born by individuals and communities). On the same page, however, he explained very cautiously and hesitantly that behind these entities are different "Bevölkerungselemente" (elements of people), which can be interpreted as history of peoples (Pittioni 1954, 539).

Kulturkreislehre and *Siedlungsarchäologische Methode*

How far did the concept of the *Kulturkreislehre* differ from archaeological theories elsewhere in Europe? Parzinger argues that in the nineteenth and early twentieth century no broad conceptual differences between German, Scandinavian, British or English Archaeology existed, and archaeology of that time was truly European (Parzinger 2002, 36). I would argue that this might be valid at a very general level only. Theories implied different nuances according to the topics of research as well as the individual researchers and their academic environment, which is linked to their political and religious setting. The cultural historical archaeology was the dominant paradigm, but early functionalist and structuralist approaches were in the stage of development in the 1920s. They did not follow the *Kulturkreislehre's* view that culture is the sum of individual elements that can be observed separately, but saw culture as a system of relationships of technological, social, economic and religious institutions (Kossack 1999, 46; Parzinger 2002, 38).

In a drastically and quickly changing political landscape, the most urgent archaeological questions at the end of the nineteenth and beginning of the twentieth centuries and typical for this era of research were ethnic interpretations of the archaeological record. Among the many scholars who pursued this particular interest, Gustaf Kossinna can be singled out as the most forceful and influential representative in Germany. In many ways, the "*Siedlungsarchäologische Methode*" (Settlement Archaeology) overlapped methodologically with the *Kulturkreislehre* (Bernbeck 1997, 27). Grünert argues that Kossinna might well have been influenced by the German-Austrian *Kulturkreislehre*, but does not explicitly quote them. Kossinna occasionally uses the term *Kulturkreis* as synonymous to culture or even peoples (Grünert 2002, 72).

Kossinna's *Siedlungsarchäologische Methode* was rooted in history rather than ethnography and archaeologically based on Montelius[2] (Baudou 2005, 136), as he states himself in a letter, he "followed the footsteps of the Scandinavian masters" (Schwerin von Krosigk 1982, 168–169). Kossinna wanted to write the history of

[2] Montelius' ethnohistorical methods were based on identifications of object types and their continuity in time and place, he did, however, understand the limits of ethnic interpretations and his conclusions were far more cautious than Kossinna's.

peoples, especially the German people (Gummel 1938, 316–371; Veit 1989, 40–42), wanted to clarify their origins (Kossinna 1911; e.g. *Die Herkunft der Germanen*) and define their geographical boundaries (Kossinna 1896; e.g. *Die vorgeschichtliche Ausbreitung der Germanen in Deutschland*). He argued for an independent cultural development in Northern Europe after the ice ages and strongly opposed any kind of dependency or inferiority of prehistoric Germany to the cultures of the Mediterranean. Roman archaeology, even done in Germany, was begun to be seen as counter-national around the turn of the century (Sklenář 1983, 149).

Kossinna's main guiding principle was that "sharply defined archaeological culture areas correspond at all times to the areas of particular peoples or tribes" (Kossinna 1911, 3; translation in Härke 2000, 44). He tried to link the earliest historical documentations of peoples with distribution maps of the archaeological record, and moving back in time he traced them as far back as the Mesolithic (Kossinna 1911, 29). Specific values were ascribed to different peoples – the Germanic peoples were seen as superior, as "being the bearer of progress and the creators of all great values" (Penka 1907, after Sklenář 1983, 149). It is not surprising that this view was met with approval in Germany's difficult times and opened a door to the political misuse of archaeology.[3]

The main points of contemporary criticism were the lack of any theoretical basis for Kossinna's assumptions and the lack of comprehensible links to archaeological material going beyond some dots and crosses on a map of Europe. Kossinna was critiqued by scholars such as Sophus Müller in Copenhagen, Hans Seger in Breslau, Paul Reinecke in Mainz and Erzam Majewski in Warsaw. The Viennese professor Moriz Hoernes is listed among the critics (Grünert 2002, 117–122). In regard to the 1902 paper "*Die indogermanische Frage archäologisch beantwortet*" Hoernes stated that equating prehistoric pots with historical tribes seems to be a joke, a parody (Baudou 2005, 126). In his polemic style, Kossinna got his revenge by disregarding Hoernes work as a mere gathering of descriptions ("…oder jene Klasse von 'Forschern' in der Art von Moritz HOERNES, deren Forschungstrieb vollauf befriedigt ist, falls sie ihre Fundstücke, oder wenn es hoch kommt, ihre Kulturgruppen mehr oder weniger anschaulich beschrieben haben…") (Kossinna 1911, 13).

Looking into the most influential textbooks for methods and theory for prehistoric archaeology at the relevant time in Germany, one can see how ideas from both directions were mediated and integrated into mainstream research. Karl Hermann Jacob-Friesen was the first to describe the archaeological chronology systematically in his "*Grundlagen der Urgeschichtsforschung*" (Basic Principles of Prehistoric Research). He held three main factors responsible for archaeological phenomena: the natural environment, ethnic constellations and cultural transmission via trade, migration and contact (Jacob-Friesen 1928, 120). The spatial distribution of one type of artefacts was described as a "*Formenkreis*" (circle of forms), and referring to Frobenius, he stated that a *Kulturkreis* is not defined by a single *Formenkreis*, but by the sum of all matching *Formenkreise* of

[3] As discussed, for example, by Härke (2000), Veit (1984), Werbart (1996).

material or immaterial sort (Jacob-Friesen 1928, 138). His aim was "defining as many individual distribution areas of given forms as possible, gathering those together into *Kulturkreise* and establishing their chronological succession" (Jacob-Friesen 1928, 145; after Veit 1989, 41). In contrast to Kossinna, Jacob-Friesen paid attention to an evaluation of distribution maps, saw some types as more chronologically relevant than others, and differentiated between regional and inter-regional types (Jacob-Friesen 1928, 174, after Baudou 2005, 132).

Shifting from material culture to forms of practice, Hans Jürgen Eggers proposed the mapping of burial and deposition customs, which he found more relevant in terms of determining the settlement area of certain tribes and peoples than the sum of many maps of types (Eggers 1937, after Eggert 2001, 278). He argued that the isolated recognition of cultural elements and their spatial distribution is not enough and instead functional relationships between them should be explored (Eggert 2001, 281). He thoroughly worked through and criticised Kossinna's method, especially in terms of the questions that were asked. The ethnic interpretation became just "one among many possibilities" (Eggers 1950, after Carnap-Bornheim 2001, 183). Eggers' "*Einführung in die Vorgeschichte*" (Introduction to Prehistory) was first published in 1959 (Eggers 1986).

Because of overlapping themes and methodologies, it is difficult to pinpoint the differences between *Siedlungsarchäologische Methode* and *Kulturkreislehre* precisely and particularly the links between Kossinna and *Kulturkreislehre* need to be explored further. Much of the variations and nuances in the interpretation of archaeological remains depend on the individual scholars of the time and their political views, and Kossinna and Schmidt probably represent the most extreme divergence in opinions. In summary, one can say that an evolutionary approach and the tracing back of specific peoples far back into prehistoric times is typical for the *Siedlungsarchäologische Methode*, whereas the *Kulturkreislehre* is essentially creationist and starts with a primary culture where all significant elements were already in existence. Cultures were believed to remain constant, unless cultural contact and fusion enabled innovation and cultural change.

The main aim of the *Kulturkreislehre* was the description of the cultural history of the world, whereas the *Siedlungsarchäologische Methode* focused on Europe and on proving the German race's superiority. The dangerous attempt to ascribe values to peoples, cultures and races was done by both, albeit in quite different ways, grounded in different political values and resulting in divergent conceptualisations of nature and culture.

What is Left, What is Right? Remnants of the *Kulturkreislehre* Today

The global, systematic and abstract model of world cultures the *Kulturkreislehre* presented both in ethnology and archaeology did not withstand the test of time and proved to be incorrect. The concept, however, was influential in the training of generations of prehistorians in Central Europe and shaped the way cultures and peoples were thought of for decades. Menghin influenced archaeologists in Argentina (Kohl and Pérez-Gollán 2002, 561) and almost all Austrian lecturers have studied with one of Menghin's pupils or heard their lectures (Urban 1997, 11). Thinking about archaeological cultures in terms of circles on a map has remained rather common among Central European archaeologists.

The use of the term *Kulturkreis* on its own, however, does not always integrate all that is implied by the theoretical paradigm of the *Kulturkreislehre*. The term *Kulturkreis* is sometimes merely used to refer to the distribution area of artefacts, an "archaeological culture". Culture, however, cannot be imagined without people – although post-war prehistorians were in fact very cautious about naming peoples, it led to rather weird constructions like "*Träger der Streitaxtkulturen*" (bearers of the hand-axe cultures) or simple "*Bandkeramiker*" (linear band people) (Bergmann 1974, 129–138). Even though names of peoples were not spelt out explicitly, concepts of peoples and tribes remained behind the concept of archaeological cultures (e.g. Veit 1989, 50; Kossack 1999, 3). The possibility of explaining the archaeological record considering ethnic interpretations was ignored or worked around, until the post-war generation could discuss the concepts and return to ethnic interpretations open-mindedly, free from Kossinna's legacy (Smolla 1980, 9). In an attempt to redefine ethnicity in early mediaeval archaeology, Brather has recently discussed the origin and use of crucial terms use in the German discourse like "*Volk*" (people), "*Kultur*" (culture), "*Rasse*" (race) and "*Sprache*" (language). Whether "archaeological cultures" are just a descriptive aid for classification of archaeological material or if they do – against all odds – reflect some kind of reality, concrete communities in time and space, are questions still vividly debated (Brather 2004, 52–76). Cultures and *Kulturkreise* "won't go away" because theories are not developed in a void, but informed by and developed from the archaeological material emerging from a particular context.

An example of where different material culture and practices were probably deliberately used to create group boundaries is middle Bronze Age Hungary. In order to investigate practices around the body in cremation and inhumation graves, Marie Louise Stig Sørensen and I chose to compare small groups in close geographical proximity with distinct funerary characteristics: in the Encrusted Ware Culture scattered cremations predominate, in the Vatya Culture urn burials were used and in Füzesabony inhumation is most common. The groups seem to use burial practices informed by their lifestyle – their subsistence patterns, material emphasis and organisation of settlements. The differences in burial practices seem to be expressions of a deliberate emphasising of difference. Group identities may be deliberately maintained and funerary practices probably centre staged in the construction and maintenance of distinct identities (Sørensen and Rebay 2008b). If differences in practice and material culture are traceable in the archaeological record and suggest maintained borders and boundaries between groups, it is relatively easy to map the phenomena and draw circles around them. Inconsistencies in this model can then be relatively easily explained through exchange between groups, or, more violently, through battles, raids and mutual conquest of sites (e.g. Bóna 1975).[4]

The Hallstatt Culture is an excellent example of how much particular strands of the *Kulturkreislehre* are still intermingled in the language of archaeological research. Research at Hallstatt began as early as 1846 with the discovery of a large early Iron Age cemetery connected to a prehistoric salt mining population; well documented, systematic excavations followed immediately. Already the very early attempts to date and classify the eponymous cemetery of Hallstatt were based on ethnic concepts. In the absence of an established chronological framework for dating, classical authors were consulted for clues. A constant confusion of dating and cultural classification is typical for archaeology influenced by the *Kulturkreislehre*; sites and, in particular, cemeteries were often dated as whole, rather than dating individual graves with the result that whole cemeteries were taken as representative for a phase instead of being ordered into a succession. Rather than acknowledging that societies can change and develop (and thus result in cemeteries of multiple phases), a static model of culture was assumed.

The first classification of the cemetery in 1848 as pre-Roman "Celtic" was built on the arguments that the lack of weapons implied it was not Germanic, and the lack of coins suggested it was also not Roman (Gaisberger 1848). The Three Age System could have helped clarifying its position – the Three Age System was, however, eyed with scepticism in southern Germany and Austria until well into the late nineteenth century. Von Sacken rejected a classification of cemetery finds on the basis of materials. He described the mixture of bronze and iron finds as characteristic (Sacken 1868, 129), and explained the use of both cremations and inhumations on one cemetery as caused by a mixed population of Etruscans and Celts (Sacken 1868, 146). Following a similar logic to the distribution of materials and artefacts, practises like inhuming or cremating bodies after death were equally seen as mutually exclusive and assigned to one or the other people, before it was recognised that both Etruscan and Celtic people changed their preferred burial practice through time.

One of the reasons why the Three Age System was first rejected in Central Europe is probably the complex character of Central European archaeology: few single occupation Bronze Age sites were known at the time, which would suggest a "pure Bronze Age" in the same clarity as in northern Germany and Denmark, but a number of late Bronze Age cemeteries contained iron as well as bronze. Another reason were technological concerns: it was doubted that holes could be made into stones or bronzes decorated without using metal tools (Sklenář 1983, 88, 118), which would suggest both materials were in use at the same time. As a consequence, it was much easier to think of Hallstatt and comparable contemporary sites as a *Kulturkreis* rather than being integrated into the more

[4] Militaristic language was typical for the war and post war period, and due to popular and nationalistic concerns, some of traditional Hungarian archaeology has been overshadowed by the archaeology of the Hungarian Conquest period.

evolutionary Three Age System (Sørensen and Rebay 2008a). The differences in material culture in the area of the Hallstatt Culture called for a division: this division into "*Westhallstattkreis*" and "*Osthallstattkreis*" was already envisioned in Otto Tischler's analysis of the cemetery of Hallstatt in 1881, implemented by Moritz Hoernes for a wider area and became geographically fixed by Georg Kossack in 1959 (Weiss 1999, 11). Main differences were recognised in weaponry (swords and daggers in the west, lances, axes and armour in the east), pottery decoration styles, burial traditions (inhumations were preferred in the west, cremations in the east) and settlement patterns. The relatively uniform and homogeneous western *Kreis* had to be matched up to an eastern *Kreis*, although Müller-Scheeßel has recently argued that similarities within the eastern *Kreis* are few and an opposition was simply created to match expectations (Müller-Scheeßel 2000). In fact, the "*Osthallstattkreis*" is a conglomerate of groups with quite different economic backgrounds, material expressions and burial as well as settlement traditions. The common division into "*Westhallstattkreis*" and "*Osthallstattkreis*", with the eponym site Hallstatt neatly situated at the overlapping point of the circles in the middle is deeply ingrained and repeated until today (e.g. Sommer 2006; Torbrügge 1991, 1992; Urban 2000), although most scholars today are aware of the relative arbitrariness of the concepts. Furthermore, the extension of the Hallstatt Culture as a whole became a model to describe the distribution of the preceding late Bronze Age "Urnfield Culture", despite the fact that the cremation of human bodies and subsequent burial in urns became a much more widespread practice in the European late Bronze Age.

Conclusion

The *Kulturkreislehre* (theory of cultural circles) was an influential theoretical concept at the end of the nineteenth and during the first half of the twentieth century in Central Europe. The rejection of evolution and the belief that migration and contact between cultures were the primary triggers of change explains why concepts of static rather than dynamic archaeological cultures continue to be dominant in Central European prehistory. The return to a rather descriptive, classificatory and technocratic archaeology without much interpretation with implicit rather than explicit theory left residual ideas unchallenged. Remnants of the *Kulturkreislehre* still influence the way in which the archaeological record and archaeological cultures are thought about and dealt with. However, an awareness of past theoretical concepts, including a history of how ideas where shaped and by whom, may help us to move forward. This is necessary to estimate the validity of concepts of archaeological cultures, to recognise splitting and lumping due to ideology and national concerns rather than archaeology, and to identify soft and hard cultural boundaries beyond simple topographical models. Archaeological cultures may be fiction, but group identities in prehistory are not.

Acknowledgements I would like to thank Otto Urban, Marie Louise Stig Sørensen and Roderick Salisbury for a number of interesting discussions about research history and feedback on my reflections on the *Kulturkreislehre*.

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Chapter 4
Cultural Innovation from an Americanist Perspective

Michael J. O'Brien

Introduction

No matter how wide a search one might conduct, it would be difficult to find another topic in anthropology that has played as an important a role as innovation in framing arguments about why and how human behavior changes (O'Brien 2007; O'Brien and Shennan 2010). Clearly, innovation was implicit in the nineteenth century writings of ethnologists, such as Tylor (1871) and Morgan (1877), both of whom viewed the production of novelties – new ideas, new ways of doing things, and the like – as the underlying evolutionary force that keeps cultures moving up the ladder of cultural complexity. From their point of view, the vast majority of cultures that have ever existed pooped out somewhere on the way up – presumably because they either ran out of good ideas and products or were too set in their ways to borrow them from other cultures. A few were innovative enough to escape the lower rungs and develop into civilizations through the acquisition of traits, such as writing, calendars, and monumental architecture.

Innovation was an equally important component of the work of later cultural evolutionists, such as Steward (1955) and White (1959). For them, the evolutionary process was perhaps less directional and goal-oriented than it was for the earlier evolutionists, with the source of innovation wrapped up in the kind of mechanisms a group needs to meet the challenges of its physical and social environment. For Steward especially, innovations were viewed as adaptations – traits invented or borrowed to better acclimate groups to their physical and cultural environments. This was not an unreasonable view for someone whose early career was built on studying groups living in the rugged, semiarid Great Basin of western North America (e.g., Steward 1938).

Ethnologists are not the only social scientists interested in the processes by which humans acquire cultural traits. A recent workshop at the Santa Fe Institute

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B.W. Roberts and M. Vander Linden (eds.), *Investigating Archaeological Cultures: Material Culture, Variability, and Transmission*, DOI 10.1007/978-1-4419-6970-5_4,

centered on innovation, building on the work of economist Schumpeter (1934), who made the distinction between *invention* – the creation and establishment of something new – and *innovation* – an invention that becomes economically successful and earns a profit (see Erwin and Krakauer 2004). Translating Schumpter's notions into biological terms, an invention is any trait that appears, and an innovation is a trait that at some point comes under selective control. In other words, an innovation is an *adaptation* – a character that affects the fitness of its bearer. Put more correctly, the *absence* of the character has a negative effect on the fitness of an organism.

In the remainder of this chapter, I touch upon the production and spread of cultural innovations, or what are often termed in ethnology and archeology "culture traits," "features," or "characters." Certainly, those are the labels that are familiar to most ethnologists and archeologists. I use examples drawn from what I know best, which is a mix of American ethnology and archeology and evolutionary science. My time frame is roughly from 1900 to the present – a span that precludes my being able to do justice to the expansive literature on the subject of cultural innovation.[1] Rather, I hope to provide a glimpse at how perspectives and research questions have evolved. Interestingly, terms have changed, and analytical methods have matured, but the basic questions have pretty much remained the same.

Cultural Innovation in Historical Perspective

Even a brief perusal of the American literature of the last hundred-plus years shows that ethnological and archeological explanations of cultural change have centered on the introduction and spread of novelties. American culture historians of the twentieth century routinely looked to diffusion and trade as a source of innovations, borrowing without comment the models of their ethnological colleagues. Sometimes, innovations were viewed as having been borrowed – often from incredible distances and by incredible means (e.g., Ford 1969; Meggers et al. 1965) – and other times they were viewed as products of what Adolf Bastian referred to in the mid-nineteenth century as the "psychic unity of mankind" (Lowie 1937). Tylor (1871) favored that explanation for the majority of cultural similarities he viewed in the ethnological record. There was a third alternative, and it was manifested most clearly in the work of Steward (1955) – what became known as *multilinear evolution*. It is worth a bit of scrutiny because of its significant and long-lasting effect on American archeology.

Steward asked why, for example, did many of the same culture traits occurring within, say, a patrilineal hunting-and-gathering group in West Africa also occurs

[1] See Lyman (2008) for a detailed discussion of the early history of cultural-transmission studies in ethnology and archaeology, Lyman and O'Brien (2003) for a similar history of work on the

within a hunting-and-gathering group in the Great Basin? Obviously, these two groups were not phylogenetically related, so the similarities must be the result of something else. The environments in which they live are distinctive in terms of terrain, vegetation, and rainfall, so it makes no sense to say that the groups were similarly adapted to similar environments. For Steward, the answer resided in what he termed the *cultural cores* of the groups – similar solutions not to similar environments but to similar environmental *problems*. Those problems transcended the actual kind of environment in which a group lived. Thus, a hunter–gatherer in West Africa and one in the Great Basin might well face exactly the same economic problems and develop the same kind of kinship system, technology, and social hierarchy – similar solutions to similar problems – despite exploiting entirely different resources.

Although his emphasis was on the technological aspects of a culture, Steward also included "such social, political, and religious patterns as are empirically determined to be closely connected" with the core (Steward 1955, 37). Radiating out from the core were "secondary features" that are "determined to a greater extent by purely cultural–historical factors – by random innovations or by diffusion – and they give the appearance of outward distinctiveness to cultures with similar cores" (Steward 1955, 37). Clearly, Steward was arguing that if the ethnologist (or archeologist) could determine which traits were at the core of a culture and which ones were secondary, then the traits could be used to assess the degree of cultural relatedness between that culture and others. The more core traits that two cultures possess, the more phylogenetically related they are. If two cultures hold few or no traits in common, then either the cultures are unrelated or they were once related but at such a distant point in the past that the phylogenetic signal has all but disappeared.

This argument was not new; ethnologists – Boas (1904), Wissler (1917, 1923), and Kroeber (1923, 1940), for example – had long used trait similarity as a measure of culture relatedness, and the method had passed into archeology in the form of what became known as the *direct historical approach* (Steward 1942). The method was classically used by Thomas (1894) to debunk the American moundbuilder myth in the 1880s (O'Brien and Lyman 1999) and later by Strong (1935) and Wedel (1938) on the Great Plains, not only as a means of tracking the passage of time, but also for identifying the ethnicity of the people responsible for the artifact assemblages (Lyman and O'Brien 2001). The analytical protocol of the direct historical approach was simple. To trace connections, one began with the most recent, or historically known, culture traits and then worked backward in time, using similarity in traits as the basis for putting assemblages closer together or farther apart in time.

Despite the widespread use of culture traits as measures of relatedness or of functional convergence, there was less emphasis on trying to figure out exactly what *is* a cultural trait. Most researchers assumed that such traits are mental phenomena that one acquires through teaching and learning, but no one presented an explicit theoretical definition of a cultural trait. This was highly problematic and meant that the units varied greatly in scale, generality, and inclusiveness (Lyman 2008). There were numerous efforts to resolve the difficulties of classification and scale, but they did little to resolve the issue. Let us look briefly at how A. L. Kroeber approached the problem. I use Kroeber as an example because he arguably had more of an impact on the study of innovation and its spread than any other anthropologist, especially through his *Culture Element Distribution* studies, which he carried out during the 1920s with his students at the University of California, Berkeley. Any modern study of innovation faces the same conceptual and methodological hurdles that Kroeber did, and in some respects the manner in which he addressed them surpasses much of what is seen in the modern literature.

Kroeber (1935, 1) noted that with respect to culture elements, "the question of first importance is whether the elements operated with are justifiable units." He further noted that three conditions had to be satisfied to answer that question affirmatively: "First, the elements must be sharply definable. Second, they must be derived empirically, not logically. And third, they must be accepted for use without bias or selection" (Kroeber 1935, 1). These are problematic for any number of reasons, a point that was not lost on Kroeber or the myriad of other ethnologists and archeologists who examined innovation. Workable solutions, however, were difficult to come by. Taking Kroeber's three conditions in reverse order, condition number three, that traits must be accepted for use without bias or selection, which is the least problematic of the three, provided that traits can be identified in the first place. It also presumes that analysis is statistical and based on probabilistic sampling, which is what Kroeber was interested in.

Condition number two, that traits must be derived empirically as opposed to logically, means that the units are pulled directly from the traits themselves as opposed to being imposed on them by the analyst. Here, Kroeber failed to keep distinct the description of an empirical unit and the definition of a measurement unit. The former could comprise any set of one or more characters, whereas a definition would comprise only the necessary and sufficient conditions for the identification of an item as a member of a particular ideational unit (Dunnell 1986). The conflation of empirical units (things) and measurement units remains a serious problem in anthropology (Lyman and O'Brien 2003; O'Brien and Lyman 2002).

As problematic as condition two might be, it was Kroeber's first condition that traits must be "sharply definable" that causes perhaps the greatest concern. What does "sharply definable" mean? In Kroeber's day rarely was there consensus, with individual researchers simply defining traits on an ad hoc basis. There was almost universal acceptance that traits could be defined at various scales (Lyman 2008), but there was a decided lack of unanimity over how to scale them. Things are no different today. In a review of a paper published by three of Kroeber's students in which they applied Chi-square analysis to a set of culture traits from several Polynesian islands (Clements et al. 1926), Wallis (1928) noted that traits should be scaled using the terms "generic" and "specific." Wallis believed that a generic trait, whether technologically complex or not, was likely to have a wide geographic distribution precisely because it was generalized and inclusive. He also believed that technologically complex traits were likely to be invented only once and thus their distribution was a result of diffusion. The examples he used were myths and radios: "myth-making is a universal culture process, whereas radio-making has been limited to a single invention" (Wallis 1928, 95).

Clements (1928, 302) responded in exactly the manner that any modern biologist would, pointing out that a "generic trait" often tends to be composed of simpler traits and that a complex trait may in turn be part of a still larger trait complex; "thus it will be seen that unless we are dealing with the simplest units, the question of what is or is not generic is quite relative." But then he added, "the use of generic traits as such, then, is not to be recommended, and in the statistical method it is essential for all traits to be reduced to their simplest elements. That is to say, the sample must consist of *specific* traits only" (Clements 1928, 302). Clements never really addressed how to ensure that only specific traits were being examined, nor did anyone else.

And what about the issue of trait dependence/independence, which is something that biologists routinely deal with? Clements and others might have preferred dealing only with specific traits, but what if traits were not only *transmitted* as packages but *arose* as packages? This was rarely addressed, although Driver and Kroeber (1932) tried to do so in an important paper, "Quantitative Expression of Cultural Relationships." They asked if the traits they were using to determine cultural relationships among California groups were independent or whether they were linked into larger packages. Their answer was that "while we are not prepared to answer this question categorically, we believe that culture traits are in the main if not in absolutely all cases independent" (Driver and Kroeber 1932, 212–213). But then in a footnote they pointed out that this independence is

> within the limits of ordinary logic or common sense. Essential parts of a trait cannot of course be counted as separate traits: the stern of a canoe, the string of a bow, etc. Even the bow and arrow is a single trait until there is question of an arrow-less bow. Then we have two traits, the pellet bow and arrow bow. Similarly, while the sinew backing of a bow cannot occur by itself, we legitimately distinguish self-bows and sinew-backed bows; and so, single-curved and recurved bows, radically and tangentially feathered arrows, canoes with blunt, round, or sharp sterns, etc. (Driver and Kroeber 1932, 213)

What can we make of all this? For one thing, if some of the best minds in the formative years of American ethnology and archeology had a tough time identifying what a culture trait entailed, there is every reason to suspect that the whole concept is more complicated than it might seem at first glance. Did things become less complicated during the second half of the twentieth century? Not by my read. Ethnologists for the most part drifted away from emphasizing culture traits and put more emphasis on cultures as wholes, leaving traits to their archeological colleagues to worry over. Despite any number of archeological classificatory schemes that made use of culture traits (e.g., McKern 1939; Strong 1935; Wedel 1938; Willey and Phillips 1958), there was little consensus on exactly what a trait was. As a result, traits were ad hoc constructions that varied tremendously in scale, often making it impossible to compare results.

Biologists might be quick to point out that there are also procedural problems in their discipline, where there is no standard set of characters used in the creation of taxa, but I would argue that the situation is murkier in anthropology. Biologists, for example, learn early in their training the difference between a character and a character state, but the distinction is made much less frequently in anthropology. The one place where I think anthropologists *have* made insightful comments is with respect to what early in the twentieth century became known as *trait complexes* – minimally defined as "groups of culture elements that are empirically found in association with each other" (Golbeck 1980). More specifically, most researchers (e.g., Wissler 1923) defined a trait complex as a collection of traits that are *functionally* interrelated. Although ethnologists used trait complexes as another means of comparing cultures, the concept "trait complex" has a role to play in modern cultural evolutionary analysis, if for no other reason than it reminds us that cultural phenomena evolve as complex wholes, not as tiny parts. Selection can, and often does, act as a tinkerer – and "one who does not know exactly what he is going to produce but uses whatever he finds around him" (Jacob 1977, 1163) – but it is the "cascading" effects (Schiffer 2005) of that selection that is important (O'Brien and Shennan 2010).

Cultural Transmission: The Spread of Innovation

From the beginning, regardless of how ethnologists and archeologists viewed culture traits, and irrespective of their arguing over whether a particular trait was transmitted vertically (cultural ancestor to cultural descendant) or horizontally (cultural group to unrelated cultural group), there was complete agreement that traits, like culture itself, were acquired, not inherited. Kroeber (1923), for example, explicitly distinguished between the transmission of genes, which involves heredity, and the transmission of culture, which involves acquisition and learning. For Kroeber (1923, 3), "heredity is displaced by tradition, nature by nurture." In his view, tradition involves a "non-biological principle" because biological transmission is limited "only to blood descendants," whereas cultural transmission can be between "individuals and groups not derived by descent from" the originators of the cultural trait being transmitted (Kroeber 1923, 7). In fact, he went on to note, cultural transmission can be from genetic descendant to genetic ancestor and further, cultural transmission does not necessarily produce change in the sense that the genotype of a descendant differs from that of its ancestor, but rather the results of cultural transmission involve "accretion to the stock of existing culture" (Kroeber 1923, 7).

This is true – cultures, however defined, take on culture traits, adding them to the repertoire of already acquired traits. Of course, organisms do this too, but let's not quibble with Kroeber on this point and instead focus on a problem with his statement "accretion to the stock of existing culture." The problem is that Kroeber apparently overlooked the fact that cultures lose traits in addition to "accreting" them. To him, once the cultural stock was formed – similar to Steward's (1955) "core" – it became simply a matter of hanging ornaments on it. But cultures aren't stable; rather, they are constantly evolving amalgams of traits at every conceivable scale. Cultural transmission assures that this is the case. Traits are acquired, and traits are lost, all at a dizzying pace and through a variety of processes. To Kroeber, though, and others both before and after him, what really mattered was diffusion – the sharing of ornaments across the cultural landscape. Diffusion became synonymous with transmission, or, more precisely, transmission *and* acceptance (Koppers 1955).

That cultural transmission does not involve change in a finite number of traits comprising a culture (as opposed to an organism), but instead cumulative growth in the number of traits held by a population of humans, was a recurring theme in the early twentieth century (Lyman 2008; Lyman and O'Brien 2003). This, of course, in no way precludes the application of Darwinian principles to the study of cultural features, although anthropologists and archeologists have fought mightily for over a century to keep biology and culture separate. There were numerous early uses of evolutionary terms in American archeology (e.g., Colton 1939; Colton and Hargrave 1937; Gladwin and Gladwin 1934; Kidder 1915; Kidder and Kidder 1917), but they were founded in a very basic, common sense understanding of biological evolution. The lack of development of an archeological theory of cultural evolution resulted in the largely trial-and-error construction of the units employed to establish temporal control over assemblages of artifacts (Lyman and O'Brien 2006; Lyman et al. 1997). That such units – once tested for their temporal sensitivity – may or may not also reflect ancestral–descendant relationships between them was recognized by some (e.g., Ford 1940), but no one really knew how to construct units that clearly would reflect such relationships. The door was finally slammed shut on the use of biological principles to help understand cultural evolution when Brew (1946, 53) declared that "phylogenetic relationships do not exist between inanimate objects."

Brew, of course, was correct: tools do not breed. But tool makers *do* breed, and they *do* transmit information to other tool makers, irrespective of whether those other tool makers are lineal descendants. Transmission, particularly between parents and offspring of the same sex (Shennan and Steele 1999), creates what archeologists have long referred to as tool *traditions* – patterned ways of doing things that exist in identifiable form over extended periods of time. It seems naive, given what we know of the archeological record, not to believe that tool forms are modeled on preexisting forms. Further, cultural phenomena are parts of human phenotypes in the same way that skin and bones are, and as such they are capable of yielding data relevant to understanding both the process of evolution and the specific evolutionary histories of their possessors.

But that is a modern view and not one held throughout much of the twentieth century. Not only was there a wide gulf between such things as pots and bones, there were completely different views on the shape of biological and cultural evolution, the former portrayed as diverging and the latter as being simultaneously diverging *and* highly reticulate, running like a braided stream in channels that are constantly diverging and converging. This view prompted Kroeber's (1948) metaphor of biological evolution as a tree with ever-diverging branches and cultural evolution as a tree with tangled branches. Without clear, unequivocal, and irreversible divergence, how could one hope to trace ancestry except in the most superficial way? Perhaps a trait could be traced back in time, but how did it relate phylogenetically to other traits? What Kroeber ignored – and he subsequently was joined by generations of anthropologists – was over a century of work in historical linguistics, which showed that it was indeed possible to trace the ancestry of languages, despite borrowing and reverse borrowing. Borrowing does not create a "hybrid" culture or language (Goodenough 1997).

With the growing interest in Darwinian evolution that became noticeable in anthropology and archeology after around 1980 (e.g., Dunnell 1980), researchers began to reconsider the role of innovation in the evolution of cultural systems. Importantly, evolutionary research in the social and behavioral sciences in general began to be geared toward identifying innovation not only as a "thing," but also as a "process." Considerable interest was focused on cultural transmission (e.g., Boyd and Richerson 1985; Cavalli-Sforza and Feldman 1981; Cloak 1975; Durham 1991; Henrich and Boyd 1998; Lumsden and Wilson 1981; Richerson and Boyd 1992), but despite this interest, we are still left with the questions, "What, exactly, *is* the unit of cultural transmission, and how would we know if we found it?" (Pocklington 2006) Various researchers have proposed names for these units – *menemotype* (Blum 1963), *sociogene* (Swanson 1973), *instruction* (Cloak 1975), *meme* (Aunger 2002; Blackmore 1999, 2000; Dawkins 1976), and *culturgen* (Lumsden and Wilson 1981) – but there is little consensus as to what the units embody, similar to the earlier situation with culture traits.

Some researchers have suggested that perhaps we don't need a consensus. In one of the most fully developed discussions of cultural transmission, Boyd and Richerson (1985, 37–38) indicate that they "do not assume that culture is encoded as discrete 'particles'" and that "it is possible to construct a cogent, plausible theory of cultural evolution without assuming particulate inheritance." Not all researchers would agree; Aunger (2002), for example, argues that memes do have a physical basis. If Boyd and Richerson are correct, however, and I believe they are, this is good news for those of us interested in cultural evolution because we can get on with the important issue of where the units that get culturally transmitted come from in the first place (O'Brien and Shennan 2010; Bentley et al. 2011).

Just because the units of cultural inheritance are not particulate in the same way genes are, it does not mean that biology is incapable of offering helpful analogues when it comes to understanding the production and transmission of novelties (Eerkens and Lipo 2007; Shennan 2002b). And to be clear, the analogues are just analogues, not metaphors. In a recent paper published in *Behavioral and Brain Sciences*, Mesoudi et al. (2006) argue that we can take advantage of the analogues between cultural and biological evolution in order to model the structure of a science of cultural evolution after the structure of the science of biological evolution. In brief, if both cultural and biological changes are governed by the same underlying Darwinian processes of variation, differential selection, and the inheritance of selected variants, then the cultural and biological sciences should broadly share the same methodological and conceptual divisions.

Innovation, then, becomes a key area of analytical focus, especially with respect to the form of the innovation and the process that creates it in the first place. It is one thing to know how and under what conditions a trait is transmitted, but it is a different matter to understand where it came from. Even more important is the understanding that especially with respect to cultural transmission, which is exponentially faster and has less fidelity than biological transmission, the transmission process itself is a continuous creator of innovation. Much more so than I think is the case in biology, tempo and mode interact in cultural situations to create a new source of innovation and to create it at scales much larger and more complex. This is an exciting area of research for those interested in niche-construction theory as it pertains to humans (Bleed 2006; Laland and O'Brien 2010; Laland et al. 2001; Odling-Smee et al. 2003).

It might be useful in this context to think of cultural traits as "recipes" (Lyman and O'Brien 2003; Neff 1992). These comprise the materials required to construct a tool, for example (the "ingredients"), and the behavioral rules required to construct and use the tool (the "instructions"). Similarly, cognitive psychologists (e.g., Weber et al. 1993) have proposed that people represent tools as interlinked, hierarchical knowledge structures, incorporating behavioral scripts governing their construction and use, much like the recipe concept. Biologists, too, use the "recipe" metaphor to describe the development of organisms from genetic information (Dalton 2000; Ridley 2003). In archeology, the potential exists to move beyond metaphors and incorporate behavioral data from ethnographic studies of tool construction and use, psychological data regarding the representation of tool knowledge in the brain, and archeological data regarding the evolution of tools (Mesoudi and O'Brien 2008c) – topics that have everything to do with the production and spread of innovations.

Boyd and Richerson's collective work (e.g., Boyd and Richerson 1985; Bettinger et al. 1996; Richerson and Boyd 1992), often referred to as "dual-inheritance theory" (Shennan 2002a), is useful here. It posits that genes and culture provide separate, though linked, systems of inheritance, variation, and evolutionary change. The spread of cultural information is viewed as being affected by numerous processes, including selection, decision making, and the strengths of the transmitters and receivers. But there is much more to their work than how and why traits spread. Their work also demonstrates that some innovation is produced through the intricacies of the transmission process itself – hence my earlier comment about the relevance of niche-construction theory.

One illustration of Boyd and Richerson's models of cultural transmission is Bettinger and Eerkens's (1999) analysis of stone projectile points from the Great Basin. There the bow and arrow replaced the atlatl around AD 300–600 – a replacement documented by a reduction in size of projectile points. The weight and length of points manufactured after AD 600, however, was not uniform across the region. Rosegate points from central Nevada vary little in weight and basal width, whereas specimens from eastern California exhibit significant variation in those two characteristics. Why the differences, and what do they tell us, if anything, about the production and spread of innovations?

Bettinger and Eerkens proposed that the variation is attributable to differences in how the inhabitants of the two regions obtained and subsequently modified bow-related technology. In eastern California, bow-and-arrow technology was both maintained and perhaps spread initially through what Boyd and Richerson (1985) refer to as *guided variation*, wherein individuals acquire new behaviors by copying existing behaviors and then modifying them through trial and error to suit their own needs. Conversely, in central Nevada, bow-and-arrow technology was maintained and spread initially through *indirect bias*, wherein individuals acquire complex behaviors by opting for a single model on the basis of a particular trait identified as an index of the worth of the behavior. Bettinger and Eerkens proposed that in cases where cultural transmission is through guided variation, human behavior tends to optimize fitness in accordance with the predictions of the genetic model – individual fitness is the index of success, with little opportunity for the evolution of group-beneficial behaviors. In instances where transmission is through indirect bias, which tends to produce behaviorally homogeneous local populations, conditions may be right for the evolution and persistence of group-beneficial behaviors.

From the standpoint of the study of innovation, the models present widely differing scenarios. In both, individuals copy existing behaviors wholesale – innovations can suddenly "appear" in a new region as large, complex packages (projectile points, for example) – but in guided variation individuals begin tinkering with certain aspects whereas in indirect bias they do not. Under perhaps extreme conditions individuals may not even be aware of the underlying principles of how and why something works. All they know is that it *does* work, and they reproduce it wholesale. Of course, the copying process itself is rarely faithful, thus presenting plenty of chance for copying errors, which themselves are novelties. Whether or not the errors are reproduced is a separate matter entirely.

A few years ago, Alex Mesoudi and I realized that to our knowledge, no experimental studies had attempted to simulate the cultural transmission of prehistoric tools, which the models of Boyd and Richerson (1985) and others, and the analyses of Bettinger and Eerkens (1999), suggest played an important role in generating systematic patterns in the archeological record. Theoretical models are wonderful things, and applications of the models to actual data are why we do science, but controlled "middle-range" experiments provide the necessary bridge between the two (Mesoudi 2008). In that vein we designed an experiment to examine the cultural transmission of projectile-point technology, simulating the two transmission modes – indirect bias and guided variation – that Bettinger and Eerkens (1999) suggested were responsible for differences in Nevada and California point-attribute correlations (Mesoudi and O'Brien 2008a, b).

In brief, groups of participants designed "virtual projectile points" and tested them in "virtual hunting environments," with different phases of learning simulating indirectly biased cultural transmission and independent individual learning. As predicted, periods of cultural transmission were associated with significantly stronger attribute correlations than were periods of individual learning. This obviously has ramifications for how we look at innovation. In simplified terms, the more "loners," the more innovation; the more group-oriented individuals who want packages off the shelf, the less innovation (O'Brien and Shennan 2010). The experiment and subsequent agent-based computer simulations showed that participants who could engage in indirectly biased horizontal cultural transmission outperformed individual-learning controls (individual experimentation), especially in larger groups, when individual learning is costly and the selective environment is multimodal (Mesoudi and O'Brien 2008a, b).

Cultural transmission in a multimodal adaptive landscape, where point-design attributes are governed by bimodal fitness functions, yields multiple locally optimal designs of varying fitness. Our experimental results supported this argument, with participants in groups outperforming individual controls when the group participants were permitted to copy each other's point designs. Computer simulations confirmed that this social learning strategy of "copy-the-successful" was more adaptive than a number of other social learning strategies, especially in larger groups of more than 50 people, which have been typical throughout much of human evolution (Dunbar 1995), and showed that the multimodal adaptive landscape assumption was key to this advantage.

This latter finding is potentially important to the production of innovation, as it demonstrates that the nature of the selective environment significantly affects the aspects of cultural transmission. Whereas previous experiments (e.g., McElreath et al. 2005) have used relatively simple learning tasks requiring a participant to select one of two options (e.g., crops or rabbit locations), Mesoudi and I used a more complex learning task involving multiple continuous and discrete functional and neutral attributes, some of which have bimodal fitness functions. The resulting multimodal adaptive landscape was instrumental in generating and maintaining diversity in the virtual-point designs.

We also found that the "copy-the-successful" strategy outperformed the "copy-the-majority" strategy. Indeed, the latter performed no better than individual learning because individuals are just as likely to converge on a local optimum as a global optimum in the absence of information regarding the success of those individuals (unless individuals at the global optimum outcompete individuals at the local optima and become the majority). This finding contrasts with previous models that suggest that conformist transmission is adaptive under a wide range of conditions (Henrich and Boyd 1998), possibly because those models assume that individuals exhibit only one of two behaviors, one of which has a higher payoff.

Conclusions

I doubt we could ever find a work by Kroeber that included the terms "conformist transmission" or "adaptive landscapes," but even a brief perusal of the extensive literature on culture traits makes it clear that anthropology has long had an interest in identifying units of cultural transmission and using them to examine the various modes that humans have evolved to transmit information among themselves. That history also reveals not only the roots of modern theoretical difficulties with identifying units of cultural transmission but also some of the properties that such a unit needs to have if it is to be analytically useful to theories of cultural evolution. Given the exponential growth in the literature on both the units of transmission and the processes through which information is transmitted and received (e.g., O'Brien et al. 2010; Rendell et al. 2010, 2011; Whiten et al. 2011), the next decade should witness substantial progress in our understanding of cultural innovation in all its various guises. On a broader plain, evolutionary anthropology has made great strides in developing a body of theory that complements biological evolutionary theory as opposed to borrowing it wholesale and hoping that it contains something of value. There is every reason to suspect that this trend continues.

Acknowledgments My ideas on cultural transmission have been greatly influenced by my collaborations with Lee Lyman, Alex Mesoudi, and Stephen Shennan.

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Chapter 5
Culture in the Lower Palaeolithic: Technological Variability in Middle Pleistocene Europe

Hannah Fluck

Introduction

Cultural variability in the Palaeolithic is a difficult subject. The very question as to whether non-*Homo sapiens* species are capable of cultural behaviour remains debatable. However, increasingly compelling arguments from many working in primatology for the presence of culture among other great apes, such as William McGrew's "cultured chimpanzees" (McGrew 2005) and Carel van Schaik's work highlighting the presence of different behavioural traditions in orangutan (van Schaik et al. 2003), are gradually eroding the counter-argument. Nevertheless, for many, to identify cultural variability in hominin behaviour is to demonstrate the humanity of the hominins responsible for the Palaeolithic archaeological record. This chapter seeks to highlight the difference between an "anthropological" and an "archaeological" definition of culture and explores when and how these two facies of culture overlap in the European Lower Palaeolithic between c. 500,000 and 300,000 years ago. The history of research into the Clactonian and Acheulean is used to illustrate the need for distinction between these two definitions of culture when approaching the Lower Palaeolithic record.

Anthropology and Archaeology

The question of the identification and definition of culture is by no means a straightforward one. The anthropological debate concerning the use of the word is considered to have begun when Sir Edward Tylor defined culture as

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B.W. Roberts and M. Vander Linden (eds.), *Investigating Archaeological Cultures: Material Culture, Variability, and Transmission*, DOI 10.1007/978-1-4419-6970-5_5,

> "that complex whole which includes knowledge, belief, art, morals, law, custom and any other capabilities and habits acquired by man as a member of society" (Tylor 1871, 1).

Since then, a plethora of definitions have emerged (e.g. see Cronk 1999; Ingold 1988; Kroeber and Kluckhohn 1952; McGrew 2005 for further discussion) with little consensus. One of the more concise definitions of culture is proposed by the primatologist Bill McGrew as "the way we do things" (McGrew 2003, 433), and for McGrew this definition also includes chimpanzees, and potentially other animals, such as cetaceans.

For archaeology, with the clear emphasis on *material* culture, the definition has hardly been easier and, in fact, there has been surprisingly little discussion about what is meant by the term culture in an archaeological context. A commonly recognised definition, however, is that summarised by David Clarke (1962): "a constantly recurrent group of contemporary artefacts within a limited geographical area" (Clarke 1979, 490).

While this chapter is not intended as a debate about definitions, I do want to highlight certain differences between "culture" when used in an anthropological sense and "culture" in an archaeological sense which are pertinent to any discussion of Palaeolithic archaeology. Anthropologically speaking (although I include species other than *H. sapiens sapiens* in this), to borrow McGrew's definition, culture is the way we do things: it is our way of doing things that differentiates us from others and them from us. The key feature of the anthropological definition of culture, through all its complexity, is the human (or indeed chimpanzee) scale: it occurs and is observed at a scale that consists of and is mediated by individuals, with units of analysis observable in the time frame of individual lifespans.

When it comes to Palaeolithic archaeology, there is a distinct empirical discussion concerning "archaeological cultures" *sensu* Clarke (1962). Archaeological cultures are distinguished at the level of the artefact – they comprise and are expressed through empirically observable variations in objects. Crucially in Palaeolithic archaeology these "archaeological cultures" are usually referred to as "industries", a term which highlights the fact that the main artefact types making up these assemblages are stone tools. Although there are clearly more overlaps between the two approaches, as archaeologists we often try to ascribe cultures in an anthropological sense, the empirical validity of archaeological cultures (*sensu* Clarke) is a valid and important debate. Like it or not, these "units of analysis" are used for shorthand communication in the world of archaeological research. This is especially true for the Palaeolithic, and it is perhaps in the Palaeolithic that the distinction between culture in an anthropological sense and culture in an archaeological sense is particularly strong. The strength of this distinction lies in the units of time Palaeolithic archaeologist have to work with. Although individual sites, such as Boxgrove (Roberts and Parfitt 1999) may offer snapshots of this distance, past observations and comparisons between datasets are simply not possible at the level of the individual lifespan (e.g. Pettitt 1995).

Middle Pleistocene Archaeology

The Palaeolithic covers the material culture of a number of different hominin species, which may or may not be directly ancestral to extant *H. sapiens*, over a period greater than two million years. Despite attempts (e.g. Delisle 2000 for an overview, Foley 1987), there are in fact very few Palaeolithic cultures which are exclusively associated with a single species. The time depths at individual sites can be huge with a stratigraphic resolution often more familiar to geologists than archaeologists.

Palaeolithic archaeology is often described as the one truly global archaeological period with broad brush interpretations of vast geographical areas and equally large swathes of time (e.g. Gamble 1993, 1999; Wymer 1981). In Lower Palaeolithic research, it is not uncommon for comparisons to be made between "contemporaneous" assemblages that could in fact be up to 100,000 years apart.

Given the problems, particularly the chronological, inherent in much Palaeolithic research it is possible to make the distinction between culture in an anthropological sense and archaeological cultures, in a way that may become less clear in more recent archaeological periods. To talk about cultural similarities and changes in an anthropological sense for an assemblage within a *minimum archaeological-stratigraphic unit* (see Stern et al. 1993) of over 100 kya, with a resolution covering hundreds, even thousands, of generations, is absurd. In these instances, the assemblages represent averaged behaviour over time frames most anthropologists can only dream about, and it is important to remember this when assessing interpretations of Palaeolithic datasets. Of course, there are some famous exceptions where the material remains of fleeting Pleistocene moments have been preserved for posterity: the Lower Palaeolithic site of Boxgrove, West Sussex, on the south coast of England provides some famous examples (Bergman and Roberts 1988; Mitchell 1996; Pitts and Roberts 1998; Roberts and Parfitt 1999). However, the vast majority of our knowledge of our Pleistocene ancestors comes from assemblages that have been time-averaged to some extent (for example see Hosfield 1999 and McNabb 2007 for specific discussion of time averaging issues). While challenging, this variation in resolution of the Palaeolithic record is a real strength of research in this particular period – no other area of archaeology or anthropology has access to data on human behaviour over such long periods of time.

For this reason, this discussion focuses on what I shall refer to as archaeological cultures: that is the observed patterns in material culture present in the archaeological record. For the Palaeolithic, this usually means patterns in stone tool technology and typology. For some researchers, the key patterns are the presence/absence or proportions of different retouched tools, such as types of scrapers, denticulates, notches or handaxes, while for others it is whether or not a particular technique has been employed in the manufacture of the stone tools, e.g. the presence of core preparation strategies, such as Levallois; for most, it is a combination of the two. In either instance, the descriptions of tool types and technologies, and the diagrams illustrating the proportions in which they are present in the assemblages under analysis, have become our yardsticks, our means of comparison and the language we use to discuss the empirical record.

Variation in the Lower Palaeolithic

As alluded to already, the definition, identification and interpretation of archaeological cultures in the Palaeolithic require the broadest of brushes. No other archaeological period can claim a single archaeological culture that stretches in time from around one and a half million years ago to 300,000 years ago, and in space from the southern Cape to Central England and from Lisbon in the west to Bangladesh in the east; but this is often maintained to be the case for the Acheulean (see Fig. 5.1).

The Acheulean is traditionally presented in the literature as a period of technological stagnation when the same thing, i.e. handaxes (see Fig. 5.2), was made repeatedly by hominins who lacked the capabilities of doing anything more inventive. This view is particularly prevalent in the work of researchers investigating later Palaeolithic periods where the stagnation of the early Palaeolithic is contrasted with the sudden and vibrant changes of the more recent Palaeolithic. Such an approach has often been taken with regard to what has been called the "human revolution" at the Middle-Upper Palaeolithic transition (e.g. Mithen 1996),

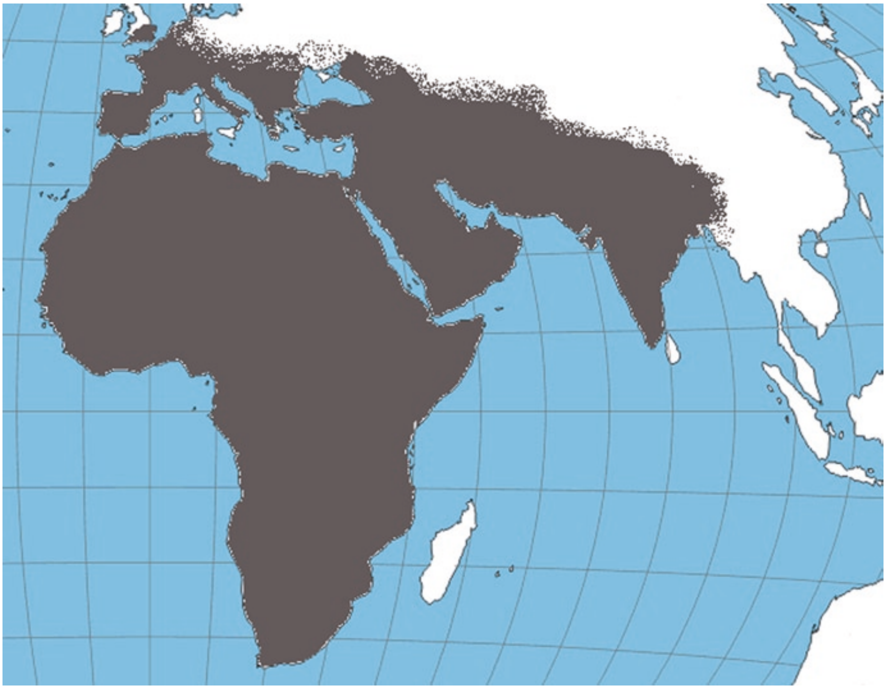

Fig. 5.1 Map showing rough extent of Acheulean sites (*shaded area*)

Fig. 5.2 Biface from Broom, near Axminster in England. Photographed by L.S. Basell, with kind permission from Somerset County Museum where the artefact is now held

although some authors have begun to move away from the revolutionary approach to change (e.g. Gamble 2007). While undoubtedly there are striking similarities in the presence of bifacially flaked tools throughout the Lower Palaeolithic world, the use of the term as a blanket description of the record for this period conceals considerable variation.

Historically certain archaeological cultures have been identified within the European Lower Palaeolithic, e.g. Abbevillian, Clactonian, Mesvinian, Tayacian (e.g. Breuil 1932a, b; Collins 1969; Warren 1926; Wymer 1968), but these have gradually fallen out of use as the overwhelming similarity of handaxe assemblages has been emphasised (e.g. McNabb 1992; Jaubert and Servelle 1996; Geneste 1990). The majority have now become variations of the Acheulean *sensu lato*. The extent to which the Acheulean has become the all encompassing Lower Palaeolithic culture varies between researchers. Without wishing to stereotype modern cultural differences among researchers from different parts of the world, there has been a general trend, particularly among many colleagues trained in France, to broaden definitions of the Acheulean to incorporate wider and wider degrees of variation (e.g. Geneste 1990; Jaubert and Servelle 1996 but for an alternative view see Molines 1996, 1999; Monnier 1996). The historical research by colleagues in Italy has tended to emphasise smaller variations between assemblages, identifying numerous local “cultures” in both archaeological and anthropological sense (e.g. Bietti and Castorina 1992; Mussi 2001; Palma di Cesnola 1996, as discussed in Villa 2001). In Britain, the debate has focused on the presence of, and distinction between, two assemblage types – the

Acheulean and the Clactonian (Ashton et al. 1994, 1998; Ashton and McNabb 1994; Baden-Powell 1949; McNabb 1992, 1996, 2007; Ohel 1979; Wenban-Smith 1998; White 2000).

Clactonian Debate

Essentially, the Clactonian label is assigned to assemblages lacking handaxes and the Acheulean to those assemblages where handaxes are present, and historically the Clactonian has been defined as a non-handaxe culture in the anthropological and archaeological sense of the word as defined above. Its status as a distinct culture, both in terms of its empirical technological and typological distinction from the Acheulean and with regard to its status as an "anthropological culture" has been debated for almost a century, a discussion which has echoed the development of the discipline of Palaeolithic archaeology in Britain.

Definition and Identification

At the turn of the twentieth century, Lower and Middle Palaeolithic assemblages fell into one of three categories – Eoliths, Acheulean and Chellean handaxe industries, or the Mousterian flake industries – following the typological divisions developed by the French Palaeolithic pioneer de Mortillet (Collins 1986; Wymer 1968). This scheme was one of progressive epochs with each stage more advanced than the previous one (Fig. 5.3). However, during the first few decades of the twentieth century the idea of progressive epochs had begun to be challenged (e.g. Breuil 1913) and discoveries were being made from certain sites in southern England, such as Swanscombe and Clacton-on-Sea, that did not appear to fit into these accepted categories (Smith and Dewey 1913; Warren 1911a, 1922, 1923).

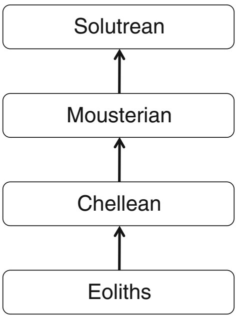

Fig. 5.3 Illustration of the de Mortillet scheme

Having been identified as important for Pleistocene fauna since the mid-nineteenth century (Kenworthy 1898), in 1911 S. Hazzeldine Warren put Clacton-on-Sea (Essex, UK) on the Palaeolithic map (Warren 1911a, b, c) and he continued to study the Clacton area for a further 40 years (Warren 1922, 1923, 1926, 1932, 1951, 1955). The Clacton artefacts – consisting of simple cores, flakes and flake tools – did not fit into the accepted categories of the day: they were not eoliths, but nor were they handaxes; they consisted of flakes and flake tools but were not Mousterian. Moreover, the Clacton artefacts were not isolated discoveries: the Swanscombe Lower Gravel was yielding flakes and cores but no handaxes (Smith and Dewey 1913), and similar flints were also being collected from Little Thurrock by B. Wymer (although the material from this site was not published until 1957, by his son John, partly because at the time Wymer senior did not know what to make of the artefacts) (Wymer 1957).

These assemblages remained a typological enigma until the mid-1920s when the Abbé Breuil noted the similarity between the Clacton-on-Sea artefacts and those from Mesvin, Belgium after viewing Warren's collection (Warren 1922). In fact, the assemblage was initially called Mesvinian until Warren introduced the term "Clactonian" in 1926.

Clacton-on-Sea was not the only locality where non-biface assemblages were being found: the Swanscombe localities of Barnfield Pit and Rickson's Pit were also yielding similar assemblages (Dewey 1930, 1932, 1959; Smith and Dewey 1913). By the 1930s, the Clactonian was embedded in the British Palaeolithic with syntheses by Breuil (1930, 1932a, b), Warren (1922, 1923, 1926, 1932), Chandler (1930, 1932), Oakley and Leakey (1937), and Smith and Dewey (Dewey 1930, 1932; Smith and Dewey 1913). However, while it was agreed by all that this industry was distinct from other Palaeolithic industries in its lack of bifaces, the positive characteristics that precisely defined these assemblages as Clactonian were far harder to agree upon. For Breuil, the Clactonian was characterised by the presence of flakes with low flaking angles, wide striking platforms and prominent percussion cones and bulbs (Breuil 1930, 1932a, b), even to the point that he would classify isolated artefacts rather than considering entire assemblages. Despite the experimental work by Baden-Powell (1949) suggesting that these "characteristic flakes" are merely the result of hard hammer percussion, and later work by Ohel supporting this (Ohel 1979), the term "Clactonian flakes" is still used by some authors on the continent as a descriptive term (e.g. Palma di Cesnola 1996). Breuil's emphasis on the flakes as the important aspect of the Clactonian contrasted with Warren's approach who considered the cores to be the tools and the flakes to be largely by-products: whether the Clactonian is primarily a core or a flake assemblage is a debate that continued into the 1970s (see Ohel 1979).

A significant change in the approach to Palaeolithic archaeology in the post-war period was the consideration of whole assemblages rather than relying on the presence of particular type fossils to characterise the assemblage. Francois Bordes was one of the pioneers of whole assemblage analysis (Bordes 1953; Bordes and Bourgon 1951), and this move away from *fossiles directeurs* to systematic typology and statistical indices was one of the greatest changes to Palaeolithic research of the past century. Today, the Bordes Typology for the Lower and Middle Palaeolithic

(Bordes 1961) is one of the most widely used and universally understood systems for stone tool analysis in the world. It is not without its problems, however, and it has been found to be unsuitable for a number of assemblages identified in Europe, including those from the Clactonian sites (e.g. Aculadero, Spain: Querol and Santonja 1983; Vertésszölös, Hungary: Kretzoi and Dobosi 1990). The artefacts from these assemblages are rarely standardised and, in the case of El Aculadero and Vertésszölös, often contain high numbers of pebble tools, artefacts which are not given much attention in Bordes typology. The very character of these assemblages is one of the unstandardised tools. The inability to apply such universal typologies to many Middle Pleistocene non-biface assemblages is one of the reasons they have been little understood and rarely brought into discussion.

The end of the 1970s saw the beginning of a period in which the very idea of the Clactonian as a distinct archaeological culture was question with a publication in *Current Anthropology* by Milla Ohel (1979). Ohel proposed that the Clactonian sites were not a separate industry but rather represented areas where the Acheulean knappers were preparing raw material to take elsewhere and produce handaxes. Emphasising similarities in the knapping strategies of the two industries, he drew upon empirical evidence to support these inferences. Most importantly, he used archaeological data to demonstrate, as Baden-Powell had previously with experimental data (Baden-Powell 1949), that the Clactonian flakes Breuil had identified as characteristic of the assemblages were in fact a widespread phenomenon characteristic of hard hammer percussion in general. He also demonstrated that many of the assemblages from earlier Clactonian excavations displayed collection biases towards "Clactonian" types and probably did not reflect the complete assemblages. The Clactonian debate as we know it today had begun – the question was no longer what the Clactonian represented in terms of chronological, evolutionary or cultural stages (as an anthropological culture), but whether it could be described as a separate phenomenon (an archaeological culture) at all.

The main contributors to this debate were John McNabb and Nick Ashton who argued that there were no differences between the Acheulean and Clactonian assemblages other than the number of handaxes present. McNabb in particular also argued for the presence of handaxes in some Clactonian assemblages (e.g. McNabb 1992, 1996). In more recent years (McNabb 2007), he has conceded that perhaps many of these instances of bifaces in the Clactonian may be doubtful and, that despite concerted efforts by a number of scholars, the fact that the Clactonian refuses to be explained away suggests that there may be something there. For McNabb (2007), this is a pattern visible only at the large assemblage scale of greater than 500 artefacts.

Technologically, the Clactonian is a flint industry, broadly dated to early MIS 11, consisting of unprepared, large, hard hammer flakes and cores. The cores are characterised by a strong alternate flaking patterns, and combinations involving alternate flaking. The retouched tools (simple scrapers, flaked-flakes and retouched notches) make up a small percentage of the total assemblage (typically less than 10% of the assemblage) and handaxes are absent.

Interpretation, Dating and Evolutionary Schema

Initially, Warren (1922) proposed that the Clactonian (or Mesvinian as it was then known) industry was not connected to the Chellean or Acheulean, but that it might be a precursor of the Mousterian and contemporary with, or slightly earlier than, the Acheulean. This suggestion fitted in with developments in global Palaeolithic typologies which were increasingly allowing for different contemporary traditions, in particular with the work of Hugo Obermaier on the Palaeolithic of Spain, Italy and Switzerland (Groenen 1994; Obermaier 1924; Trigger 2006). The Abbé Breuil later supported this parallel phyla approach when he considered the relationship of the flake-based and core-based industries of France with the Clactonian evolving into the Mousterian via the Languedocian, Tayacian and Mesvinian on the one hand, and the Abbevillian evolving into the Micoquian via the Acheulian on the other (Breuil 1932b; Groenen 1994).

As the Clactonian became widely accepted as a "flake-industry", the concept of flake cultures, as opposed to core-tool cultures, was strengthened. Flake cultures included the Clactonian, the Levalloisian and the Mousterian; core cultures were exemplified by the Abbevillian and the Acheulean. However, there were doubters of this clear-cut distinction and Oakley, as Warren had earlier, began to consider the possibility that the Clactonian may be related to the Choukoutien-Soan core and flake-industry of the Far East (Oakley 1949) – possibly as an "early offshoot". The general "Big Picture" (see Dennell 1990) approach of this period meant that new finds were fitted into a broader, pre-existing view of steady development rather than challenging contemporary perspectives.

At this point, the Clactonian was a term that was applied to assemblages across Europe – Breuil noting Clactonian assemblages from Portugal (Breuil et al. 1942) and France (Breuil 1932a), and with others later identifying Clactonian assemblages from Italy (see Palma di Cesnola 1996 for examples). The Clactonian as a concept was entrenched in not just the British Lower Palaeolithic, but pan-European Palaeolithic schema.

In his definitive paper on Clacton, Warren (1951) maintained that the industry was primarily a core-tool industry (see McNabb 2007 for a summary of his peculiar definitions of cores). Rather than discussing the internal "progressive evolution" of the Clactonian which Breuil had emphasised (Breuil 1930), and undoubtedly influenced by Movius, he suggested that as the Asian chopper tools were conditioned by the raw material, so the Clactonian manufacturers were restricted by the uneven nature of their irregular flint nodules. While believing there was a connection with the early pebble tool industries identified by Leakey in Africa and Movius in Asia, he noted that as similar artefacts could be found through to the Neolithic, it was difficult to make a judgment about the evolutionary standing of the industry (Warren 1951). Both these points – the role of raw material in the shaping of an industry, and the idea that even more technically advanced cultures still make simple tools when it suits them – have continued to play a key role in the debates surrounding the interpretation of many Lower Palaeolithic industries.

As the view of steady and progressive evolutionary change was shattered in the aftermath of World War II (Dennell 1990) and the New Archaeology of the 1960s and 1970s heralded a new age in archaeology. In the Palaeolithic, the New Archaeology meant that the primary focus of investigation was no longer solely a vertical one, attempting to fit sites into a linear evolutionary sequence. Instead, researchers began to pay more attention to the spatial and temporal distribution of sites and artefacts. Central to this were scientific dating techniques which allowed absolute dates to be obtained for sites. However, while these new techniques and approaches certainly revolutionised Palaeolithic archaeology elsewhere, such dating techniques could not (and often still cannot) be applied to many British Lower Palaeolithic sites in secondary contexts which, therefore, had to rely upon other geochronological dating methods.

While the Clactonian lithic assemblages were growing with the results of new excavations [at Little Thurrock (Snelling 1964), Clacton Golf Course (Singer et al. 1973) Barnfield Pit (Ovey 1964; Waechter et al. 1970) and Purfleet (Palmer 1975)], important advances in pollen and molluscan studies had enabled a detailed review of the Hoxnian interglacial, and allowed these sites to be placed within the subdivisions of the Hoxnian. This was the interglacial that, traditionally, was associated with the Clactonian. Although these data had been used previously to gain an understanding of the environmental context for the Clactonian, these advances, in combination with a greater understanding of the Pleistocene climatic cycles provided by marine and terrestrial cores and isotope studies, dramatically changed the geochronological understanding of the British Pleistocene. It was found that the Clactonian assemblages at Swanscombe and Clacton-on-Sea were in fact *earlier* than the Acheulean industry identified at Hoxne. Such a picture was repeated across the board and the Clactonian was happily placed as the earliest industry in Britain. Although some, such as Waechter, hesitated in assigning the Clactonian label too swiftly to newly discovered assemblages, by and large the validity of the Clactonian assemblages as an independent phenomenon was not questioned.

Wymer (1974) used this pollen and molluscan data to show that the temporal gap between what he saw as the earlier Clactonian and the ensuing Acheulean was very slight. As such, he argued that the disappearance of the Clactonian and the appearance of the Acheulean were so sudden that rather than a gradual development from one to the other, the former must have been replaced by the latter. Implicit in this idea of the replacement of industries, of archaeological cultures, was the replacement of populations: the Clactonian making peoples were replaced by the incoming Acheulean handaxe makers.

With considerable advances in the disciplines associated with geochronology, scientists began to link together the marine core records, ice core records and the terrestrial glacial sequences. The result was a greater level of understanding of the complex climatic changes *between* and *within* the glacial and interglacial sequence. This work meant that the chronological understanding of the Pleistocene changed dramatically during the 1980s. From having been considered to date to c. 250 ka prior to the 1980s, the Clactonian sites were now considered to be nearly double that age.

It was not just the age of the sites that were disputed, the accepted technological sequence was also shaken. The discovery of Boxgrove (Roberts and Parfitt 1999) during the same decade also helped to push back the dating of the Lower Palaeolithic in Britain with handaxes now apparently pre-dating many of the Clactonian sites. This was a challenge to the previously progressive approach to lithic technology, and it was confounded by the findings from excavations at the site of High Lodge (Ashton et al. 1992), another site where the typology of the tools was considered to be more developed than expected for the dating of the site. These were revolutionary discoveries and forced a whole suite of new questions about the relationship between typology, technology and Palaeolithic behaviour – technology and typology could no longer be relied upon as a sole chronological indicator. The Clactonian could also no longer be seen as ancestral to or preceding the Acheulean. The presence of two parallel industries in the British Lower Palaeolithic was once more a possibility and the idea of archaeological cultures as chronological markers took another blow.

This view was strengthened by the detailed research undertaken by the British Museum at East Farm, Barnham in the early 1990s (Ashton et al. 1994, 1998). This site had previously been believed to demonstrate that the Clactonian chronologically preceded the Acheulean, however, the re-excavation revealed a far more complex situation. The excavators suggested that rather than consisting of two stratigraphically distinct assemblages, the different industries represented different localities, possibly for different activities, within a complex landscape.

The outcome of the past two decades of geochronological and biostratigraphic research, particularly with regard to fluvial deposits, is that the majority of Clactonian sites are now considered to date to MIS 11 (McNabb 2007). Although, further research, in particular by Mark White, has suggested that Clactonian sites may also be present at the end of MIS 10 and beginning of MIS 9 (Cuxton and Purfleet) (White et al. 2006; White 2000; White and Schreve 2000). White has proposed that the Clactonian represents a pioneer stage in the occupation and reoccupation of Britain which is why it appears to be associated with the very beginning of interglacials. He proposes that either the population density at the time would have been such that the skills necessary to manufacture handaxes might have been unsustainable, or the pioneer re-settlers may have come from parts of the continent where handaxes were not routinely made (White 2000). Previously, Steve Mithen had proposed that learning and transmission of skills may play a role in the explanation of the Clactonian (Mithen 1994). He suggested an association between the Clactonian and wooded environments on the basis of primatological research, suggesting that primates living in wooded and forested environments were more likely to have individual learning strategies, compared to the group strategies of primates living in more open environments. While this is an interesting idea, the correlation between wooded environments and Clactonian sites, open environments and Acheulean sites does not stand up to scrutiny (see McNabb and Ashton 1995). However, the argument that there is a relationship between the way individuals learn, their group size, environments and the sort of artefacts they might produce is a strong one for explaining archaeological cultures. The transmission of learnt behaviour among individuals and between generations is a key element in anthropological culture. Group size affects how this occurs, and this group size is affected by the environment in which hominins live. The patterns in the archaeological cultures observed at the small scale of most Lower Palaeolithic data may show similarities not because they are indicative of a single anthropological culture but because they are indicative of certain behaviours, or circumstances which leave similar archaeological patterns.

While the Clactonian has oscillated from archaeological to anthropological culture, it is a fine illustration of varying attitude to culture in Palaeolithic archaeology. The empirical definition of an archaeological culture is constantly in flux – it needs to be constantly tested and reassessed. For the Clactonian, the fact is that an archaeological culture defined by negatives did not explain it away. It is an empirically verifiable phenomenon still in need of explanation.

A Wider Picture

Although for many, certainly for the majority of researchers outside the UK (e.g. Byrne 2001; Fernández Peris 2006; Jaubert and Servelle 1996; Raposo et al. 1996), the Clactonian debate had been concluded with the work of McNabb and others arguing that the term was redundant, the debate has recently found renewed vigour among researchers in the UK. In particular, recent discoveries by Francis Wenban-Smith at Ebbsfleet (Wenban-Smith et al. 2006) have added new fuel to the fire. As Mark White has pointed out, while arguments against the existence of the Clactonian as a separate phenomenon have raised some valid points and generated important discussions, they have failed to offer satisfactory explanation for the variation observed in the record (White 2000). Arguing that the discussion regarding the Clactonian has not yet run its course, he turns to the wider European context to postulate four scenarios for non-handaxe assemblages: firstly, very early occupations pre-dating the use of handaxes; secondly, regions where handaxes were not made; thirdly, chronologically discrete periods when handaxes do not occur in regions where they are found at other times; and fourthly, occasional occurrences of assemblages without handaxes geographically and chronologically contemporaneous with handaxe assemblages. Although White's paper is primarily setting out descriptive scenarios for future analysis, McNabb (2007) notes that there are strong cultural undertones to his argument. The revival of cultural explanations is more clearly stated in the recent work of Wenban-Smith with the Clactonian seen as part of the cultural ebb and flow of technological change and variability, driven by social learning (Wenban-Smith 1998, 2004; Wenban-Smith et al. 2006). In many ways, the debate has come full circle – there are more than a few echoes of a cultural historical approach in this recent discussion. However, to date a systematic comparison with the European data has not been undertaken until now.

In the course of undertaking such a study, it has become clear to me that, when we begin to look at the broader picture, the universal, consistent nature of Acheulean hand axe manufacture has to be called into question. The Clactonian is not alone. There are

Table 5.1 Table showing some of the main European Middle Pleistocene non-handaxe sites and their dates (after; Bridgland et al. 2006; Dobosi 2003; Fernández Peris 2006, 2007; Fernàndez Peris et al. 2000; Geneste and Plisson 1996; Molines 1999; Moncel 2003; Querol and Santonja 1983; Rigaud and Texier 1981)

<table>
<tr><th>Marine isotope stage</th><th>Date</th><th colspan="6">Sites</th></tr>
<tr><td>MIS 7</td><td>242 kya</td><td rowspan="3">Cova del Bolomor</td><td></td><td></td><td></td><td></td><td></td></tr>
<tr><td>MIS 8</td><td>301 kya</td><td rowspan="6">El Aculadero ?</td><td></td><td></td><td></td><td></td></tr>
<tr><td>MIS 9</td><td>334 kya</td><td>Purfleet?</td><td>Vértesszölös?</td><td rowspan="5">St Colomban? Menez Dregan?</td><td rowspan="5">Les Tares</td></tr>
<tr><td>MIS 10</td><td>364 kya</td><td></td><td></td><td></td></tr>
<tr><td>MIS 11</td><td>427 kya</td><td></td><td>Swanscombe and Clacton-on-Sea localities, Little Thurrock</td><td>Vértesszölös?</td></tr>
<tr><td>MIS 12</td><td>474 kya</td><td></td><td></td><td></td></tr>
<tr><td>MIS 13</td><td>528 kya</td><td></td><td></td><td>Vértesszölös?</td></tr>
</table>

other instances of assemblages without handaxes within the European Lower Palaeolithic (see Table 5.1 for some examples). However, while the absence of handaxes may lead some to question whether there is a connection between these assemblages (e.g. Collins 1969), any similarities and differences are not necessarily indicative of cultural (in the anthropological sense) connections as Collins had argued.

The open-air travertine site of Vértesszölös in northern Hungary has long been used as an example of a Lower Palaeolithic assemblage characteristic of a non-handaxe culture in both senses (the Buda industry); however, despite some claims to the contrary, it is an isolated occurrence. As a counter-argument to the Buda non-handaxe culture, the small size of the raw material has often been cited as the reason for the lack of handaxes. However, I have argued elsewhere (Fluck and McNabb 2007) that the small river pebbles were not the only raw material used at the site – other raw material was available in larger pieces and was used. The isolation of this site makes it difficult to interpret: with no other Lower Palaeolithic assemblages nearby (the nearest is several hundred miles away), it is difficult to place the site within a wider technological, behavioural and landscape context. The small size of the artefacts (the average length is 26 mm), despite the availability and apparent use of larger raw material would suggest that either small artefacts were deliberately made, used and discarded, or small artefacts were discarded at the Vértesszölös site and larger raw material was curated and moved elsewhere. The latter would be consistent with a differential use of the landscape, possibly similar to the differential and complex use of the landscape as observed at Barnham St Gregory (Ashton et al. 1994, 1998). There, at certain locations, possibly as stopping off points on hunting or foraging trips where raw material was either unknown or was scarce, artefacts that were no longer useful were discarded and the larger fragments were taken away with the hominins when they moved on.

Cova del Bolomor in the Valldigna Valley, Valencia, Spain is also a solitary site – the oldest site known from the area. It is a rock shelter site almost continuously occupied from MIS 9 to MIS 5e, with the lowest level dated to c. 350 kya (Fernández Peris 2007). The lithic assemblage does not fit easily into any Lower or Middle Palaeolithic assemblages but, like Vértesszölös, its isolation makes it difficult to interpret. Excavations are ongoing but evidence of fire has been found in some of the lower levels and the faunal assemblage is especially rich, particularly in equid. Its location overlooking the Valldigna valley makes it an ideal location for a hunting party and it appears that the site was frequently and repeatedly occupied for short periods of time over many generations.

The Colombanien sites of Brittany are some of the few examples of Lower Palaeolithic assemblages which, like the Clactonian sites, appear to show a consistent pattern at a number of localities throughout a region (Monnier and Molines 1993). The assemblages are characterised by a large chopping tool element on various beach pebbles and a smaller flake tool element mostly on flint, and the sites seem to be particularly associated with marine caves. For the excavators Jean-Laurent Monnier and Nathalie Molines, the assemblages are the local coastal version of the Acheulean – handaxes are found but at different locations and are not as abundant as they are inland.

Conclusions

Most of the objections to the identification of the Clactonian as a distinct archaeological culture have focused on problems with the identification of an archaeological culture based on the absence of a key artefact type – the handaxe. And much of the controversy that ensued was due to the conflation of archaeological and anthropological cultures. However, if we define an archaeological culture as a group of sites or assemblages that are consistently similar to each other and consistently different from others, then the Clactonian fits the bill. In the first instance, the identification of an archaeological culture identifies patterns in the material culture without judging whether or not those patterns reflect culture in an anthropological sense.

Looking further afield, we can see that the Clactonian is not alone: other sites and assemblages are known from the Middle Pleistocene that also lack the types usually used to identify them. There may be many different explanations for these assemblages and each must be studied in its own context as well as part of a broader picture. That Palaeolithic archaeology concerns itself in the first instance with the identification of archaeological cultures is important. In Palaeolithic archaeology, the distinction between culture in the archaeological and anthropological sense is a valuable one. When we separate the two and concentrate on the investigation of archaeological cultures, what becomes clear is that far from being a period of stasis, uniformity and automatic behaviours there is a rich record demonstrating a complex and varied hominin behaviour in the Middle Pleistocene. The production of these different archaeological cultures may be due to function, raw material resources or mobility. In the case of the Clactonian, found in time averaged deposits representing the tool production, use and discard of many generations, these assemblages represent a particular pattern of learned behaviours – the way they did things, at that place at that time with that raw material. However, before we take the interpretative step to attribute anthropological culture to these archaeological cultures, there are many other factors that must be considered. These assemblages represent repeated behaviours at particular localities over a number of generations, and those same hominins may have undertaken very different activities at different localities leaving very different artefacts. The number of sites where handaxes are absent suggests that, contrary to the popular interpretation of the handaxe as the original "swiss army knife", not all these activities required handaxes. While this may seem like an obvious statement, the idea that a group of Middle Pleistocene hominins may have produced different material culture at different locations implies a level of adaptability and flexibility not often attributed to the likes of *Homo heidelbergensis.*

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Chapter 6
Techno-Modes, Techno-Facies and Palaeo-Cultures: Change and Continuity in the Pleistocene of Southeast, Central and North Asia

Ryan J. Rabett

Introduction

The possibility that behavioural differences might have existed between early Afro-European and Asian hominin groups is one of the most debated subjects in Palaeolithic research (e.g. Boriskovsky 1978; Davis 1987; Davis and Ranov 1999; Hutterer 1977; Keates 2002; Movius 1944, 1948, 1978; Okladnikov 1978; Ranov 1995; Ranov and Davis 1979; Schick and Zhuan 1993; Vasil'ev 1993; Yi and Clark 1983). Although inter-regional studies of East, Central and North Asia have been attempted (e.g. Dennell 2004; Rolland 2002; Schepartz and Miller-Antonio 2004), they have tended to focus on Lower and Middle Pleistocene hominin dispersal across Asia. Although geographically broad symposia have been convened on early modern human diaspora across Asia (e.g. Derev'anko and Shunkov 2006), systematic inter-regional comparisons have been few.

Over the past 20 years, studies of the Middle and Upper Pleistocene Old World have closely examined the fossil and material cultural evidence for the emergence of *Homo sapiens* (e.g. see papers in Mellars and Stringer 1989; Mellars et al. 2007) and particular emphasis has been placed on the status of populations' biological and behavioural "modernity." During this period, the onus has been on reconstructing defining behaviours at the federate (or pan-species) level. Although the comparatively sparse nature of the evidence continues to affect what can be said realistically, discussion about humanity's inter-regional character has also become sidelined. Debate between proponents of the "Out of Africa" and "Multi-regional" hypotheses of early human genesis, so prominent at the beginning of this research period (e.g. Mellars and Stringer 1989), subsided as the weight of genetic evidence and general opinion came to favour a singular origin in Africa. Attention to inter-regional studies has, as a consequence, diminished. Yet both the data and the questions remain.

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B.W. Roberts and M. Vander Linden (eds.), *Investigating Archaeological Cultures: Material Culture, Variability, and Transmission*, DOI 10.1007/978-1-4419-6970-5_6,

Multi-regionalism may not be part of our genetic heritage, but have we been too quick in assuming that this precludes its importance in our behavioural ancestry? The current chapter assesses the character and trajectory of technological developments in Southeast, Central and North Asia during the Middle and Upper Pleistocene and the potential impact of the Asian evidence on current models of early modern human behaviour.

Southeast Asia exhibits extraordinarily long-term use (c. 1 my) of diverse and generally un-standardised low-input lithic technology (Reynolds 2007). Although such components are not restricted to Southeast Asian industries, occurring in European and African records as well, it is significant that they remain paramount here even after the appearance of anatomically modern humans in the region. Elsewhere, these technologies seem to become eclipsed by a growing emphasis on higher input forms – although the degree to which this stems from interpretations that have favoured the more complicated forms remains unclear (T.E.G. Reynolds, personal communication). The distinct character of these pebble and flake ["Mode-1"(*after* Clark 1971)] lithics and the apparent absence of bifacial technologies equivalent to the Acheulean ("Mode-2") were central in the creation of the so-called Movius Line separating western from most eastern provinces of the Old World (Movius 1944, 1948). Central Asia was chosen as a point of comparison because it is recognised as a major region of cultural contact between western and eastern Old World provinces, while the Altai Mountains represent one of the most well-researched areas of North Asia, and its archaeological sites have the longest stratigraphic sequences in Siberia (Derev'anko et al. 1998; Kuzmin and Orlova 1998). Although Upper Pleistocene conditions there were not identical to those in Europe, and the impact of regional-scale variables (such as the influence of the Mediterranean Sea upon the latter) should not be underestimated, both regions experienced considerable climatic instability and extensive glaciations during this period. On a broad scale, therefore, the Altai provides a suitably parallel setting to examine the effects of glacial adaptation in relation to Asian technological suites.

In this discussion, the term "Palaeolithic" is used only as a descriptor for stone technology; it is not used as a temporal referent. I also use the term "techno-facies." This might be defined (after the lithological meaning of "facies") as a body of lithic technology with specified characteristics that distinguish it from other comparable industries, reflecting particular processes of manufacture or setting. This has two immediate advantages. Firstly, a lithological "facies" refers to descriptive rock attributes independently of time (Cross and Homewood 1997, 1619). Given the nature of the Pleistocene record of Asia, the decoupling of technological forms from temporal succession is clearly desirable. Secondly, other medium- and high-order classificatory approaches, e.g. the identification of Palaeolithic "cultures" – following Gabriel de Mortillet (Chazan 1995), "Modes" (Clark 1971) or "technocomplexes" (Clarke 1968), have strived to provide overarching frameworks based on typology, technology or techno-adaptive strategy. "Techno-facies" addresses the same need to rationalise terminology for local affiliate industries in a way that is non-value-laden and relevant. It, therefore, fills the classificatory space beneath these higher-order descriptions, currently populated by a variety of disparate terms that tend to encourage either reductionism (e.g. Mousterian-*like*) or imprecise copy (e.g. a *variant* of the Mousterian). To refer to a *techno-facies* of a Palaeolithic culture immediately implies that this is one of many industries that could be said to comprise a palaeo-cultural entity collectively.

Finally, the following spatial and temporal divisions are recognised in this discussion. Geographically (Fig. 6.1), Southeast Asia is recognised here as incorporating Malaysia, Vietnam, Thailand, Indonesia, the Philippines, Cambodia, Burma

Fig. 6.1 Locations of the three principal regions of analysis referred to in the text

(Myanmar) and Laos. Central Asia is defined (*after* Ranov et al. 1995) as including the modern geopolitical entities of Turkmenistan, Uzbekistan, Kyrgyzstan, Tajikistan and southern Kazakhstan. The North Asian evidence is confined to the Altai Mountains. The temporal divisions herein are based on geomagnetic stratigraphy. The Middle Pleistocene is defined as lasting for 775 – c. 120 kya (*after* Gibbard 2003) and the Upper Pleistocene, c. 120–11.5 kya (*after* Gibbard and van Kolfschoten 2004). In keeping with this scheme, the "Late Pleistocene" is informally defined as marking the period c. 40–11.5 kya, commencing at the Laschamp geomagnetic excursion – ($^{40}Ar/^{39}Ar$) dated to 40,000 ± 2,000 bp (Guillou et al. 2004).

Background

The first half of the twentieth century saw significant growth in Palaeolithic research outside Europe. This was driven in part by a new wave of academic scholars: the likes of Louis Leakey in East Africa and Dorothy Garrod in the Levant, as well as by non-academics working in the colonial services of the Dutch, British and French possessions in Indo-China and the Indo-Malay archipelago. In Europe, evidence for increasing diversity in Palaeolithic tool forms (in terms of core preparation, tool standardisation and the appearance of tools in other media, principally bone) lent itself to then contemporary perceptions of cultural evolution. As inter-regional studies began to emerge, however, it became apparent that lithic material culture from other parts of the world did not correspond neatly to the European sequence. One of the leading scholars of the time, Hallam Movius Jr., stated in 1944 that the most significant conclusion of the Joint American Expedition to Southeast Asia was formal recognition that Palaeolithic technologies there appeared to have followed a developmental course that was distinct from that found in other areas of the Old World (Movius 1944). The persistence of pebble and flake industries that he saw across Asia was clear indication that from a very early period, the rate of technological change was different (slower) to that seen in Europe and Africa. This difference was such that any attempt to classify all Old World Palaeolithic industries through a single system would be highly problematic (Movius 1944, 106). The evidence suggested that the pebble and flake industries that he had identified in Southeast Asia and which others were finding in Africa, Europe and South Asia represented a widely distributed technological "substratum." Over part of this wide area – Africa, Europe, the Middle East, Central and Southern India – this technological form was later supplanted by emerging bifacial industries. In Southeast Asia and China it persisted, while in Northern India, there appeared to be a greater admixture of the two (Movius 1948, 410).

After 15 years of fieldwork, Movius (1948) concluded that Southeast Asia could not, in cultural terms, be considered "progressive," as broadly the same technological suite persisted throughout the Middle and into the Upper Pleistocene. Paraphrasing the view of Teilhard de Chardin, he proposed the evidence indicated that from the very earliest times, the region had been one of "cultural retardation" (Movius 1948, 411). This statement, in the final paragraph of his treatise on the subject and the division along typological lines that he proposed became the touch-stone for a debate that did not advance significantly for 30 years (Movius 1978) – 30 years after that the position is scarcely different.

Movius was not alone in recognising or attempting to reconcile the technological differences that appeared between the Pleistocene records of the Old World. In a 1951 paper, S.N. Zamiatnin asserted that the Upper Palaeolithic could be broadly divided into three major cultural areas: a Mediterranean-African zone, a European Periglacial zone and a Sino-Siberian zone. The last of these was so distinctive from the other two that he suggested the other two could almost be treated as a single unit (Vasil'ev 1993). What was perceived to be the biological reductionism of Movius' "retardation" statement would be rejected by leading Soviet Palaeolithic archaeologists (e.g. Boriskovsky 1978, 89–90). In 1966, Okladnikov envisaged more than one stream to cultural evolution. "[T]wo regions took shape in which the evolution of culture went by separate paths: East Asian and Mediterranean-African" (*quoted, in translation*, in Davis 1987, 22). He also concluded that the similarities that set the Asian Palaeolithic apart as a whole were tempered by internal variability of its own and that Asia should not be treated as a "single Siberian-Chinese Palaeolithic province" as Movius and Zamiatnin had done (Okladnikov 1978, 324). This position would be developed by V. Ranov, who divided the Central Asian Palaeolithic into two regional groupings: "cultures of Near Eastern type," which carried technological elements suggestive of links to the Mediterranean and Near East, and "cultures of Asiatic type," which included Siberio-Mongolian industries and carried elements suggestive of influences from the Northeast and South Asia. Ranov viewed the persistence of pebble-tool technologies from the Lower Palaeolithic to as late as the Neolithic as evidence of a parallel, but fundamentally separate line of development to that which was taking place in Europe (Davis 1987).

While evidence for this difference is tangible and growing, accounting for it continues to confound. It has been suggested, for example, that the general paucity of classic bifacial technologies in most parts of Southeast, Central and North Asia may be an indication that hominins in these regions were derived from a population that had left Africa, c. 1.9–1.7 mya, before the development (or at least rise to dominance) of bifacial industries (Foley and Lahr 1997). The earliest date for the latter is currently c. 1.7–1.6 mya (Carbonell et al. 2008). However, it does not explain the fact that Asia east of the Movius Line is not actually devoid of bifacial technology (e.g. Corvinus 2004; Mokhtar Saidin 2006) – any more than was the Developed Oldowan A (1.65–1.53 mya) (Kimura 2002). Neither does it explain why some early European industries, such as the Clactonian, Bilzingsleben and Vértessölös, did not contain bifaces (Ranov 1995). Across most of Asia (outside South Asia) and for an as yet unknown reason Mode-2 technology never became as dominant and pan-continental as it did in Africa or Europe. There remains considerable support for seeing the Movius Line as a raw material boundary (e.g. Foley and Lahr 2003). A general lack of fine-grained stone is argued as the main cause of the distinctions between Afro-European and most Asian industries (Klein 1999; Mellars 2006).

A long history of archaeological investigation continues to show that the Pleistocene contained at least two broad technological and potentially behavioural "lines of development," whose last common point was sometime around 1.9 mya-though some writers suggest that there is no reason that it could not have been earlier, e.g. Dennell and Roebroeks (2005). The Asian record would come to contain its own subgroupings of technological variability, but its trajectory would take a different path to the Afro-European, as the following sections examine in more detail.

Southeast Asia

The Palaeolithic archaeology of Southeast Asia is well known for its pebble and flake-based technologies and for the significant degree of overlap that exists between its industries – a characteristic that makes definitive "cultural" allocations geographically mercurial and temporally ambiguous. The arrival of anatomically modern humans did not seem to impact significantly on this technological suite (Fig. 6.2 and Table 6.1).

Fig. 6.2 The locations of Southeast Asian sites discussed in the text

Table 6.1 Upper Pleistocene to Early Holocene sites from Southeast Asia listed with their associated industries

Site name	Location (decimal degrees) Lat.	Long.	Layer	Date range (uncalibrated for ^{14}C) bp	Method	Lithic industry description	Raw material provenance	Hominin remains
Golo Cave	–0.01	129.40	195–200 cm	9,260 ± 80 (ANU-9768)	^{14}C	–	–	–
			210–215 cm	32,210 ± 320 (WK-4629)	^{14}C	Flake	Local (but >5 km)	–
Gua Balambangan	7.25	116.91	40 cm	8,930 ± 150 (Beta-109140)	^{14}C	Flake (no pebble tools)	c. 1 km	–
			150 cm	16,800 ± 210 (Beta-105172)	^{14}C	Flake (no pebble tools)	c. 1 km	Teeth
Gua Gunung Runtuh	5.117	100.967	70 cm	9,460 ± 90 (Beta-37818)	^{14}C	Pebble tools (<10% flake implements)	–	Skeletal
			190 cm	13,600 ± 120 (Beta-38338)	^{14}C	Pebble tools (<10% flake implements)	–	–
Kota Tampan (KT1987)	5.054	100.973	Ash	68 ± 7 (No laboratory code)	Fission track	Flake tools predominate	Local	–
(KT2005)	5.055	100.973	4	c. 70 kya (No laboratory code)	OSL	Flake tools predominate	–	–
Lang Kamnan	13.981	99.420	II	7,540 ± 180 (OAEP-1179)	^{14}C	Pebble and flake	<5 km	–
			III (hearth)	15,170 ± 70 (Beta-70982)	^{14}C	Pebble and flake	<5 km	–
			IV	27,110 ± 500 (GX-20072)	^{14}C	Pebble and flake	<5 km	–

(continued)

Table 6.1 (continued)

Site name	Location (decimal degrees) Lat.	Long.	Layer	Date range (uncalibrated for ^{14}C) bp	Method	Lithic industry description	Raw material provenance	Hominin remains
Lang Rongrien	8.216	98.883	6	9,655 ± 90 (SI-6817)	^{14}C	Pebble and flake (Hoabinhian)	Local	–
			8	27,110 ± 615 (SI-6816)	^{14}C	Flakes predominate (c. 8% pebble tools)	Exotic – some local	–
			9	37,000 ± 1,780 (SI-6819)	^{14}C	Flake predominate (5% pebble tools)	Exotic – some local	–
Leang Burung 2	–5.00	119.65	IIIa	19,405 ± 250 (BM-1492)	^{14}C	Flake (incl. Levallois technique)	c. 15 km	–
			II	30,848 ± 330 (GrN-8649)	^{14}C	Flake (incl. Levallois technique)	c. 15 km	–
Niah Caves	3.819	113.778	36–48″	9,995 ± 40 (OxA-15157)	^{14}C	Flake – some pebble tools	–	Skeletal
			Direct date	35.2 kya (No laboratory code)	U-series	Flake	c. 48 km – some local	Cranial (+ skeletal)
			(3158)	45,900 ± 800 (OxA-V-2057-31)	^{14}C	Flake	–	–
Patjitan [a]	–8.13	111.01	Terrace	Late Pleistocene or Early Holocene	Terrace formation	Pebble and flake (Hoabinhian)	–	–
Sai Yok cave [a]	14.13	99.11	–	9–11 kya	Typological	Pebble (flake debitage) (Hoabinhian)	–	–

Tabon Cave	9.273	117.976	IB	9,250 ± 250 (UCLA-284)	^{14}C	Flake	Local	–
			Direct date	16,500 ± 2,000 (No laboratory code)	Th/U	–	–	Frontal bone
			III	23,200 ± 1,000 (UCLA-699)	^{14}C	Flake and pebble	Local – some un-sourced	–
			IV	30,100 ± 1,100 (UCLA-958)	^{14}C	Flake	–	–
			Direct date	31 + 8/–7 kya (No laboratory code)	U-series	–	–	Mandible
			Direct date	47 + 11/–10 kya (No laboratory code)	U-series	–	–	Tibia
Tingkayu	–4.75	118.0	N/A	<28,300 ± 750 (ANU-3444A)	^{14}C	Pebble and flake (including lanceolates)	Local	–

Only selected dates – including those discussed in the text – are given for each site
[a]The coordinates of this location are approximate

Although Southeast Asia was not subject to the glaciation experienced in the higher latitudes of continental Asia during the Pleistocene, there is increasing palynological and geological evidence that changes in this tropical environment were more significant than once thought (Flenley 1997). The overall climatic picture for the Pleistocene in Southeast Asia was one of relatively cooler conditions with more marked seasonality, 30–50% lower annual rainfall and lowland temperatures depressed by c. 5°C during the Last Glacial Maximum (Broecker 1996). The exposure of the Sunda Shelf (a territory covering much of the present-day South China Sea) and the consequent changes to prevailing ocean currents also brought a more continental climate to many areas that are presently maritime.

The lowland landscape was also subject to comparatively dynamic changes. The late appearance of strict arboreal primate fauna in Java during MIS 5e (c. 131–114 kya) is taken as an indication that contiguous rainforest did not exist between the mainland and Java until around this time (van den Bergh et al. 2001). The presence of an open, "savanna-like" environment stretching from the eastern highlands of Sumatra, through the heart of the Sundaland and into Borneo is thought to have helped facilitate the arrival of *Homo erectus* – c. 1.8 mya (Swisher et al. 1994) and later, *H. sapiens* into the region (Bird et al. 2005). Forest cores are recognised as persisting in parts of Sumatra and northern Borneo, and there was probably a network of forests along the main river valleys that stretched across the Sunda plains. Though as recent work at the Niah Caves (Fig. 6.2), Sarawak has demonstrated that even within such refugia lowland rainforest may have been episodic and non-analogous to modern conditions (Barker et al. 2007; Hunt et al. 2007). These data have revealed a much more complex picture of early modern human settlement than the one that revolved a decade ago around their ability to adapt to continuous closed rainforest (e.g. Headland 1987; Bailey et al. 1989). Evidence of occupation at Niah from as early as c. 47 (cal.) kya suggests that early modern humans here were capable of exploiting arboreal and terrestrial fauna, employed technologies to extract carbohydrates, and successfully neutralised plant toxins (Barker et al. 2007; Barton 2005; Rabett and Barker 2007).

There is also increasing evidence that Late Pleistocene tropical hunter gatherers focused on exploiting water courses and lake environments, which also appear to have provided the bulk of lithic raw materials. Research into site function is still in its infancy in Southeast Asia, and changes in lowland forest structure complicate matters further. However, it has been suggested that because tropical environments tend to favour high species diversity and low species density, hominins would likely have needed to be very mobile and excessive transportation of stone between locations may have been prohibitive (Reynolds 1993). It is difficult to determine the Pleistocene distribution of versatile materials, such as rattan and particularly bamboo, whose utilisation often figures in explanations for Southeast Asia's low-input lithic industries (e.g. Mellars 2006; Pope 1988). Bamboo distribution, though, is thought to have been comparable to that of today, if less evenly spread across the region (Raddatz 2006, 22). Modification of these organic materials into effective technologies can be carried out using unspecialised and simply-made implements. Lithic use-wear studies on Southeast Asian assemblages are still few in number (e.g. Bannanurag 1988; Mijares 2000, 2002; Sinha and Glover 1984); however, high magnification studies of quartz and basalt have shown that use-wear recovery from coarse-grained stone artefacts is feasible (Raddatz 2006). Foley and Lahr (1997) nonetheless, rightly, point out that the significance of bamboo as a major driving force affecting the development of stone technology here might be overestimated as all evidence of hominin occupation outside Africa before 500 kya involve broadly similar (Mode-1) industries, irrespective of environment.

Establishing the provenance of lithic raw materials used in tool manufacture is one of the avenues that researchers use to assess Pleistocene group mobility and behavioural patterns in Africa (Ambrose 2001; Minichillo 2006). In Southeast Asia, the appearance of non-local stone in tool assemblages and the potential significance of this have yet to be addressed systematically. While lithic raw materials collected in the immediate vicinity of sites do appear to dominate (e.g. Anderson 1997; Forestier et al. 2006; Ha Van Tan 1976; van Heekeren 1972; Mokhtar Saidin 2007a; Moore and Brumm 2007; Reynolds 2007; Shoocongdej 1996), there are instances of materials arriving from considerably further afield. For example, Zuraina Majid (1982) noted that the nearest collection point for some of the lithic material used in the Pleistocene levels of the West Mouth, at Niah (sandstone, chert and jasper), was likely the Tinjar River, approximately 48 km away. At Leang Burung 2, 94% of the lithic assemblage was fabricated on three varieties of chert from a river bed c. 15 km away (Glover 1981). The contention that the amorphous character of Southeast Asian lithics may be put down to a lack of "good quality" stone persists in the literature (van Stein Callenfels and Evans 1928; Mellars 2006; Pookajorn 1985; White and Gorman 2004). However, in most cases, this is questionable. Standardisation of flake morphology could be achieved on coarse-grained materials if it was deemed important to do so (T.E.G. Reynolds, personal communication). In all likelihood, there is a significant element of choice in the way this technology was manufactured that we do not yet fully appreciate.

While non-cryptocrystalline rocks often dominate the region's Palaeolithic assemblages – e.g. 96.90% through three phases of occupation at Lang Kamnan in Thailand from the Late Pleistocene to Mid-Holocene (Shoocongdej 1996, 2000), they are part of a wide range of other recovered raw materials. These include not only andesite, basalt, geyserite, limestone, quartz, quartzite, sandstone, shale and suevite, but also fine-grained cherts, jasper and chalcedony (Anderson 1997; Forestier et al. 2006; Fox 1970; Glover 1981; Mijares 2002; Mokhtar Saidin 2007b; Pookajorn et al. 1996; Shoocongdej 1996; Tweedie 1940, 1953; Zuraina Majid 1982; Zuraina Majid et al. 1994, 1998a, b). Importantly, Reynolds (1989, 1990) has pointed out that even where fine-grained stone is present in assemblages, this does not appear to have, by and large, resulted in Pleistocene knappers pursuing radically different reduction strategies (see also Morwood et al. 2008; Movius 1978). There is one notable exception and one curious exception to this regional trend that are worth considering.

On at least one occasion Pleistocene humans appear to have had the need or developed the propensity to produce stone tools of a far more formal character – though once again these seem to have been added to an otherwise unspecialised tool suite. While the bulk of the lithic assemblages from Tingkayu locales I and II in Sabah comprises unexceptional large pebble tools, cores and flakes made of brown chert pebbles or grey tabular chert, a subset of the assemblage comprises bifacial pieces and lanceolate points (Bellwood 1988). Tools of this form have not thus far been found anywhere else in Borneo or in the rest of Southeast Asia. The large number of flakes recovered is interpreted as likely debitage from biface production (which appears to have occurred in two forms, ovate/rectangular and pointed). The sites are dated provisionally to between 28 and 17 kya. The latter date is based primarily on the fact that nothing remotely similar appears in any of the later lithic assemblages from the nearby cave sites in the Baturong and Madai massifs, where human occupation is dated to between 17 and 7 kya.

The second exception to this rule involves the c. 31–19-kya dated site of Leang Burung 2 in southern Sulawesi which, also unique for the region, has produced Levallois-like points ($n=7$) from three of its levels. While no classic Levallois cores were recovered, bifacial cores ($n=3$) capable of producing such points were found in related contexts and indicate the presence of a minor but very different technology to that prevailing in this and most other Southeast Asian Pleistocene assemblages (Glover 1981, 25–29). The significance of these isolated occurrences has yet to be established, but it implies that the capability to produce such techniques of flaking did exist among some populations in this region but was not a widespread or persistent characteristic of stone technologies here.

Although knappers do not seem generally to have used a different sequence of reduction and trimming particular to fine-grained stone, this is not to say that raw material was incidental to technological systems. On the one hand, a range of different materials might be used to create broadly the same kind of artefact, as seen for “adzes” at Sai Yok, Thailand (Kamminga 2007). On the other, Zuraina Majid has suggested that the natural form and mechanical characteristics of raw materials influenced what was chosen to carry out particular tool-assisted tasks (Zuraina Majid 2003; Zuraina Majid et al. 1994, 1998a).

Many Pleistocene sites in Southeast Asia have been characterised as “lithic workshops.” At these sites, debitage often dominates (e.g. Fox 1970; Glover 1981; Mokhtar Saidin 2006; Zuraina Majid and Tjia 1988) and may be a further indication of an expedient approach to stone tool manufacture (Reynolds 1993). However, a different situation at Lang Rongrien, in southern Thailand (Anderson 1997), and a general low incidence of lithics at the Niah Caves (Reynolds 2007), does imply that there existed subtleties in where and when (and why) lithic reduction episodes occurred that have yet to be fully explored. Recent work by Moore and Brumm (2007) has presented an intriguing and significant reconsideration of the long-standing analytical distinction made between Southeast Asia’s separate “core tool” and “flake tool” industries (e.g. see Bellwood 1997).

Drawing on original analysis of two separate lithic assemblages from the island of Flores and supported by a wide-ranging review of the literature, these authors conclude that rather than representing two independent industries, core and flake tool assemblages are, in truth, spatially separated elements of a single stone reduction strategy (Moore and Brumm 2007). Within their sample, large flakes were struck from comparatively heavy raw materials at source and these, rather than the cores, were transported across the landscape and later deposited as part of activities carried out in caves and rock shelters. They contend that it is this process that has produced two seemingly different industries. Their research also suggests that this basic component of stone tool behaviour may have an extraordinary time depth (in excess of 800 ky) and was employed across species boundaries between *H. erectus*, *H. floresiensis* and *H. sapiens*. Variation does occur between assemblages, and this supports another long-known (e.g. see Movius 1944, 104), but rarely considered observation that distinctions between Late Pleistocene Southeast Asian industries appear to be built around the adding or removing of certain tool elements onto a basic, widespread and long-term suite. Support for this idea can be found elsewhere in the region. For example, Zuraina Majid et al. (1994) observed that the lithic assemblage studied from the site of Gua Gunung Runtuh – with an occupation spanning the Pleistocene–Holocene transition (Zuraina Majid et al. 1994; Zuraina Majid 1998) – was produced following the same technological approaches recorded at the much earlier (c. 74 kya) local site of Kota Tampan – KT1987 and KT2005 (Hamid Mohd Isa 2007), with variation appearing principally in the range of "types" present. Indeed, Moore and Brumm (2007) contend that the "grafting" of new elements onto this base technology may be characteristic of modern human adaptation to this region.

European Late Pleistocene tool production indicates a preference for using a comparatively narrow range of *specific raw materials*, which would then have many and varied (task-specific) forms imposed upon them. In contrast, in Southeast Asia, it may be the case that *specific activities* were a guide to raw material selection and, by extension, one of the factors in locating sites in the landscape. Access to fine-grained raw materials was probably *not* an overriding criterion driving the lithic industries of this part of the world. Even when sites were located very close (sometimes less than 1 km away) to chert or other silicified deposits, e.g. at Gua Balambangan off the north coast of Sabah (Zuraina Majid et al. 1998b), Tabon, Palawan (Fox 1970, 1978), or the Punung area of Java (Sémah et al. 2004), knappers still produced industries that were Mode-1 based despite evidence that there was considerable awareness about the facility of different raw materials.

Such awareness is also apparent in surviving non-lithic tools. The presence of bone or shell implements in archaeological assemblages has often been cited as a response to a lack of suitable locally available stone. Contrary to this assertion, research carried out into regional bone technology (Rabett 2002, 2005) and Pleistocene shell technology (Szabó et al. 2007) strongly suggests that early human groups had a practical grasp of the mechanical properties of different materials in the environment.

Early Southeast Asian bone technology appears to exhibit a pattern of minimally invasive modification: preexisting forms were augmented, but significant imposition of form is a comparative rarity (Rabett and Barker 2007). This technological scheme does not appear to change from its earliest appearance – currently c. 45 kya (Pasveer 2006; Rabett et al. 2006) – until the Mid-Holocene, when bone implements carrying greater imposed form begin to appear within otherwise low-input bone tool assemblages, e.g. at Khok Phanom Di in south central Thailand, or Ban Lum Khao in central Thailand (Rabett 2002, 2004).

Excavation at Golo Cave, Gebe Island, Indonesia, has revealed early evidence (32–28 kya) of systematic and standardised knapping of the operculum from a large species of subtidal zone gastropod (*Turbo marmoratus*), existing alongside an expediently flaked lithic industry. Since the reef and lagoon habitats of this species do not appear to have been targeted for subsistence purposes, a deliberate collection strategy is favoured (Szabó et al. 2007). Although extensive shaping of raw material may have appeared earlier in the region's shell technology on the basis of this evidence, indications are that the process of manufacturing was also influenced by the natural form of the material (Szabó et al. 2007).

Modifying different media through material-specific operational sequences is inkeeping with current expectations about modern human capabilities from this and other regions. The difference in Southeast Asia is that the employment of standardised typological templates was far less pronounced. It did occur during the Late Pleistocene, as both the Tingkayu and Golo examples demonstrate, but did not seem to rise to significance until the Holocene. Of the three durable tool media (stone, bone and shell), shell may have been perceived to offer techno-cultural possibilities the others did not, although further research will be needed to confirm this. Overall, a lower incidence of imposed form and tool standardisation almost certainly results from more than a simple lack of suitable raw materials. One may conclude that rather than marking part of a global shift in human behaviour, the incidence of imposed technical form, like parietal art, may have more parochial roots (Rabett and Barker 2007).

The arrival date of *H. sapiens* in Southeast Asia is still open as human remains are very scarce. However, increasing use of direct dates on those that do exist points to their presence in Island Southeast Asia from at least c. 37 (cal.) kya (Barker et al. 2007). It is possible that early modern humans entered the region as early as the last major faunal turnover 128–118 kya (van den Bergh et al. 2001; Westaway et al. 2007). This finds some credence through the identification of a putative modern human molar within faunal remains from this turnover (Storm et al. 2005); from archaeological sites (without human remains) dated to MIS 4 and older in Peninsular Malaysia (Mokhtar Saidin 2007b; Zuraina Majid 2003); and through recent mitochondrial DNA (mtDNA) studies that indicate a time depth for modern Australo-Melanesian groups of c. 60–65 kya (Macaulay et al. 2005). However, an MIS 4/5 arrival of *H. sapiens* would require a considerable reassessment of the initial exit from Africa (Stringer 2007).

Central Asia

The Central Asian archaeological record is discontinuous, although it has particular importance as it falls (together with part of South Asia) at the western extent of the Asian technological complexes and the line of potential contact between eastern and western provinces of the Pleistocene Old World (Fig. 6.3 and Table 6.2).

Fig. 6.3 The locations of Central Asian sites discussed in the text

Although there are putative hand-axe industries from sites in Kazakhstan and Turkmenistan, evidence for substantial occupation of the region before the Middle Pleistocene is currently scarce. Kul'dara, an open-air site near Khovaling, Tajikistan (Fig. 6.3), contains some of the earliest archaeological evidence from this part of Asia: c. 850 kya (Ranov 1995; Ranov et al. 1995). The site lies at the bottom of a gorge of the same name and contains only a small ($n=40$) collection of verifiable artefacts. Mostly, the assemblage comprises small flakes and flake tools, though two blades and part of a putative bifacial element are described (Ranov 1995). There is no indication of standardisation in form on any of the pieces. Although pebble tools do not appear, the knapping technique does not involve core preparation and is considered most likely to belong to a pebble-tool industry (Ranov 1995). The assemblage is made on felsite porphyry, quartzite, silicified volcanic rocks, silicified schists, limestone and poor quality flint of unknown provenance but pebble form.

During the Middle Pleistocene, the site ledger increases to c. 70 sites from a variety of contexts – rock shelters, river terraces and other open-air locales. Even with this larger sample, though, dating resolution is still very patchy: chronologies are still heavily reliant on typological seriation and geological formations (Vishnyatsky 1999). Furthermore, many of the recorded archaeological locales comprise collections of surface finds – often of pebble tools. The difficulty of placing such Mode-1 industries to particular time periods in Asia only compounds this situation. A series of sites in the loess palaeosols of southern Tajikistan, however, are

Table 6.2 Middle Pleistocene to Early Holocene sites from Central Asia listed with their associated industries

Site name	Location (decimal degrees) Lat.	Long.	Layer	Date range (uncalibrated for ^{14}C) bp	Method	Lithic industry description	Raw material provenance	Hominin remains
Karasu[a]	41.65	73.183	Upper	24,800 ± 1,100 (No laboratory code)	^{14}C	Mousterian-UP industry	Most c. 1 km	–
Karatau I[ab]	38.116	69.117	Overlying 6	194,000 ± 32,000 (No laboratory code)	TL	–	–	–
			Palaeosol 6	–	–	"Pre-Mousterian" Pebble and flake industry	Local predominate	–
			Underlying 6	210,000 ± 36,000 (No laboratory code)	TL	–	–	–
Khudji	38.614	68.213	8	42,100 + 2,400/−1,870 (GrN-23686)	^{14}C	Mousterian with blade component	Local – some exotic	Tooth
Kul'dara[a]	38.14	69.84	Overlying 11	780 kya (No laboratory code)	Palaeomag.	–	–	–
			11 and 12	c. 800 kya (stratig. Association)	–	Pebble and flake	Local	–
Lakhuti I[ab]	38.21	70.06	Palaeosol 5	150–130 kya (No laboratory code)	TL	Pebble and flake (minor blade element)	Local	–
Lakhuti III[a]	38.21	70.06	Palaeosol 4	90–70 kya (No laboratory code)	Geological	Pebble and flake (minor Mousterian)	Local	–

Obi-Rakhmat	41.569	70.133	4	19,700 ± 4,000 (No laboratory code)	^{14}C	UP with Mousterian elements	Local	–
			7	46,170 ± 810 (No laboratory code)	^{14}C	UP with Mousterian elements	Local	–
			10–14	48,800 ± 2,400 (AA-36746)	^{14}C	UP with Mousterian elements	Local	–
Samarkandskaya	39.65	66.94	Unknown	–	–	UP components with pebble tools	–	Mandibles (2)
Sel'ungur	39.94	71.34	Overlying 1	126,000 ± 5,000 (LU-936)	U-series	–	–	–
			3	–	–	Pebble and flake	–	Teeth (6)
Teshik-Tash	38.336	67.143	1	–	Undated	Mousterian ("typical")	Local	Skeleton

Only selected dates – including those discussed in the text – are given for each site

[a] The coordinates of these locations are approximate

[b] Recent studies of loess sedimentation rates and magnetic susceptibility at both these sites suggest an age 400–600 kya (Davis and Ranov 1999)

stratigraphically relatable to one another and can be linked to geomagnetic chronology (Shackleton et al. 1995). Together, these provide one of the clearest sequences of temporal development in lithic industries available for the Asian Middle Pleistocene. One of these occupations, the open-air site of Karatau I is situated 1,700 m asl in the watershed of the Yavansky Karatau Range and contains a pedocomplex attributed to MIS 15 (c. 600 kya) (Vishnyatsky 1999). The stone tool industry here (c. 600 artefacts) consists of mostly flakes, flake tools and "choppers"; bifaces are absent. Tools were made predominantly on locally obtained and fragile metamorphic pebbles, with some elements made on poor quality flint, silicified limestone, schist and quartzite pebbles, also available on the terraces of the local river (the Vaksh). As at Kul'dara, the knapping technique, its components and absence of standardisation mean that it is classed as a pebble-tool industry (Ranov 1995).

About 80 km east of Karatau I, the site of Lakhuti I was discovered within similar loessic deposits, on the right bank of the Obi-Mazar River. The density of artefacts here is higher and again dominated by flakes, scrapers and choppers. Raw materials were diverse: quartzite, sandstone, hornfels, cornelian, crystalline cherts and possibly andesite, all locally available in pebble form. Though regarded as "less archaic" than Karatau (it contains several prepared cores and other Levallois flake-like elements, as well as a slightly higher incidence of blade-like forms), the Lakhuti I industry is described as being derived from the same pebble-tool tradition (the "Karatau culture") and is attributed to MIS 13 (c. 500 kya) (Ranov 1995; Vishnyatsky 1999).

Lakhuti III (c. 1.5 km away from Lakhuti I) is also reported as another derivation of the same basic pebble-tool industry, dated to c. 400 kya. According to Ranov (1995), the lithic assemblage here is dominated by debitage (67%) and flakes (15%) on a similar range of raw materials as that seen at Lakhuti I. These data, though, derive from section cleaning only and may not be representative.

The cave site of Sel'-Ungur on the Sokh River, Kyrgyzstan, is one of the few sites in the region that is radiometrically dated to the Middle–Upper Pleistocene boundary. A Uranium-series date on a travertine layer overlying the cultural deposits providing a minimum age of 126,000 ± 5,000 bp (LU-936) is consistent with the character of both palynological and faunal evidence from the site. The lithic industry is made primarily on jasper and slate pebbles, and comprises a range of choppers, side scrapers, notches and denticulates and retouched flakes. A single, questionable biface exempted, the assemblage is akin to those from Karatau and Lakhuti (Vishnyatsky 1999). Thus the full span of Middle Pleistocene occupation of Central Asia appears to be characterised by Mode-1 industries made on locally obtained raw materials, with occasional technical additions appearing along side this.

Upper Pleistocene sites are virtually unknown from Central Asia. No significant settlement has so far been recorded from the plains and mountains between the Caspian and Aral Seas, and few sites across the whole region can be attributed to the early part of the Late Pleistocene with any confidence (Vishnyatsky 2004).

Ranov (1990) states that while the Karatau culture shows marked stability over a great time span (800–70 kya), this is followed by a no less marked change of direction with the first appearance of the Mousterian. On current evidence, these

Central Asian Mousterian industries – of which four typological and geographical facies were recognised (Ranov and Davis 1979) – may have been introduced from elsewhere, though this is not yet proven. It appears that while bifacial (Mode-2) technologies appear not to have become established in Central Asia, different techno-facies of prepared core (Mode-3) industries did.

The most well-known of the Mousterian sites is Teshik-Tash cave, Uzbekistan. One of the very rare instances of Pleistocene inhumation in this region was discovered here. A Neanderthal child – recently confirmed by mtDNA analysis (Krause et al. 2007) – probably a young girl, was found in cultural Layer 1 covered by a "protective" cage of goat horns (Movius 1953). Surprisingly, despite its significance, Teshik-Tash remains undated, so associations can only be provisional. All five cultural levels are attributed to the Typical Mousterian (Ranov and Davis 1979) and exhibit no significant typological variation between them (Movius 1953, 31). The predominant lithic raw material consisted of a relatively fine-grained siliceous limestone that was available in the immediate vicinity of the cave. In addition, jasper, quartzite, quartz, volcanic rock and, very rarely, flint were also exploited; most, if not all, of these additional materials were also collected locally. More intensive reduction strategies of the finer-grained rocks are indicated, suggesting perhaps a certain preference for these over the coarser materials, although the latter was still used seemingly because they were immediately available (Movius 1953, 29).

Great uncertainty exists around the nature and agents of the transition from the Mode-3 to the blade-based (Mode-4) Upper Palaeolithic technologies in Central Asia. At Obi-Rakhmat, NE of Tashkent, Uranium-series dating (in 1969) placed one phase of occupation to a time comparable to Sel'-Ungur, c. 125,000 bp (though the error margin is very large). A second Uranium-series sample produced a date of 44,000 ± 1,000 bp. Unfortunately, the exact location of each sample has been lost, rendering both questionable. The latter of the two is in concordance with a series of new AMS radiocarbon dates taken recently (1998–2001) from the middle layers at the site (Layers 6–14), most of which are between 48,800 ± 2,400 bp (AA-36746) and 36,170 ± 810 bp (no lab code) (Fedeneva and Dergacheva 2006; Krivoshapkin et al. 2006), though not without reversals. The total lithic assemblage (c. 31,000 artefacts) is made on locally available silicified limestone, quartz and quartz sandstone pebbles, though there is no consensus about how it should be classified. Classic Upper Palaeolithic prismatic cores are absent, though apparently "proto-prismatic" cores have been identified. Most agree that the industry exhibits unidirectional change from lower to upper layers with increasing numbers of Upper Palaeolithic elements, such as retouched blades and burins, though debate continues about how they compare to classic Bordian forms. For example, Vishnyatsky (1999, 98–99) notes the presence of burins and endscrapers, and argues that these are "far from Upper Palaeolithic standards" and that the industry as a whole is Middle Palaeolithic. Krivoshapkin et al. (2006, 15) see a "predominance of Upper Palaeolithic tool types" and rather than being wholly Middle Palaeolithic in character, they suggest that it retains Middle Palaeolithic elements, e.g. Levallois and Mousterian points, in appreciable percentages – e.g. 16.1 and 6.3% respectively in Layers 6–9, from a total tool kit in these layers of 174 specimens.

Similar technological ambiguity is reported from the site of Karasu, c. 140 km north of Chimkent in Kyrgyzstan. This site has an uncalibrated radiocarbon date of 24,800 ± 1,100 bp from the top of five cultural layers. The industry contains scarce blades or prismatic cores. It could again be considered Middle Palaeolithic technologically; however, typologically, endscrapers make up more than half of the assemblage, burins are present and the methods of retouch employed all mark this as an Upper Palaeolithic industry (Vishnyatsky 2004).

In contrast to the Middle Palaeolithic assemblages, these appear to support a model of gradual technological development of Upper Palaeolithic industries out of a foundation of different local Mousterian techno-facies, rather than something brought by an immigrating population (Krivoshapkin et al. 2006; Kuhn et al. 2004). But how to account for such technological continuity in light of western Eurasian evidence, which all but confines the production of Upper Palaeolithic technology to anatomically modern humans and registers its appearance in terms of replacement?

Determining exactly who was making what technology in Central Asia during the Upper Pleistocene is frustrated by the extreme scarcity of hominin remains. Aside for Teshik-Tash, the only other hominin finds of note come from Sel'Ungur: six teeth with mosaic characteristics that place them somewhere between *H. erectus* and *H.neaderthalensis* (e.g. Vishnyatsky 1999). In 1997, a human deciduous (probably) I_2 was also recovered from the site of Khudji, Tajikistan. Though clearly human, the tooth has a range of subtle morphological features, only some of which resemble Late Pleistocene Neanderthals, making it slightly enigmatic (Trinkaus et al. 2000). Evidence of verified anatomically modern human remains from the Late Pleistocene is even more scarce and not helped by the paucity of sites. The only notable material evidence is fragments of two mandibles and teeth found in 1962 and 1966 respectively during excavations at the site of Samarkandskaya, Uzbekistan, but their exact provenance remains unclear (Vishnyatsky 2004).

Work by Comas et al. (2004) on mtDNA from a sample of 12 modern Central Asian populations suggests that most of their present genetic make-up derives from already differentiated groups. These appear to have come from eastern and western Asia, and with elements of the East Asian haplogroups G and D arriving from 30–25 kya. The mtDNA evidence implies slightly younger ancestry than that suggested by Y-chromosome analysis carried out on modern Central Asian populations (Spencer Wells et al. 2001). The Y-chromosome data indicates that a very small fraction of the modern gene pool may date to the populating of Central Asia by anatomically modern humans 40–50 kya. An unknown period of overlap between modern and archaic hominins is implied – and dating of Teshik-Tash is crucial here. If proven, Central Asia seems to have followed quite a different developmental trajectory to that by western Eurasia.

The assemblages discussed briefly here do not represent in any way an exhaustive survey. As some of the better dated sites covering the period from the apparent earliest colonisation to the middle part of the Late Pleistocene, they do offer a window into the character and persistence of technological forms. Mode-1 industries, the "Karatau culture," persisted from c. 850 kya until well into the Upper Pleistocene (Ranov 1990, 1995). Building a robust understanding of the significance of localised techno-facies of the Karatau culture will have to await future research, as will the related issue of explaining observed variability in relation to inter-site functionality. The diversity of raw materials used might suggests that access to locally convenient packages of stone was more important than the targeting of material of defined "quality". There remains no indication of who was making these industries.

The sudden appearance of Mode-3 industries during the late Middle or early Upper Pleistocene marks a significant technological shift and is consistent with the arrival of an incoming population. It is unclear whether this was of Neanderthals alone or archaic modern humans as well. However, the developmental trajectory of these Mousterian industries, once established, seems to have followed a different track from that seen in western Eurasia. Local techno-facies are diverse during the Late Pleistocene, making any inter-site comparisons on typological grounds problematic (Vishnyatsky 2004). Yet, in an interesting parallel to earlier periods and the story in Southeast Asia, this diversity is set against a background of persisting technological and typological features, this time of Middle Palaeolithic character. Most raw materials continue to be locally acquired and tools are made on a range of stone types.

The evidence for continuity and localised development from Middle and Upper Palaeolithic industries does not sit easily with the European picture of punctuated Pleistocene cultural turnover, or with the assumptions that have arisen from them about fundamental differences between modern and archaic hominin behaviour. There remain many gaps in this record and the potential effects of variables such as climate have yet to be fully accounted for, but as things stand, the evidence continues to support the contention that the sequence of material culture development apparent in Europe is not seen in Central Asia any more than it was in Southeast Asia and this fact cannot be ignored.

North Asia (Altai Mountains)

Lying on the margin of SW Siberia (Fig. 6.4, Table 6.3), the Altai is regarded as the most likely "gateway" for the initial peopling of Siberia, probably from Central Asia (Derev'anko et al. 1998). The Altai Mountains (up to 4,506 m asl) dominate the south, and to the east are the lower mountain ranges of the Salatir and Alatau flanking the Kuznesk Basin, while open lowlands to the north are laced by the River Ob and its tributaries. The mountains afford protected valleys with their own localised micro-climates that under modern conditions are generally less severe than those experienced on the open steppe. Thus, the annual temperature range and receipt of rainfall are strongly dependent on the topographic setting (Chlachula 2001).

During the Middle Pleistocene, warm interglacial, mixed forest conditions, likely associated with MIS 11 (c. 400 kya), were followed by a marked deterioration in climate (MIS 10). Major periods of glacial advance occurred at this point and again,

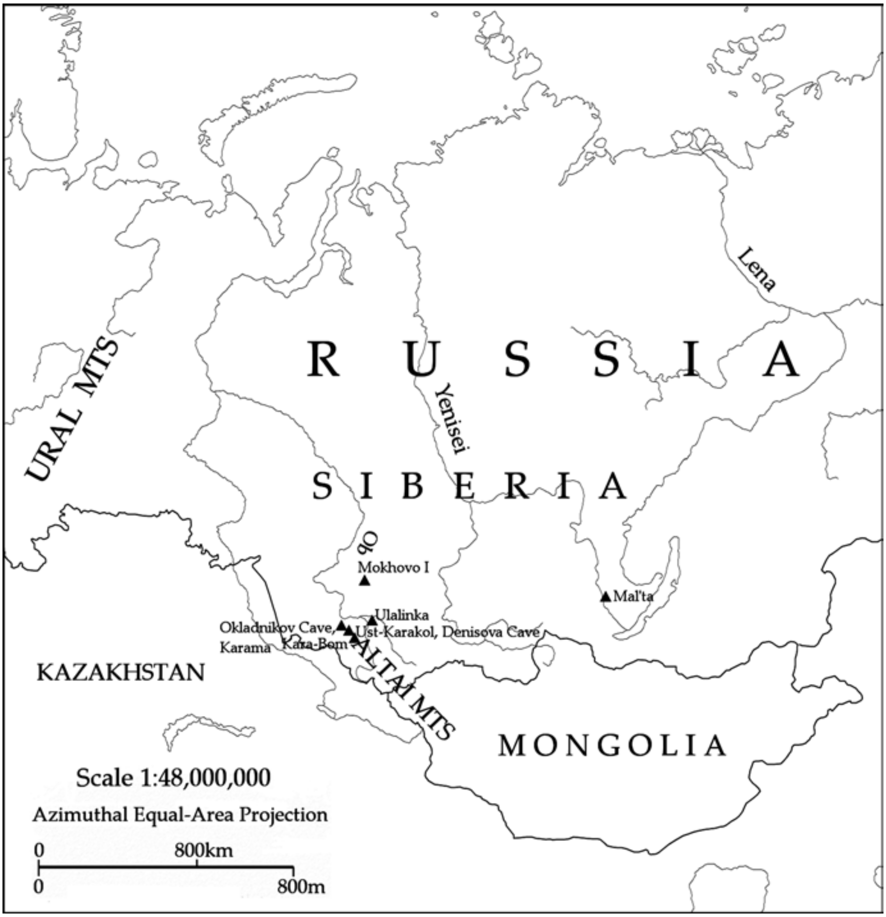

Fig. 6.4 The locations of North Asian sites discussed in the text

and more extensively, c. 230 kya (MIS 8), with an intervening return to broadleaf forest taxa during MIS 9. The Altai region experienced two major glaciations during the Upper Pleistocene. The Zyriyanka (MIS 5d-4) was followed by the Karginsky "interglacial" 50–25 kya, which effectively falls within the boundaries of MIS 3 (59–24 kya), while the subsequent and lesser Sartan glacial period (23.5–10.5 kya) corresponds to MIS 2. The Karginsky is recognised as having spells of climatic amelioration and deterioration. During these "optimum" warm periods, woodlands expanded as far north as the lower reaches of the Ob and Yenisei rivers, while northern parts of west and eastern Siberia were mostly sedge and grass tundra with limited forest development in sheltered aspects. In sum, marked and repeated cycles of glaciation brought significant changes in vegetation and landscape form to the region, while topographic features further fractured these conditions into smaller, localised climatic units (Chlachula 2001; Kuzmin and Orlova 1998).

Table 6.3 Middle Pleistocene to Early Holocene sites from the North Asian region of the Altai Mountains (plus selected refer sites) listed with their associated industries

Site name	Location (decimal degrees) Lat.	Long.	Layer	Date range (uncalibrated for ^{14}C) bp	Method	Lithic industry description	Raw material provenance	Hominin remains
Denisova Cave	51.383	84.666	9	46,000 ± 2,300 (GX-17062)	^{14}C	UP – some Mousterian elements	Local – some exotic	–
			12	101,000 ± 25,000 (RTL-612)	TL	Mousterian – some UP elements	Local	Tooth
			22.2	282,000 ± 56,000 (RTL-548)	TL	Mousterian	Local	Tooth
Kara-Bom	50.183	86.683	3	21,280 ± 450 (SOAN-300)	^{14}C	UP and Mousterian (declining)	<5 km – c. 5% exotic	–
			5	30,990 ± 460 (GX-17593)	^{14}C	UP (increasing diversity) and Mousterian	<5 km – c. 5% 80 km	–
			6	43,300 ± 1,600 (GX-17956)	^{14}C	UP (increasing diversity) and Mousterian	<5 km – c. 5% 80 km	–
			9	>44 kya (AA-8894A)	^{14}C	Mousterian – some UP elements	<5 km	–
Karama[a]	51.48	84.52	–	c. 775 kya (No laboratory code)	TL	"Pebble tool" industry	–	–
Mal'ta	52.816	103.55	8	21,000 ± 140 (GIN-7706)	^{14}C	UP (including significant mobiliary art)	Local – some exotic	Skeletal (2 juvs.)
Mokhovo I[a]	54.55	86.35	–	c. 430 kya (No laboratory code)	TL	"Acheulian" (flake and pebble)	–	–

(continued)

Table 6.3 (continued)

Site name	Location (decimal degrees) Lat.	Long.	Layer	Date range (uncalibrated for ^{14}C) bp	Method	Lithic industry description	Raw material provenance	Hominin remains
Okladnikov Cave	51.633	84.483	2	37,750 ± 750 (RIDDL-719)	U-series	Mousterian – some UP elements	–	Tooth
			3	40,700 ± 1,100 (RIDDL-720)	U-series	Mousterian – some UP elements	–	Teeth
			7	43,300 ± 1,500 (RIDDL-722)	U-series	Mousterian	–	Tooth
			3	30–38 kya[b]	^{14}C	Mousterian – some UP elements	–	Humerus (juv)
			2	24,260 ± 180 (KIA-27010)	^{14}C	Mousterian – some UP elements	–	Humerus (ad.)
Ulalinka[a]	51.9	85.9	Upper horizon	<13 kya (No laboratory code)	Geological	UP	–	–
			Lower horizon	c. 775 kya (No laboratory code)	Palaeomag.	"Pebble-tool" industry	Local	–
Ust-Karakol	51.383	84.683	5	31,345 ± 275 (SOAN-2869)	^{14}C	"Un-standardised" UP – some Mousterian	Local	–
			19A	133,000 ± 33,000 (RTL-661)	TL	"Poorly diagnostic"	Local	–

UP upper Palaeolithic technology

Only selected dates – including those discussed in the text – are given for each site

[a]The coordinates of these locations are approximate

[b]Represents a range of (uncalibrated) dates from different laboratories on this specimen: 29,990 ± 500 bp (KIA-27011), 34,860 ± 360 bp (Beta-186881), 37,800 ± 450 bp (OxA-15481) (Krause et al. 2007, Supplementary Material)

The initial appearance of hominins in the Altai probably occurred during one of the Middle Pleistocene interglacial periods as part of the northward expansion of warm-adapted biotic communities, possibly from Mongolia (Chlachula 2001). Whether these represented successful and sustained colonisation events, or shorter term forays is not clear. Technologically, this phase of occupation is represented by pebble-tool industries recovered from river terraces and former lake shores in the region, such as Ulalinka and Karama (Fig. 6.4). At Ulalinka, palaeo-magnetic results have even posited a Lower Pleistocene age, while geological studies of the upper culture-bearing terrace concluded an age less than 13 kya (Chlachula 2001; Shunkov 2005). The former may be dating the sandy loam of the lower cultural horizon containing abundant split quartzite pebbles and pebble artefacts; the latter may be dating the higher Upper Pleistocene horizon, though this cannot yet be substantiated (Okladnikov and Pospelova 1982). Karama was discovered in 2001 and is only dated by geological association to the very late Lower Pleistocene (possibly MIS 19). Interestingly, it is reputed to show indications of core preparation and secondary retouch alongside "archaic" morphological features (Shunkov 2005). In the Kuznetsk Basin, the open-air site of Mokhovo I has produced a small assemblage of less than a dozen quartzite pebble flakes. The overlying geological formation is TL dated to c. 430 kya and an associated faunal assemblage also corroborates a Middle Pleistocene age (Chlachula 2001).

Although less than adequate for comprehensive reconstruction, evidence for an early Middle Pleistocene hominin presence is to be found in the Altai and may represent one or more waves of immigration. Technologically, it appears to follow the continent-wide pattern of pebble-tool-based industries which occasionally display "Acheulean elements" of bifacial flaking (Chlachula 2001; Derev'anko et al. 2005), though some argue that Middle Pleistocene industries enter from adjacent parts of North and Central Asia (e.g. Shunkov 2005). These later industries are classified as various techno-facies of the Mousterian and have been identified at several sites in the Altai. They provide more robust evidence of a possibly more sustained early hominin occupation in this region. Radiometric dates, though, are still restricted to a small number of sites, and of these, Denisova Cave, the open-air site of Ust-Karakol, Okladnikov Cave and the open-air site of Kara-Bom are the most well dated and researched.

The extent of the cultural sequence at Denisova Cave (NW Altai foothills above the Anui River) is unmatched by any other site in Siberia. Small-scale excavation commenced here in 1977 and to date has uncovered 22 principal cultural layers from 282,000 ± 56,000 bp (RTL-548) (Layer 22.2) by TL dating, to 9,890 ± 40 bp by ^{14}C (Layer 1b) (Chlachula 2001; Derev'anko et al. 2005). The lower component of Layer 22 and the entirety of Layer 21 appear to correspond to interglacial phases, which may lend support to the early occupational pulse model. They contain comparatively small numbers of lithics (n = 117 and n = 45 respectively) fabricated on clastic materials from local river alluvium – a procurement strategy that would continue to dominate throughout the sequence (Goebel 2004). Layer 22 reportedly contained examples of the Levallois prepared core technique and Levallois flakes and points; side scrapers; denticulates and, apparently, a burin (Derev'anko and

Markin 1998, 92). These authors describe all levels at Denisova as containing Mousterian and Upper Palaeolithic components in varying proportions. The tools of European Upper Palaeolithic character, such as burins, end scrapers and blades, may appear throughout the sequence but rarely exceed 10% of the total lithic assemblage by layer in the lower layers, which are otherwise dominated by Mousterian elements. This relationship begins to change around Layer 13 [note: Layer 12 is dated to 101,000 ± 25,000 bp (RTL-612) by TL], where the difference is 30.7–69.3% Mousterian. Thereafter, Upper Palaeolithic elements at first balance the Mousterian and then, by Layer 9 [46,000 ± 2,300 bp (GX-17062) by ^{14}C], account for greater than 90% of tools – though "archaic" elements are still present (Derev'anko and Markin 1998, 93). There is also a shift in raw materials between the predominantly Mousterian and predominately Upper Palaeolithic phases of the cultural sequence. This is marked by an increase in the use of harder stone in the Upper Palaeolithic-dominated assemblages. Although the range of raw materials (dominated by local volcanic rocks, aleurolites and sandstones) remains the same, non-local jasperoids (sourced to 30–50 km away) used in the manufacture of a narrow selection of specialist elements such as micro-blades and *grattoirs* (Anoikin and Postnov 2005; Postnov et al. 2000) begin to appear. Here also, there is a clear linkage between tool type and raw material choice and broad preference for local materials, even though these were comparatively more difficult to flake.

Early bone technology appears part way through the sequence at Denisova. It is most notable from the Upper Palaeolithic-dominated assemblage in Layer 9. Although four pieces are reputed to come from Layer 11, most are likely to be intrusive from "pit" features dug from Layer 9. Even so, this assemblage includes finely worked and perforated bone work and pieces with linear patterns of inscription (Derev'anko and Markin 1998, 89–91) in a layer TL dated to 50,000 ± 12,000 bp (RTL-608) (Derev'anko et al. 2005). Layer 11, though, is dated by ^{14}C to 37,235 ± 1,000 bp (SOAN-2504) (Dolukhanov et al. 2002). While potentially consistent with the upper error range of the RTL-608 assay, this reversal indicates that the complexity of the relationship between these layers probably requires further attention.

The appearance of non-local lithic raw materials and complex bone technology could mark a shift in behaviour (and hence possibly the arrival of modern humans), but this seems to be set against gradual and well-documented in situ developments in the site's sequence of stone technology. If any significance is to be attributed to these changes, it probably should not, therefore, be judged directly against the European sequence or its associated behaviours.

As is the case in Central Asia, hominin remains from Siberia, particularly from before the Last Glacial Maximum (LGM), are sparse. Fragmentary evidence has been unearthed at the Denisova Cave in the form of a deciduous M_2 from the late Middle to early Upper Pleistocene: Layer 22.1 and an I^1 from Layer 12. The closest comparable remains are those from the Teshik-Tash individual, though the mixture of traits in the Deniscova specimen means that a positive identification to *H. neanderthalensis* cannot be ascribed with confidence (Shpakova and Derev'anko 2000).

Approximately 3 km to the SW of Denisova Cave, at the confluence of the Karakol and Anui rivers is the open-air site of Ust-Karakol. Excavated in 1986 and 1993–1997, it contains a series of cultural layers interspersed between archaeologically sterile lithological strata (Derev'anko and Markin 1998). The oldest cultural layer (19A) is dated by TL to the Middle–Upper Pleistocene transition – 133,000 ± 33,000 bp (RTL-661) (Derev'anko et al. 2005), but contains artefacts that are described as "poorly diagnostic" (Chlachula 2001, 150). Locally occurring volcanic rocks, aleurolites and sandstones dominate (91.5%) lithics, again, despite being comparatively hard to knap; a feature that, it has been suggested, may reflect short expected use-life (Derev'anko et al. 2005). Mousterian tool types are found in early Upper Pleistocene (TL-dated) layers and elements persist into the Late Pleistocene assemblages, which commence within cryoturbated loess 35,100 ± 2,850 bp (SOAN-3259). While these assemblages contain blades ($n = 142/657$ of lithic artefacts) and blade-derived types (e.g. burins and end scrapers), some tools (including end scrapers) appear also to have been made on flakes, while well-represented side scrapers are made on blades and flakes. The "amorphous and unstandardised" (Derev'anko and Markin 1998, 100) nature of the Upper Palaeolithic industry from Ust'Karakol has given rise to the interpretation that it represents a phase of technological and possibly typological transition.

The third site of significance from the Anui River valley is Okladnikov Cave, a multicomponent occupation c. 30 km north of Denisova Cave. Archaeological remains come from five of the seven excavated strata here. Dates (U-series) run from 43,300 ± 1,500 bp (RIDDL-722) in Layer 7 to 33,500 ± 700 bp (RIDDL-718) in Layer 1, with the greatest concentration of cultural material in Layers 2 and 3. Rodent activity has affected the upper three layers, though six dates from Layer 3 place it consistently between 28 and 43 kya (Derev'anko and Markin 1998; Derev'anko et al. 2005). Various forms of side scrapers dominate all layers, though Mousterian points and denticulates are also well represented. Within the total inventory ($n = 3,824$), there are also small numbers of end scrapers (from all layers), burins (only in Layer 2) and blades (Layers 1–3), though most blades ($n = 48/71$) are struck from Levallois cores. Levallois flake removals are present in all layers, with the exception of the small inventory ($n = 34$) from Layer 6. Overall, this assemblage is interpreted as the type Mousterian site for Siberia (Derev'anko et al. 2005).

Besides Denisova Cave, Okladnikov is also the only other Siberian site with pre-LGM hominin fossils. Five teeth and two skeletal fragments (an adult phalanx and a distal humerus) were obtained from Layer 2. Two further fragments – subadult distal portions of a femur and humerus – were recovered from Layer 3. The distal humerus is direct dated (^{14}C) to 29,900 ± 500 bp (Viola et al. 2006). Previously, nothing about the skeletal remains has suggested them to be anything other than *H. sapiens*, though limited knowledge about juvenile Neanderthal bone structure was raised as a caveat to this interpretation (Alekseev 1998). New mtDNA work on the subadult humerus suggests a strong likelihood of it being Neanderthal, though examination of the mtDNA from the adult bones does not (Krause et al. 2007). This extends the Neanderthal range a further 2,000 km east from where it was previously confirmed (Krause et al. 2007). However, it still leaves questions of technological agents and the unsatisfactory relegation of the industry at Okladnikov to "transitional" status unresolved (Shunkov 2005; Vasil'ev 2001).

The last site in this brief survey is another multicomponent, open-air site: Kara-Bom is situated in the middle range of Altai at c. 1,100 m, on a south-facing slope. This site is seen as crucial to tracking and understanding the Middle–Upper Palaeolithic transition in North and Central Asia (Derev'anko et al. 2000). The site contains 11 lithological layers (all of which contained cultural material), which the excavators combined into three main phases of deposition. The first phase is identified as Mousterian and dated to between 62.2 kya (ESR – no laboratory code given) and two infinite ^{14}C dates showing ages >42 kya (AA-8873A) and >44 kya (AA-8894A), all are from Layer 9. The early Upper Palaeolithic (EUP) phase (Layers 6 and 5) is ^{14}C dated to between 43,300 ± 1,600 bp (GX-17596) and 30,990 ± 460 bp (GX-17593), while the later Upper Palaeolithic (LUP) phase (Layers 4–1) has one ^{14}C date for L3 of 21,280 ± 450 bp (SOAN-300) (Derev'anko et al. 2005; Dolukhanov et al. 2002).

At the site, greater than 98% of all Mousterian and Upper Palaeolithic artefacts were made of the same high-quality black and dark grey chert, obtained from the Altair River, 1–2 km away, but a small amount of jasper does begin to appear in Layers 5 and 6 as an introduced exotic resource from almost 80 km away (Derev'anko et al. 2005). Implements made using Levallois core reduction (including Levallois points) are strongly featured in the Mousterian technological phase, though Levallois and sub-prismatic core-struck blades are also prominent (c. 15% of the assemblage) (Brantingham et al. 2001). Some of these were used to make notches and denticulate forms, while more classic Upper Palaeolithic *grattoirs*, burins, borers and backed knives were also made (Derev'anko et al. 2000).

The EUP phase is marked by a more than tenfold increase in the occurrence of sub-prismatic blades and a fourfold increase in bladelets compared to the Mousterian. Levallois blade occurrence remains at approximately the same level, though there is a marked decline in the number of Levallois flake cores ($n = 2$, compared to $n = 19$) (Brantingham et al. 2001). New tool elements appear during this phase, such as multifaceted burins and blade end scrapers. Derev'anko et al. (2000) characterise the main typological components of the tool kit as 38.9% Upper Palaeolithic, 28% notched and denticulate Mousterian, 9.8% Levallois-Mousterian. Also recovered from this phase are concentrations of goethite, a component used in the creation of pigment, and evidence of pigment applied to a pebble and three perforated pendants. One, a bone pendent, exhibits a notable imposition of form, reminiscent to that of the basket-shaped beads from the European Aurignacian sites in the Vallon de Castelmerle (White 1989), but employing a different material and manufacturing technique. The other two pendants (one on a bovid incisor and the other on a long-bone fragment) were both bi-conically drilled (Derev'anko and Rybin 2003).

The LUP phase exhibits a similar pattern: 32.3% Upper Palaeolithic, 35.1% notched and denticulate Mousterian and 6.7% Levallois-Mousterian; however, some further typological changes are noted. Levallois points, which had been getting progressively more elongate and resembling blades, now drop out of the assemblage altogether and proto-wedge-shaped cores (a major feature of post-LGM site inventories in Siberia) begin to appear.

Overall, the technological evidence from Kara-Bom is taken as a clear indication that Upper Palaeolithic tools arose out of the local Mousterian, within which incipient Upper Palaeolithic forms are visible and strongly influenced by developments in Levallois reductive flaking. Once again, Mousterian elements persist into these later assemblages as seen at Denisova and Ust-Karakol. Vasil'ev (2001, 72) has described this as an "enrichment" of the Mousterian with Upper Palaeolithic elements; a comment that also has resonance with Southeast and Central Asian technological development – introducing new components around a core of enduring and simply knapped pieces.

In the absence of good fossil evidence, population genetics could offer an avenue into studying the antiquity and distribution patterns of modern human presence in Siberia. Unfortunately, mtDNA from East Asian and Siberian populations still remains poorly understood, with the most detailed studies focused not on patterns in Asia but more on identifying source populations for the spread of humans into the Americas. Derenko et al. (2003) have identified that modern south Siberian populations show considerable mtDNA diversity and carry the genetic signature of an early phase of Asian colonisation by modern humans. The highest incidence of west Eurasian lineages appears in the Altai region, suggesting that a population from the west may have arrived here close to the beginning of the Late Pleistocene, but the majority of sequences studied (out of a sample of 480 individuals) belong to East Asian haplogroups, implying that most modern human migration into southern Siberia came from Central and/or East Asia and not the west.

The Pleistocene climatic and environmental changes experienced in the Altai Mountains have been linked to the ebb and flow of early hominin presence here. The early technology was comprised broadly of Mode-1 industries with occasional bifacial components. The appearance of various techno-facies ascribed collectively to the Mousterian (Mode-3) may represent a second colonisation of the Altai, though an emphasis on the exploitation of local raw materials, which featured in the early use of the region, continues with this more intensive occupation and does not change greatly through the Upper Pleistocene.

Even during the late Middle Pleistocene, small numbers of Upper Palaeolithic types appear within Mousterian-dominated lithic inventories. This is a feature that some argue does not appear in European Middle Palaeolithic industries (Anikovich 2007), though this is probably contestable. There is strong evidence in Siberia that what followed was a gradual transition and reversal of this relationship, with Upper Palaeolithic forms coming to dominate and, in some cases, demonstrably emerging out of local Levallois techniques, and with Mousterian elements persisting in small numbers. Some scholars do see evidence in the technology for population replacement comparable to that seen in Europe, but the continuity of Mousterian traits into Upper Palaeolithic industries and the scarcity of hominin remains prevent easy resolution of this important issue. Despite the climatic instability, which certainly would have affected human behaviour here, the technological record still might be said to have followed a characteristically *Asian* rather than an *Afro-European* path:

one marked, in large part, by continuity. That said, the transition to Upper Palaeolithic-dominated industries does fall at approximately the same time as it does in western Eurasia, and pieces of durable mobiliary art, recovered from nine EUP sites in Altai, also begin to appear only at c. 43–37 kya (Derev'anko and Rybin 2003), a coincidence that remains to be explained.

While the agents of the earliest pebble-tool industries are unknown, *H. sapiens* was almost certainly in the Altai region for an undisclosed time prior to the Late Pleistocene. The presence of *H. neanderthalensis* has now received positive support from mtDNA work (Krause et al. 2007), making concurrent occupation of this landscape by both species as increasingly plausible. Technological similarities between the Siberian Altai, Central Asia and Levantine sites (Brantingham et al. 2001; Derev'anko et al. 2000, 2005; Goebel 2004; Rybin 2005) may point towards at least one possible population scenario that might go some way to explain the in situ development of Upper Palaeolithic forms. The early occupation of the Levant by *H. sapiens* c. 130–100 kya (Grün et al. 2005) is often portrayed as abortive. These groups are thought either to have retreated into Africa under worsening environmental conditions c. 75 kya, or else become extinct, leaving the region devoid of modern humans until c. 45 kya (e.g. Marks 1990; Shea 2007). Perhaps, instead, the Levant's archaic population of *H. sapiens* took their Late Mousterian technology at this time and turned not west but east into continental Asia.

Conclusion

In this chapter, data from three parts of Asia have been examined to assess the character and trajectory of technological developments. The evidence reviewed still indicates that the area east of the Movius Line appears to have followed a different route of technological and material cultural development to that seen elsewhere in the Old World. Populations in Southeast Asia retained low input implements around which locally relevant, adaptive and innovative forms were added. However, these additions never became so dominant as to supplant Mode-1 technologies, which survived in some places until as late as the introduction of metallurgy (Shoocongdej 1996). In this part of Asia, the technology suggests localised variability against a seemingly deeply embedded strategy that may be said to have favoured a "knowledge heavy" and "tool-expedient" approach to daily life. The imposition of technological forms onto durable materials does appear, but proportionately it forms a less pronounced element of modern human behaviour in Southeast Asia than it would in the western Old World.

In Central and North Asia, the story also starts from a pebble-tool-based complex, but this is supplanted (replaced?) by techno-facies of the Mousterian. In the view of most commentators, these industries were *not* then supplanted by incoming Upper Palaeolithic ones but rather gave rise to them. Evidence of population immigration from the West is to be found here: indeed, the appearance of the Mousterian may mark one such spread, while later small dispersals of modern humans from

Europe is suggested by the genetic (Derenko et al. 2003) and possibly the archaeological evidence (compare material from the site of Mal'ta to evidence from Moravia and the Central Russian Plain). Their initial arrival, though, does not seem to have impacted on local industries or instigated the same rate of technological turnover as that seen in Late Pleistocene Europe. Widespread change does appear in Siberia, but it is later, after the LGM and possibly in the context of a major re-colonisation episode (e.g. Goebel 1999). The EUP industries that flourished briefly and died out in Europe persist in Central and North Asia. What would follow appears to have been a localised and gradual realignment of technology: developing and embellishing preexisting, but previously minor, components, and not only introducing new ones, but also retaining older, tried and tested, elements. If there are enduring questions about who was fabricating the EUP industries of Europe, these loom even larger in Central and North Asia.

In terms of linking hominins to technology, it remains unknown why anatomically modern humans in Europe devised a suite of Upper Palaeolithic technologies (and presumably associated cultural behaviours) that became divergent enough that their adoption or parallel development by contemporary Neanderthals did not occur. Possibly there was not enough time. In Asia, the trajectory of technological development appears to have retained sufficient common ground for as many as three different species of hominin to employ different techno-facies of regional industries successfully for tens if not hundreds of millennia.

The findings of this chapter indicate, above all else, that for Southeast, Central and North Asia, technological continuity and in situ development are key factors held in common. This is not readily reducible to single causes, such as the availability of raw materials. Whilst issues such as the effect of climatic and environmental variables have yet to be fully assessed, the observation of Movius and others that there existed a deep-seated cultural difference between the Afro-European and much of the Asian Palaeolithic appears to remain valid, even though it still awaits authoritative explanation. The idea presented by Okladnikov and Ranov that these observed differences represent different "lines of cultural development" with their own internal variability has considerable appeal. There have been various assertions for the presence of bifacial industries in East and Southeast Asia in the last 50 years. Though rather than offering exceptions that prove the Movius Line to be redundant, it appears, rather, to be a matter of proportions. In the western provinces of the Pleistocene Old World, bifacial technology became the ascendant technological mode, ultimately eclipsing (Foley and Lahr 1997; Kimura 2002) local pebble-tool-based industries. In much of Asia, this did not happen. Bifacial elements appear and may even characterise certain early Asian assemblages (e.g. Schick and Zhuan 1993), but the bigger picture is one where this manufacturing method did not dominate in the same way (Corvinus 2004; Simanjuntak and Forestier 2008). This difference would become highly significant in the form and direction that future developments in lithic technology would take in African, Mediterranean and northern South Asian contexts. The archaeological evidence from the rest of Asia suggests that we should not only be looking to the stages and markers of incremental change in human history – until recently, the "European story," but also to the mechanisms and behaviours that allow technological elements to remain embryonic and undeveloped, or that allow them to persist over thousands of years.

All the while the classic European Upper Palaeolithic sequence with its high turnover of discrete industries has continued to dominate interpretation and provide the template for research elsewhere, and it has been possible to perpetuate the idea that we are dealing with cultural entities comparable to our own. The Asian evidence challenges this directly. Eight decades of attempts to grapple with the nature of a widely distributed and temporally deep cultural phenomenon such as the "Hoabinhian" from Late to Post-Pleistocene Southeast Asia (e.g. Bartstra 1983; Reynolds 1990; Shoocongdej 1996) is testament to this. The collections of materialized culture that form the Palaeolithic record are the result of lifetimes of hominin interaction with each other and the environment, but they were almost certainly not cultures as we understand them. Nowhere is this more important than when it comes to attributing sociocultural patterns of behaviour to early *H. sapiens* and our assumption – prompted by the use of ethnographic parallels – that prehistoric people who looked like us will have acted like us. That a large swathe of the Asian Pleistocene Old World should have followed a pattern and pace of behavioural change different to that of the Afro-European and South Asian during the period when anatomically modern humans were key protagonists should prompt a rethink of this. It is possible that not all of the defining behaviours, which we equate with human modernity, may not have been in our species' possession before our ancestors left Africa. Instead, some may have emerged through the interaction between populations on different kinds of culture-adaptive trajectories. Many of these behaviours certainly appear at points within the African record (McBrearty and Brooks 2000), but one would suggest that there is a strong likelihood that the spread of Pleistocene humanity across the planet was instrumental in the process of creating the geologically fleeting "modernity" that we possess today and insist on looking for in our ancestors. While modern humanity almost certainly has its genetic origins in Africa, its behavioural origins may indeed prove to be more "multi-regional."

Acknowledgements I would like to thank Marc Vander Linden and Ben Roberts for inviting me to contribute to the *Cultural Transmission* volume. The ideas and content presented in this chapter form part of a wider investigation that I am undertaking into the role of regionalism in the emergence of modern human behaviour; funding for this is being provided by the John Templeton Foundation (Grant no. 1104/RG45799) and gratefully acknowledged. I am also very grateful for the helpful suggestions and comments of my colleagues during this chapter's preparation, most notably Rebecca Farbstein, Phil Piper, Liz Raddatz, Tim Reynolds, Mohd Mokhtar Saidin, Brian Stewart and Christopher Stimpson. Any outstanding mistakes or inaccuracies, though, remain entirely my own.

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Chapter 7
Ancient Technology and Archaeological Cultures: Understanding the Earliest Metallurgy in Eurasia

Benjamin W. Roberts

Introduction

The appearance of the earliest metal objects and metal production practices in Eurasia has traditionally been seen as an argument between single and multiple inventions, following either Wertime (1964, 1973) or Renfrew (1969). The recent dating of copper smelting at Belovode, Serbia to c. 5000 BC, making it the earliest currently known evidence in the world, has served to reinvigorate these debates (Radivojević et al. 2010). The continuing proliferation of archaeometallurgical analyses on material from radiocarbon dated sites from Ireland to Thailand probably ensures their continuation but it also provides far more data which can be used to construct new models and interpretations of early metallurgy throughout Eurasia (Roberts et al. 2009; Thornton et al. 2010). The trends in global early metallurgy away from concentrating upon "origins" debates and towards the identification of mechanisms underlying technological transmission; the processes of adoption and adaptation; cross-craftsmanship and cross-material connections; and the role of metal in social dynamics are certainly encouraging. These are accompanied by the widespread, and perhaps long overdue, recognition that the appearance of metal should not automatically be equated with the emergence of elites and neither should it be assumed to be a cause of social change or even an important material within communities (Thornton and Roberts 2009). These trends are a reflection of the wider development of archaeometallurgy into a discipline that seeks to understand past societies through their metallurgy by drawing upon scientific and archaeological/anthropological methods of inquiry (Thornton 2009a).

It would appear that the old culture-historical frameworks, together their associated explanations of migration and diffusion, have finally been supplanted and can be discarded. The obvious obstacle to this is that all past experience of theoretical

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B.W. Roberts and M. Vander Linden (eds.), *Investigating Archaeological Cultures: Material Culture, Variability, and Transmission*, DOI 10.1007/978-1-4419-6970-5_7,

development in archaeology indicates that archaeological cultures remain in use, at the very least as classificatory tools, regardless of their pronounced demise (see Chap. 2). The adoption of metal attracted the early attention of two of the most influential practitioners of New Archaeology – both Lewis Binford (e.g. Binford 1962) and Colin Renfrew (Renfrew 1967, 1969, 1973, 1986) published extensive critiques of archaeological cultures together with proposals of new systemic approaches. Yet, the recent publication of papers spanning global early metallurgy by theoretically informed scholars, including this author, all involved archaeological cultures, suggesting at least a delayed abandonment of the concept (see papers in *Journal of World Prehistory* 22, 3–4). This tenacity is perhaps not so surprising given that the justifiably maligned Three Age system, whereby the appearance of metal structures the history of vast expanses of the human experience (see Rowley-Conwy 2007), remains firmly entrenched in Eurasia. The debates surrounding whether several copper objects within an archaeological culture or region should necessitate the addition of Fourth Age, if this should be termed Chalcolithic, Eneolithic or Copper Age, and whether this represents broader societal changes, continue even to this day (e.g. Childe 1944; Lichardus and Echt 1991; Chernykh 1992, 10–16; Guilaine 2006; Roberts and Frieman forthcoming a).

The purpose of this chapter is to evaluate the role that archaeological cultures could play in understanding the earliest metallurgy in Eurasia by going beyond using archaeological cultures to provide either the identity of the people responsible for bringing metallurgy or the identity of the metal objects being discussed. It seeks to build on an earlier article reviewing the early development of metallurgy in Eurasia (Roberts et al. 2009). It summarises the earliest dates for metal objects and metal production practices contained within it before proposing models for metallurgical transmission and metallurgical adoption and then exploring these in relation to archaeological cultures. Analysing the transmission and adoption of the earliest metallurgy requires an approach which systematically addresses the metallurgical knowledge, skills and equipment that would be required to perform each identifiable transformation from ore to metal – encompassing the prospecting, extraction, processing, smelting and casting and comparing them to pre-existing technologies (see Ottaway 2001; Roberts 2008a; Ottaway and Roberts 2008). This structure facilitates a cross-craft and cross-material comparison, whereby the choices identified as shaping metal are not understood in isolation but are instead interpreted relative to research on other contemporary and associated materials (e.g. Shimada 2007; Roberts and Frieman forthcoming a and b) as well as to burial, settlement and subsistence practices, which together with craft production, underpin the definition of archaeological cultures.

Earliest Metal Objects and Metal Production in Eurasia

The exploitation of copper ores and naturally occurring copper in southwest Asia by early agricultural and agro-pastoral communities as at Rosh Horesha in Israel (Bar-Yosef Mayer and Porat 2008) to Shanidar Cave in northeast Iraq (Yener 2000)

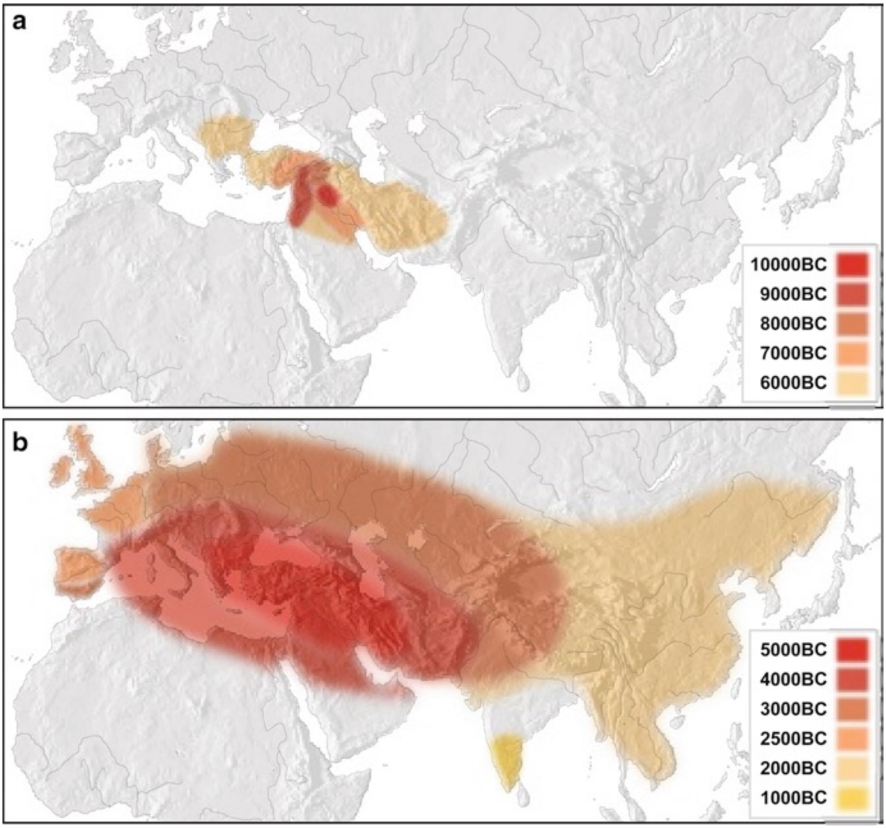

Fig. 7.1 (**a**) The spread of copper ore and native copper use in Eurasia. The exploitation of copper ores and naturally occurring copper metal in Eurasia. (**b**) The spread of copper smelting technology in Eurasia

from the 11th to 9th millennium BC represents the earliest evidence in Eurasia (Fig. 7.1a). It is within this region that the earliest annealing of copper metal is evidenced at Cayönü Tepesi, east Turkey at the end of the ninth millennium BC (Maddin et al. 1999) as well as the earliest probable smelted metal, the lead bracelet from Yarim Tepe, dating to the seventh millennium BC (Schoop 1999). The appearance of copper objects further afield occurs during the late eighth millennium BC as at Tell Ramad in southwestern Syria (Golden 2009) and Ali Kosh in southwestern Iran (Thornton 2009b). By the end of the seventh millennium BC, copper is exploited as far east as Mehrgarh in central Pakistan (Moulherat et al. 2002) and potentially as far west as Rudna Glava, Serbia (Borić 2009). On the current evidence, the appearance of copper beyond these two sites is related to the later appearance of copper smelting technology rather than the manipulation of ores or naturally occurring copper metal.

The earliest evidence for copper smelting is securely dated at Belovode, Serbia to the end of the sixth millennium BC (Radivojević et al. 2010) followed by the less securely dated Tal-i-Iblis, southeast Iran to the early fifth millennium BC (Frame and Lechtman forthcoming). In the absence of further archaeometallurgically analysed and securely dated sites between these sites, the debate of single vs. multiple centres of invention in Eurasia does not find a resolution (Fig. 7.1a). It has been argued elsewhere that the earlier metal exploitation and manipulation in northern Iraq and eastern Turkey implies a high probability that an earlier smelting site is found (Roberts et al. 2009). What is clear is that by the late fifth millennium BC, copper smelting can be identified in the southern Levant as at Abu Matar (Golden 2009, 2010) and probably in Central Europe at Brixlegg, Austria (Höppner et al. 2005; but see review in Kienlin 2010), where it would be broadly contemporary with the earliest metal objects in the region (Kienlin 2010). By the late fifth to early fourth millennium BC, copper objects containing lead and arsenic were being produced from Central Asia to southeast Europe (Chernykh 1992). The appearance of mainly copper objects dating the late fourth-early third millennium BC further east on the western and northern borders of modern China is followed by the local production of copper, arsenical copper and tin-bronze in northwest China at the beginning of the third millennium BC (Chernykh 1992; Linduff and Mei 2009). Copper and tin-bronze metallurgy in central China in the early-mid second millennium BC appears to be virtually contemporary with tin-bronze production in southeast Asia as at Ban Chiang in northeast Thailand (Pigott and Ciarla 2007; Higham and Higham 2009; White and Hamilton 2009; Pryce et al. 2011).

Copper axes and ornaments are found in northern Europe from the fourth millennium BC and, in the absence of any copper ores, would have represented the long distance movement of ores or, more probably, copper metal (Klassen 2000; 2004; Roberts and Frieman forthcoming a). In the central Mediterranean, copper objects are present in northern Italy from the early-mid fifth millennium BC (Skeates 1994; Pearce 2007, 48–52) and there is extensive copper ore extraction at Monte Loreto in northwest Italy from the mid fourth millennium cal BC (Maggi and Pearce 2005). Copper and silver production occurs on Sardinia during the late fifth to later fourth millennium BC (Lo Schiavo et al. 2005) – probably towards the end of this range given the recent radiocarbon dated sequence for early metallurgy in east-central Italy (Dolfini 2010).

In the west Mediterranean, the absence of secure contexts means that dating the earliest metal or metal production remains under debate. Copper smelting at Cerro Virtud, southeast Spain dating to the early-mid fifth millennium BC (Montero Ruiz et al. 1999; Ruiz Taboada and Montero 1999; Montero-Ruiz 2005) is potentially unreliable (see Roberts 2008a; 2009). The copper production activities at the third millennium BC sites of (Müller et al. 2007) Zambujal, Cabezo Juré (Nocete 2006), Almizaraque (Müller et al. 2004) and Los Millares (Montero Ruiz 1994) in southern Iberia remain the most comprehensively dated and analysed. In the absence of further evidence, a date of late fourth-early third millennium BC for the appearance of copper objects and production practices seems likely. This would parallel evidence in Mediterranean France, where copper and lead objects at Roquemengarde, southeast France (Guilaine 1991) slightly precede the copper mining at Les Neuf Bouches and copper smelting at the nearby La Capitelle du Broum which date to the end of the fourth millennium BC (Ambert et al. 2005; Mille and Carozza 2009). In northwest Europe, the earliest securely dated copper objects date to the mid fourth millennium BC at Vignely, northern France (Mille and Bouquet 2004). However, it is not until the mid third millennium BC that the earliest copper and gold objects are found on islands off the western Eurasian landmass as at Amesbury, southern England (Fitzpatrick 2002, 2009) and earliest copper mining as at Ross Island, southwest Ireland (O'Brien 2004). However, this is still over two millennia before the earliest metal (iron rather than copper alloy) objects in Japan, which represents the equivalent islands off the eastern Eurasian landmass (Mizoguchi 2002).

Modelling Metallurgical Transmission

The dating of the earliest metal objects and metal production across the Eurasian landmass indicates a punctuated transmission from southwest Asia that reaches the western and eastern extremities around eight millennia later, and even longer if its offshore islands are included. The temporal and spatial rate at which this transmission process occurred appears to have been highly variable, especially when the models of rapid metallurgical adoption in a few centuries across East Asia (e.g. Pigott and Ciarla 2007; White and Hamilton 2009; Higham and Higham 2009; Pryce et al. 2011) are compared with their equivalents in Europe which unfold over three millennia (e.g. Krause 2003; Roberts 2008a; Ottaway and Roberts 2008; Strahm and Hauptmann 2009; Kienlin 2010). The patterns revealed by these earliest dates provide the temporal and spatial framework within which models of metallurgical transmission can be proposed. However, the dates do not provide any suggestions of underlying mechanisms until they are integrated with evaluation of the expertise required for each stage of metal production against the relevant pre-existing expertise. Older ideas based on the determining role played by geology in the spread and development of metallurgy might seem initially appealing (e.g. Charles 1980, 1985), especially given the large expanses without copper ores. However, the distances between ore sources do not easily correlate with the duration of metallurgical transmission as evidenced by the relatively rapid transmission of copper objects across the northern European Plain to Scandinavia (Klassen 2000, 2004) or the relatively slow transmission of copper production from the copper ore sources in eastern Serbia across the Adriatic sea to copper sources in northern Italy (Ottaway and Roberts 2008; Radivojević et al. 2010). Nonetheless, the geological hypothesis has yet to be systematically tested.

The earliest metal encountered in the geographical regions spanning the Eurasian landmass is copper or a copper alloy, such as arsenical copper or tin-bronze. Although the earliest copper can be accompanied by other metals, such as lead as in southern France (Guilaine 1991), silver as in Sardinia (Lo Schiavo et al. 2005) or gold as in Britain (Fitzpatrick 2009), the evidence for the production tends to be far more fragmentary. The prospecting for copper ores throughout Eurasia could have been relatively straightforward for the experienced practitioner, especially given their abundance. However, there were plenty of other similarly coloured mineral sources that could be a source of confusion. The early selection of copper ores among other aesthetically comparable green stones in the Levant (Bar Yosef-Mayer and Porat 2008) demonstrates that this was a real possibility which could have proved problematic to later communities wishing to smelt copper ores to create metal. The extraction of copper ores from the surface would have sufficed for the earliest metallurgy. If ore veins were followed underground as at early copper mining sites, such as Rudna Glava, eastern Serbia (Borić 2009), expertise would have been required to facilitate the movement of miners, their equipment and the ore, and to provide them with adequate ventilation, illumination and drainage, all while ensuring that the underground structures did not collapse. Organisation was necessary to source, make and transport the mining tools and equipment, such as stone hammers and antler picks (e.g. Pascale De 2003) the large quantities of fuel for fire-setting (cf. Weisgerber and Willies 2001), and food for the miners. Whether close to the settlements or not, the implication is that there would have to have been dedicated mining expeditions containing several individuals with relevant expertise that had access to the ore. However, it is probable that the expertise required for copper mining could have been gained through a continuation of existing traditions of flint and stone mining (e.g. Korlin and Weisgerber 2006).

The earliest transformation of copper ore to copper metal throughout Eurasia not only involved the ability to obtain and prepare through beneficiation of the correct raw material, but also the construction of specialised ceramics for crucibles and moulds, the use of a fuel, such as charcoal, and the ability to control air flow (Bourgarit 2007). By modern standards, the earliest copper smelting in Eurasia can be characterised as relatively simple – small scale, relatively low temperature processes carried out under poorly reducing conditions on oxidic and/or sulphidic ores in small stone and clay structures and/or ceramic crucibles with no intentional addition of fluxes and little consequent slag (see Bourgarit 2007; Hauptmann 2007 Roberts et al. 2009;). The smelting would have yielded only small quantities of copper that would then have to be refined in a separate process. Yet, it would have been the smelting of the copper ore that potentially provided the greatest challenge to a metallurgical novice, especially in a different environment with new fuels or a wetter climate. Pottery precedes metal throughout in many regions of Eurasia, and it has traditionally been argued that the pyrotechnological demands involved in the making of the former would lead to the making of the latter. The presence or role of charcoal before metal production is hard to establish, as neither the surviving evidence nor the necessity can be found. However, charcoal would have been of fundamental importance in smelting not simply due to its ability to create high temperatures using relatively small quantities in a small space, but as a source of highly reducing carbon monoxide gas (see Horne 1982; Craddock 2001). In replicating the bonfire firings involved in the production of pottery, it is evident that there is a relative lack of control, rapid changes in temperature, an oxidising atmosphere and a variable duration varying from several minutes to several hours. Although temperatures of c. 1,000°C can be reached, this is only for a very short duration and cannot be maintained before dropping back to c. 600–800°C or lower (e.g. Gosselain 1992; Livingstone-Smith 2001; McDonnell 2001). It is possible that more pre-metallurgical control could have been achieved, as shown by the analysis of Neolithic red ochre decorated pottery from southeast Spain (Capel et al. 2006), but this would not be sufficient in terms of temperature, atmosphere or control to smelt oxidic and/or sulphidic ores according to experimental reconstructions (e.g. Rovira and Guttierez 2005; Timberlake 2005, 2007; Bourgarit 2007).

These differences suggest that the ability to smelt copper would have required verbal instructions and visual demonstrations from an experienced individual or community if the expertise was to be transmitted. As smelting experiments have shown, even "simple" smelting technology needed to be carried out within a fairly narrow margin of error or else the entire process would fail. It seems very probable that at least some aspects of the metal production process would either have been inevitably or deliberately restricted to certain individuals or groups. The transmission of copper production expertise throughout Eurasia would therefore have to involve the movement of a sufficiently skilled individual or a group to a new ore source. This introduces two interrelated mechanisms for metallurgical transmission – the movement of metalworker(s) and the learning of metallurgy from experienced metalworkers. This would have created an extensive yet fragile network of metallurgical expertise, potentially over substantial distances.

Analysing the movements of individual metalsmiths is only going to be feasible in exceptional circumstances, such as the "Amesbury Archer", where oxygen isotope analysis indicates that he may well have spent his formative years in an Alpine environment before making the journey to southern England, where he was buried with copper and gold objects and a cushion stone during the mid third millennium BC (Fitzpatrick 2002, 2009; Evans et al. 2006). Tracing the movements of metalworking communities is also potentially possible, especially for the earliest evidence of a new and distinctive technology in a region. This has been proposed by White and Hamilton (2009) who have argued that the earliest metallurgy in southeast Asia is due to the rapid migration of southern Siberian metalworkers, or at least metalworkers trained by them, as part of the "Seima-Turbino trans-cultural phenomenon" (but see Pryce et al. 2011 for a review). They surmise that the evidence in southeast Asia is similar enough to make it one of the regions encompassed in the rapid creation of relatively uniform metallurgical traditions across the Eurasian steppes during the late third millennium BC. This is thought to have been the consequence of the aggressive movement of tribes bringing their distinctive "Seima-Turbino" metallurgy, including binary tin-bronzes and casting in blind sockets, with them (cf. Chernykh 1992, 2008; Sherratt 2006; Kohl 2007; Hanks et al. 2007; Anthony 2007). This movement of metal technology by archaeologically identifiable mobile communities has also been identified between the southern Levant and northeast Africa by Anfinset (2010) who demonstrates that this phenomenon represented the transmission of only one of many locally unobtainable materials and commodities across the Sinai desert.

Modelling Metallurgical Adoption

In order to model metallurgical adoption, the assumption of inevitability must first be discarded. There was no inherent functional reason why metal objects or metal production should be adopted by local communities or introduced by non-local communities. Early metal tools did not provide an advantage over existing materials in performing everyday tasks – they were less effective than stone, bone or flint counterparts (e.g. Mathieu and Mayer 1997), and may not even have been hardened or used. The distinctive colours, lustre and malleability can be proposed as attractive qualities. The ability to recycle meant that object forms created elsewhere could be melted down and converted into more familiar shapes, even in regions far from ore deposits or primary production centres. But none of these imply that the appearance of metal objects and production practices throughout Eurasia was a foregone conclusion.

It could be assumed that in using the same underlying technology, and one which would have to have been learnt elsewhere, the earliest metalworking communities throughout Eurasia would make and use similar objects. The application of a vast programme of compositional analyses on copper and tin-bronze objects across the former Soviet Union by E.N. Chernykh (1992, 2008) enabled broad-scale similarities and variations in metal composition and technology to be evaluated. The desire to look at the underlying patterns evidenced in the metal rather than grouping the results by individual archaeological cultures led Chernykh (1992, 7–10) to develop a hierarchical and dependent system of regionally distinct metallurgical "provinces" whose dynamism depends on the characteristics of the primary or secondary metal production centres or "focuses", associated with one or more archaeological cultures, and frequently grouped into early metal "zones". The approach facilitates the identification of observed metallurgical patterns at a vast spatial scale, such as the Carpatho-Balkan metallurgical province during the mid fifth-early fourth millennium BC stretching across eastern Europe and deep into the Eurasian steppes, substantially beyond the scope vast majority of archaeological cultures. It is hard to underestimate the influence of this pioneering model for understanding early metallurgy in the Eurasian steppes, yet it is undermined by fundamental flaws. In identifying "foci", "zones" and "provinces" within a hierarchical framework, the model presupposes and advocates relationships based on metal-consuming peripheries being dependent on metal producing cores. The uneven distribution of copper and tin ores throughout the Eurasian steppes does mean that connections have to be established and maintained to ensure a regular supply of metal ores or objects, but it does not immediately equate to a binding relationship of dependency and inequality. Perceptions of the consumption of the metal objects being made, whether of quantity, type or composition, are inevitably highly influenced by past practices of deposition or discard, as well as recycling or re-melting (e.g. Taylor 1999). It also does not address regions, such as Western Europe, where there is a mosaic of frequently diverse metallurgical traditions within and beyond archaeological cultures distinguished by form, composition and metalworking techniques (Roberts 2008a). Fundamentally, it does not provide a model that can *explain* the earliest adoption of metal.

The problem lies in modelling metal adoption overwhelmingly from the perspective of metal producers rather than the metal consumers. Given that archaeometallurgical techniques enable far more data to be gathered from metal objects and workshops alike regarding the methods and organisation of production rather than the motivations of the metal-using communities, this is perhaps not so surprising. This bias is supported by the many myths and legends of involving metalsmiths and by early anthropological and modern ethno-archaeological accounts of the magic and taboos surrounding them (e.g. Budd and Taylor 1995; Blakely 2006). Early scholars felt that as metal represented an advanced technology, metal had to have been brought in by advanced colonisers, generally in search of new ore sources, in a manner not entirely dissimilar to contemporary colonial powers (see Roberts 2008b). This led to the long-held notion of metalsmiths whose technical expertise in creating a revolutionary new material provided them with a special status and whose movement led to the transmission of new ideas and practices (Roberts 2008b). This interpretation was articulated most influentially by V. Gordon Childe (1930), who made itinerant metalsmiths primary agents of social change in European societies, due to their mobility and perceived lack of tribal affiliations (see Rowlands 1971; Wailes 1996).

Yet, it is argued that the communities who supported the acquisition of metallurgical skills, assisted with the collective aspects of metal production (e.g. ore prospection, extraction and processing), and circulated and used the metal objects, were more influential in shaping early metallurgy than the metalsmiths. The small scale of the earliest visible metal production and consumption is more indicative of an occasional rather than continuous production process, with a relatively low level of circulation undertaken by part-time, rather than full-time smiths. It can be argued that metal production even ceased subsequent to its introduction and circulation as in northern Europe from c. 3200 to 3000 BC and northwest continental Europe from c. 3000 to 2500 BC (cf. Krause 2003, 34).

Once metal production is removed from its scholarly pedestal and copper and gold objects stop being ascribed high, yet frequently unspecified, values for prehistoric communities, it is possible to see the earliest metal in light of other materials. For instance, the burial of an individual in the Beaker culture burial rite in northwest Europe during the mid third millennium BC such as the 'Amesbury Archer' involved a thin-walled, elaborately decorated pottery vessel potentially together with polished stone bracers, finely made flint arrowheads, v-perforated buttons, possibly in amber or jet, daggers in flint or copper and earrings in gold or copper. The ability to acquire these materials or craft the desired objects required similar processes of gaining specific knowledge and skills – none of which can easily be used to elevate metal in the overall interpretation. Instead, all the materials are made to reflect a desired standard and are not rigorously demarcated (Roberts and Frieman forthcoming a). The traditional idea of Beaker culture representing an especially dynamic community with regards to metal is demolished further when it is explored in other regions. In southeast France, where copper metallurgy was established prior to the change in the inhumation practices to the Beaker burial rite actually led to fewer metal objects in more restricted range (Ambert 2001; Vander Linden 2006). Similarly, the extensive archaeometallurgical analysis of early metal objects throughout Spain failed to reveal a Beaker metallurgy as distinct from the pre-existing metallurgical traditions (Rovira and Delibes de Castro 2005). In modelling early metallurgical adoption, archaeological cultures can therefore provide a framework for exploring metal within its broader material context but not a secure framework for defining the adoption process.

Conclusion

The earliest metal objects and metallurgy in Eurasia have traditionally been classified according to the associated archaeological culture – a practice that doubtless will continue. The problematic variations in the definition of the relevant archaeological cultures, whether for example the Vinča culture in the Balkans through tell settlements and pottery types or the Beaker culture in central and western Europe through burials, pottery types and ornamental craftsmanship, will remain. However, it is argued that by analysing the earliest metal in the absence of a critical evaluation of archaeological cultures means that valuable insights into the modes of transmission and adoption could be missed.

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Chapter 8
"Culture", Innovation and Interaction Across Southern Iran from the Neolithic to the Bronze Age (c. 6500–3000 BC)

Cameron A. Petrie

Introduction

Throughout prehistory, southern Iran was a key route for interaction, communication and the dispersal of innovation from west to east and east to west (e.g. Lamberg-Karlovsky 1978, 1989; Renfrew 1996, Fig. 5.2; Sherratt 1997, Fig. 0.2; Weeks et al. 2006b, 24). This extensive region saw profound socio-economic and political transformations between 6500 and 3000 BC as the earliest village-based societies progressively became more complex, and this culminated in the rise of the first cities in Iran. Among other things, these transformations are marked by a series of innovations in ceramic production technology, including approaches to vessel forming, decoration and firing. Although constituting just one aspect of a larger cultural milieu, the major developments in ceramic technology and style are widely used to delineate individual chronological periods throughout southern Iran, and these in turn typically demarcate phases of culture change.

The approaches used to produce the earliest pottery in the ceramic Neolithic period (c. 6500–5000 BC) of southern Iran were remarkably similar across a geographical area that stretched from the lowlands of Khuzestan in the west, through the various intermontane valleys of Fars, to the plains of Kerman to the east, and beyond (Fig. 8.1). These vessels were hand formed most probably using a version of sequential slab construction (SSC), and Vandiver (1995) has shown that this technology was actually used across a larger area stretching from the Central Western Zagros to Pakistani Baluchistan (also Petrie et al. 2010). The surface was then covered with a layer of finer clay, slipped, and/or decorated before being fired at a relatively low temperature. During the Chalcolithic period (c. 5000–3000 BC), there were innovations in production technology that saw a similarly widespread distribution, including shifts in the types of raw materials used and marked increases in the firing

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B.W. Roberts and M. Vander Linden (eds.), *Investigating Archaeological Cultures: Material Culture, Variability, and Transmission*, DOI 10.1007/978-1-4419-6970-5_8,

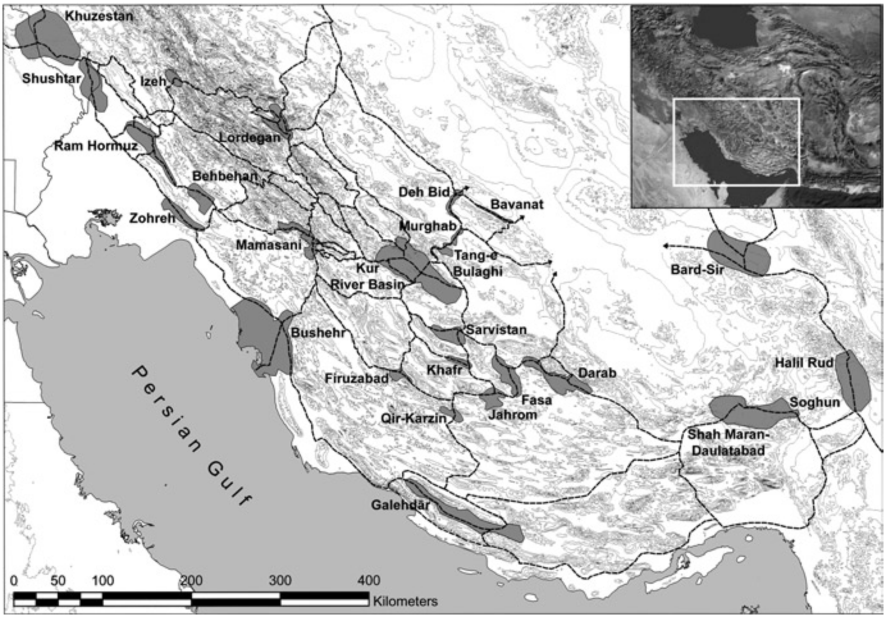

Fig. 8.1 Map showing the lowland and intermontane plains of southern Iran and the routes connecting them. Inset map shows the location of this region relative to the Iranian plateau

temperatures that were achieved. In time, the use of rotation in the forming process becomes more marked, and there was ultimately a shift to the use of fast wheels and the mass production of certain forms. Although these technologies were utilised contemporaneously in different regions over protracted periods, the ceramic vessels that were being made in each region during each phase typically display distinctive and idiosyncratic decorative motifs and painting styles.

This paper looks at the pace and nature of change in prehistoric ceramic technology and decorative style as a way of exploring the processes of culture change, innovation and transmission. The archaeological record of the regions that comprise southern Iran appears to be characterised by repeated cycles of technological innovation and conservatism in ceramic production that occur across a landscape marked by regionally distinct approaches to style. In the early mid-twentieth century, archaeologists working in Iran assumed that the concordance of distinct decorative styles and specific geographic areas indicated the existence of regionally distinct archaeological "cultures". More recent approaches avoid such explicit associations, but many of the prevailing interpretations of culture change in southern Iran verge on adopting a culture-historical standpoint, and processes, such as migration, are often put forward as an explanation for change (e.g. Alden 1982; Alizadeh 1992, 2006, 2008). In fact, studies looking at territories, boundaries and cultures during the Neolithic period throughout the Near East are seeing a resurgence (e.g. Koslowski and Aurenche 2005). Although it is not always explicitly articulated, the concept of the archaeological culture still holds currency, and throughout prehistory the material culture used in the different regions of southern Iran is distinctive (Voigt and Dyson 1992). It is likely that the geography of southern Iran has contributed to the formation of socio-economic and cultural boundaries and frontiers between the populations that occupied different regions (after Wolf 1982; Lightfoot and Martinez 1995). Nevertheless, by focussing on the stylistic differences between regions in spite of the similarities in the technological choices made by potters during the production process, archaeologists run the risk of overlooking important dynamics. Although it is quite clear that technology and style are interrelated (e.g. Lechtman 1977; Conkey and Hastorf 1990), in assessing cultural change, technology and style need not be given equal weight. The evidence for the pace of change in each in southern Iran suggests that connected yet distinct processes governed the dispersal and/or transfer of innovation in production technology (forming), and the creation of regionally distinct innovations in decorative style and surface finish (post-forming).

The scale and importance of particular innovations and the pace at which they were adopted are critical factors for understanding the dynamics of culture change across the varied cultural and physical geography of Southern Iran. Caldwell (1968, 183) suggested that the prehistory of southern Iran was marked by periods of nuclear influence followed by periods of regionalism. Using an explicitly biological analogy, Beale and Lamberg-Karlovsky (1986, 264) subsequently suggested that the pace of change throughout the prehistoric sequence at the site of Tepe Yahya might better be understood by reference to the concept of “punctuated” evolution. This paper shows that a more nuanced variant of these concepts can be applied to all of southern Iran – a complex pattern where major technological innovations in ceramic production dispersed rapidly over short to medium distances, followed by protracted periods where regionally distinct stylistic elaboration appears to have operated across both time and space. The complex and variable relationships between people, material culture, technology, style and landscapes have the potential to tell us much about human behaviour and culture change.

Culture Change and Innovation

Explaining how and why cultural change occurs are perennial challenges for archaeologists, and the role that innovation plays in this process is not always self evident. In the introduction to the volume *What’s New? A Closer Look at the Process of Innovation*, Torrence and Van der Leeuw (1989, 1; also McGlade and McGlade 1989, 282) emphasised that although the reasons how and why change occurs are fundamental aspects of human behaviour, these factors are often neglected, particularly the reasons why new behaviours are not accepted and change does not occur. In the same volume, Shennan (1989, 1996) noted that both processual and post-processual theoretical approaches have a tendency towards synchronic reconstruction, and as a result they have devoted little attention to questions of cultural transmission. He argued that by doing this, they ultimately run the risk of “failing to get as far as the questions of long-term change which are supposed to be archaeology's privileged domain" (Shennan 1989, 1996, 283). Although we have seen increasing sophistication in approaches to archaeological thought in the intervening period, archaeologists still frequently neglect to explain how and why change takes place, particularly in relation to large-scale processes. Wolf (1982) has argued that cultural evolution operates on interconnected systems where societies are linked to each other by what he referred to as "social fields". This is but one model that has been used to describe interconnections in prehistory, and joins concepts like "interaction spheres", "peer polity interaction" and "world systems" (see Kohl 2008, 496; Lamberg-Karlovsky 2009).

Philosophers, social theorists and archaeologists have long debated the nature and transmission of innovation (e.g. Plato, see D'Angour 2000; Aristotle [Politics Book V], see Pappin 2009; Marx 1867 [2004]); Durkheim 1912 [1995]; Childe 1937, 1942). Innovation is undoubtedly a complex topic that continues to be a subject of interest in a range of disciplines, including sociology, economics, business, design and technology. Innovation can be seen as both an event and a process. It is often difficult to identify the actual event due to the nature of the archaeological record, but we can see the evidence of its having taken place. Torrence and van der Leeuw (1989, 7) have pointed out that most case studies of change that deal with the role of innovation focus on either detailed studies of change, or make general observations based on comparative studies, and "the conceptual framework, the techniques, and the knowledge needed to relate these two levels to one another seem to be lacking". Before the New Archaeology, culture change was typically seen as a result of diffusion, which was envisaged as either the replacement of populations (demic diffusion), or the spread of influences from outside (cultural diffusion) – the latter being a process incorporating elements of innovation, imitation and diffusion to varying degrees (Shennan 1989, 1996, 282). For example, Childe (1937) viewed diffusion as an essential component of both technological innovation and social evolution. Simplistic diffusion-based explanations have fallen from favour, but as Sherratt (1993, 2) has pointed out, "the death of diffusion as a respectable explanation has left something of a vacuum in conceptualising … larger structures". The danger in attempting to characterise larger structures and processes is the tendency towards reductionism, where overly simplified explanations are put forward to explain nuanced dynamics and situations. More recent investigations of large-scale processes continue to subsume the concept of diffusion in incorporating the terminology of "spread", "expansion", "influence" and "dependence"/"independence" as a means of explaining culture change (e.g. Sherratt 1993, 1997, 2004, 2007). The key to rationalising these diffusionist tendencies when formulating broader models of cultural transmission has been the integration of explanations that take account of local conditions and contexts (e.g. Sherratt 1997, Shennan 1989, 1996; McGlade and McGlade 1989, 282), and also the role of individual agency, action and choice in the social process (e.g. Layton 1973, 1989; Lamberg-Karlovsky 2009).

Innovation comprises two key elements: invention or the original conception of a new idea, and adoption, or the actions involved in the acceptance and use of the invention (Torrence and van der Leeuw 1989, 3). Examples of major technological and social innovations in the archaeological record are legion, and the tendency for modern archaeologists to define a link between technological innovation and social change dates back to Thomsen (1837). However, although individual inventions might have played important roles in socio-economic development (e.g. Layton 1973, 1989), there is often no direct correlation between innovation and social change (e.g. Sørenson 1989). What is often lacking is a discussion of why innovations happen in the first place. In many ways, the key to understanding culture change and the role of innovation lies in our understanding of the process of transmission and transfer. Culture is a highly mutable concept, and archaeologists have typically envisaged it as being set of shared ideas, beliefs, attitudes, values, practices, and perhaps most visibly material things, which characterise a particular group and become distinct traditions as they are passed down through time – in essence it is something learned (e.g. Clarke 1968, 666; Torrence and van der Leeuw 1989, 5; DeMarrais 2004, 12; after Goodenough 2003, 6–7). Wolf (1984) has noted that culture is a useful starting point of inquiry. An evolutionary approach to culture suggests that individuals acquire patterns of behaviour from their parents (both "literal" and "cultural") and then intentionally/unintentionally or rationally/irrationally modify them in the light of their own experience, before passing them on to their own offspring (again, both "literal" and "cultural") (Boyd and Richerson 1985, Shennan 1989, 1996, 286; Lyman and O'Brien 2001). Although couched in a Darwinian framework, there are some similarities between this definition of transmission and the operation of Bourdieu's conception of *habitus*, which can be defined as a system of durable and transposable predispositions that develop in response to structures (class, family, education), and the external conditions that an individual encounters (Bourdieu 1977; Bourdieu and Wacquant 1992; also Dobres 2000, 136ff.).

Innovation is an inherently complex phenomenon whose comprehension requires attention to both the small-scale processes relating to the incident of invention and its adoption and spread at a local scale, and also large-scale processes where entire populations are engaged (Shennan 1989, 1996, 289). Boyd and Richerson (1985; also Shennan 1989, 1996, 289) have suggested that once a cultural system has developed, there is a tendency not to innovate, and when innovations do occur, they are often resisted. They thus see cultural traditions operating as inheritance systems in which continuity is the norm, and modifications are combinations of accident, individual choice and cultural selection (Boyd and Richerson 1985, 291; also Shennan 1989, 1996, 289–291). Social structure can thus be seen to both facilitate and impede innovation (McGlade and McGlade 1989, 282), and when innovation occurs, it is at some level a conscious decision that was presumably made in response to a particular situation where people are either prepared or required to suspend their usual routines (Shennan 1989, 1996, 289). Scale is an important element, as the significance of an innovation is variable, and can vary between minor fashion changes to major technological developments (Shennan 1989, 1996, 289), which are not necessarily interdependent. The rates of both invention and adoption are also variable, and responsive to social, economic and environmental conditions (Shennan 1989, 1996, 289).

There have been various attempts to model how, why and at what rate new ideas and technology spread. Ideally, the adoption of innovation is progressive and conforms to a regular pattern whereby there is a group of pioneering adopters, who are followed by the majority, and are trailed by a group of laggards (McGlade and McGlade 1989, 283–284; citing Rogers 1962; Rogers and Shoemaker 1971). Such models are, however, inherently determinative and lack a mechanism for taking account of variation in human behaviour, particularly factors, such as the resistance to innovation, and the mutation/modification of innovation, that might occur during the processes of transmission and transfer. An alternative model proposed by McGlade and McGlade (1989, 288ff.) proposes that innovation is a dynamic evolutionary force and its transmission is dictated by constraining and facilitating processes, such as diffusion, perception, attractiveness, resistance, adaptation and adoption. The relative importance of each individual process is dependent upon local contexts and factors. Variants on this theme have also been proposed by Allen (1989) and van der Leeuw (1989).

For archaeologists, understanding the relationship between innovation and material culture is crucial, as the latter is usually the most abundant category of evidence encountered. Fortunately, there are various ways in which material culture is approached that help to provide a means of understanding the socio-economic significance of innovation. It is now widely acknowledged that material culture plays an active role in the production and maintenance of cultural values and the operation of processes of social interaction at various scales (e.g. Bourdieu 1977; Hodder 1982, 1986; also Appadurai 1986; Miller 1987). The investigation of how the crafts that produce material culture are organised, particularly the role of the crafts person, provides a means of understanding developments in technology, economy and society (see Dobres and Hoffman 1999; Costin 2001, 273ff). Also, the technologies and the technological choices that are made in the production of material culture are increasingly seen as being embedded within their cultural milieu, i.e. the choices of the potter are dependent on both functional criteria and contextual factors, such as the social, economic, ideological and environmental setting, that influence the agency and ideology of the individuals producing the material and the *chaîne opératoire* that they utilise (e.g. Ingold 1990; Lemmonier 1993; Dobres and Hoffman 1999; also Leroi-Gourhan 1964, 1965; Cresswell 1972; Tite 1999; Sillar and Tite 2000; Roux 2003). The interrelationship of technology and culture is further emphasised by ethnographic analyses, which have shown that social boundaries and identities can both be materialised through the execution of particular technical behaviours that are heterogeneous and dynamic (e.g. Gosselain 1999, 2000, Chap. 11; Stark et al. 2000). These varying approaches each provide a means of understanding the social context of innovation and although many are typically used at a small scale, their inherent principles must be considered when trying to account for large-scale dynamics.

Change in material culture is a critical factor, whether or not it is a result of innovation, and the desire to understand how and why change occur remain issues deserving attention. The prehistoric archaeology of Southern Iran provides a specific opportunity to take a "bottom up" approach in outlining the chronological and spatial dimensions of broad-scale culture change, and to then hypothesise about the role of innovation and transmission in these processes.

The Context and Archaeology of the Lowlands and Highlands of Southern Iran

The ranges of the Zagros Mountains are the dominant geographical feature of southern Iran and demarcate several distinctive geographical zones and regions (Fig. 8.1). In the west, the lowland plains of Khuzestan and its neighbours are linked to the alluvial plains of southern Mesopotamia. The areas of Fars and Kerman to the east of this are characterised by the intermontane valleys and plains of the highlands. Intensive sedentary settlement is limited to the plains and valleys that have both adequate water resources and sufficient areas of arable land (Carter 1994, 75; de Miroschedji 2003; Roustaei et al. 2006; Askari Chaverdi et al. 2008), and with relatively few exceptions these valleys and plains are scattered throughout southern Iran and are not continuous. They are, however, connected to each other by paths, tracks, roads and passes of various lengths, which traverse the Zagros and form a network of routes that link what are often far flung regions (Fig. 8.1). This distinctive topography imposes specific constraints on human behaviour, particularly on the communication and interaction between the populations that live in the different valley systems, and the ancient inhabitants needed to be able to adapt their behaviour and subsistence practices to suit this variable landscape. In essence, this is a varied landscape that fosters the creation of socio-economic and cultural boundaries and frontiers; it is an environment where in the words of Triandis and Suh (2002) "ecologies shape cultures". Some of the valleys and plains of southern Iran have been the focus of coordinated archaeological research while others have seen little or no archaeological exploration. As a result, our knowledge of the prehistoric occupation of southern Iran is patchy and not consistently resolved. Nevertheless, broad patterns in the distribution of material culture and technological practices can be delineated, and it is possible to use those patterns to discuss the socio-economic context and the types of interaction that occurred between populations in different regions. For example, the archaeological evidence for the distribution of raw materials from specific locations makes it clear that there were connections between these regions from the earliest Neolithic.

There have been several major phases of archaeological research in southern Iran, beginning in the late nineteenth century and continuing up to the present. This research has incorporated excavation at sites of various sizes and types, extensive surveys of major routes and plains and full coverage surveys of specific areas (summarised in Voigt and Dyson 1992). At the culmination of the early phase of exploration and excavation, archaeologists typically referred to "cultures", which were defined on the basis of the discovery of distinctive types of material culture in excavations at a limited number of type-sites that were often separated by considerable distances and highly fractured landscapes. The prevailing approach to interpretation in the mid-twentieth century thus led to the delineation of cultural historical sequences that incorporated type-site names for individual periods, and typically used ceramic material as the key indice (e.g. McCown 1942; Vanden Berghe 1952, 1954). It is notable that these names continue to be employed by most scholars, primarily to differentiate chronological periods marked by distinctive changes in material culture, but also to refer to regionally distinct assemblages of material culture (e.g. Sumner 1972; Voigt and Dyson 1992; Alizadeh 2006; Potts et al. 2006; Weeks et al. 2010). The 1960s and 1970s saw a dramatic proliferation of archaeological research in most parts of Iran, particularly in the southwest and southeast. As a result of this major phase of work, several attempts were made to synthesise the archaeological evidence from these areas, particularly in terms of trying to establish the nature of cultural influence and communication between the populations living in different parts of Iran and also the surrounding regions (e.g. Caldwell 1968; Lamberg-Karlovsky 1978; Amiet 1979; Alden 1982). Much of this fieldwork halted with the Iranian Revolution. However, since 2000 there has been a dramatic increase in the publication of older excavations (e.g. Alizadeh 2003a, 2006, 2008; Potts 2001; Sumner 2003), and a considerable range of new work has also been commenced (e.g. Malek Shahmirzadeh 2002, 2003, 2004, 2006a, b; Potts and Roustaei 2006; Fazeli 2007). The combination of new work and new publications of old excavations has seen a number of different chronological schemes put forward (e.g. Alizadeh 2006, 10–13; Alden et al. 2004, Fig. 2; Potts et al. 2006, Fig. 1.3; Weeks et al. 2006b, Fig. 12). Although the sequences that are presented are all broadly similar, they differ in the details, so the chronology for the major regions of southern Iran that is presented in Table 8.1 is not universally agreed. Nonetheless, for purposes of clarity, an attempt has also been made to attribute individual chronological phases to broad periods (Ceramic Neolithic, Early Chalcolithic, Late Chalcolithic) familiar to those working in other regions.

In the early twenty-first century, we are in a position where it is possible to reassess the cultural traditions of southern Iran and consider long-term developments in material culture and technology. There are, however, several factors that constrain our understanding of culture change in these regions during prehistory. Many of these derive from the nature of the archaeological record and the intensity with which it has been investigated. Firstly, only a relatively small number of excavations have been undertaken in any one region of southern Iran, and at many sites, the exposures for which we have reliable stratigraphy are relatively limited in size. This means that regional chronological sequences have often been built up using data from individual sites, or by compiling data from several sites that have overlapping or interlocking chronostratigraphic sequences. For example, many of the sites that have been excavated in Khuzestan and Fars are low mound sites that were either occupied for a single cultural period, or were occupied, abandoned and then reoccupied in a subsequent cultural period when the inhabitants were using dramatically different material culture. In reality, smaller sites that were abandoned and reoccupied regularly often lack evidence for the transitions between cultural periods. Multi-period mound sites that are occupied for extended periods often do have evidence for transitions, but none of the sites that have been excavated has a complete

Table 8.1 Chronology for southern Iran

<table>
<tr><th>Year BC</th><th></th><th>Susiana</th><th>Fars</th><th>Fasa/Darab</th><th>Daulatabad/ Soghun</th><th>Kerman</th></tr>
<tr><td></td><td></td><td></td><td></td><td></td><td></td><td></td></tr>
<tr><td></td><td rowspan="20">Ceramic Neolithic</td><td rowspan="4">Formative Susiana?</td><td rowspan="4"></td><td rowspan="4"></td><td rowspan="4"></td><td rowspan="4"></td></tr>
<tr><td>7000</td></tr>
<tr><td></td></tr>
<tr><td></td></tr>
<tr><td></td><td rowspan="2">Archaic Susiana 0</td><td rowspan="2"></td><td rowspan="2"></td><td rowspan="2"></td><td rowspan="2"></td></tr>
<tr><td></td></tr>
<tr><td>6500</td><td rowspan="3">Archaic Susiana 1</td><td rowspan="3"></td><td rowspan="3"></td><td rowspan="3"></td><td rowspan="3"></td></tr>
<tr><td></td></tr>
<tr><td></td></tr>
<tr><td></td><td rowspan="3">Archaic Susiana 2</td><td rowspan="2">Mushki</td><td rowspan="2"></td><td rowspan="2"></td><td rowspan="2"></td></tr>
<tr><td></td></tr>
<tr><td>6000</td><td rowspan="2">Mushki-Bashi</td><td rowspan="2">Jalyan</td><td rowspan="2"></td><td rowspan="2"></td></tr>
<tr><td></td><td rowspan="3">Archaic Susiana 3</td></tr>
<tr><td></td><td rowspan="3">Jari</td><td rowspan="3">Bizdan</td><td rowspan="2"></td><td rowspan="2"></td></tr>
<tr><td></td></tr>
<tr><td></td><td rowspan="5">Early Susiana</td><td rowspan="2">Early Yahya VII</td><td rowspan="2"></td></tr>
<tr><td>5500</td><td rowspan="6">Shamsabad</td><td rowspan="6">Late Neolithic</td></tr>
<tr><td></td><td rowspan="5">Yahya VII</td><td rowspan="3"></td></tr>
<tr><td></td></tr>
<tr><td></td></tr>
<tr><td></td><td rowspan="11">Early Chalcolithic</td><td rowspan="5">Middle Susiana</td><td rowspan="2">Iblis 0/I?</td></tr>
<tr><td>5000</td></tr>
<tr><td></td><td rowspan="3">EarlyBakun</td><td rowspan="9">Chalcolithic</td><td rowspan="3">Yahya VI</td><td rowspan="3">Iblis I</td></tr>
<tr><td></td></tr>
<tr><td></td></tr>
<tr><td></td><td rowspan="6">Late Susiana/ Susa I</td><td rowspan="3">Middle Bakun</td><td rowspan="2">Yahya VC</td><td rowspan="4">Early Iblis II</td></tr>
<tr><td>4500</td></tr>
<tr><td></td><td rowspan="3">Yahya VB</td></tr>
<tr><td></td><td rowspan="3">Late Bakun</td></tr>
<tr><td></td><td rowspan="3">Late Iblis II</td></tr>
<tr><td></td><td rowspan="4">Yahya VA</td></tr>
<tr><td>4000</td><td rowspan="13">Late Chalcolithic</td><td rowspan="2">Terminal Susiana</td><td rowspan="4">Lapui</td><td rowspan="4">Lapui/Vakilabad</td></tr>
<tr><td></td><td rowspan="4">Iblis III</td></tr>
<tr><td></td><td rowspan="5">Susa II</td></tr>
<tr><td></td><td rowspan="7">GAP</td></tr>
<tr><td></td><td rowspan="3">Initial/Early Banesh</td><td rowspan="3">Unknown</td></tr>
<tr><td>3500</td><td rowspan="8">Iblis IV</td></tr>
<tr><td></td></tr>
<tr><td></td><td rowspan="2">Acropole 17–17X</td><td rowspan="2">Early Middle Banesh</td><td rowspan="2">Unknown</td></tr>
<tr><td></td></tr>
<tr><td></td><td rowspan="4">Susa III</td><td rowspan="4">Late Middle Banesh</td><td rowspan="4">Unknown</td></tr>
<tr><td></td><td rowspan="3">Yahya IVC</td></tr>
<tr><td>3000</td></tr>
<tr><td></td></tr>
<tr><td></td><td></td><td></td><td></td><td></td><td></td><td></td></tr>
</table>

prehistoric sequence and all have evidence for abandonment during specific periods. These dynamics are exacerbated by the fact that the limited areas of large sites that are excavated might not present the entire sequence of occupation at that site. Unless there are comparable excavations at other sites, it is difficult to extrapolate the results from one site to understand the dynamics operating within an entire region. Secondly, although there is considerable information available from archaeological surveys for many regions in southern Iran (Chase et al. 1967; Sumner 1972; Alden 1979; Prickett 1986; Alizadeh 1992; Kouchoukos 1998), the interpretation of survey results is constrained by our knowledge of excavated sequences. Thus while evidence for transitions might be present in surface assemblages, we lack precise evidence for the context in which they took place. There is also the additional problem of the impact of alluviation and modern agricultural practices on site visibility and preservation (e.g. Kouchoukos 1998; Kouchoukos and Hole 2003).

The nature of the archaeology of southern Iran is such that there appears to be a relationship between the adoption of innovation and culture change. It is not always clear where innovations originated, and as we often lack information about the transitions between specific chronologically distinct periods, the archaeological record often implies that innovations appeared suddenly. Thus rather than a nuanced process of invention, adoption and dispersal, we are often seeing things that appear fully formed. This means that there are inherent limitations to identifying the way in which innovations are dispersed and adopted, and it is easy to see why migration is put forward as an explanation for change. As will be shown, there are occasions where migration is the most likely explanation for the archaeological data, but it is also essential that other explanations are considered. With these limitations in mind, it is possible to outline a number of broad trends and patterns in looking at the vast sweep of evidence for ceramic production in southern Iran that dates from the mid-late seventh to the late fourth millennium BC.

Developing Ceramic Technologies in Southern Iran from the Seventh to the Fourth Millennia BC

The earliest evidence for the firing of clay to produce ceramic vessels in the Near East comes from the Central Western Zagros Mountains, which is a key region in the eastern arm of the Fertile Crescent (Mortensen 1992, 276; after Smith and Crépeau 1988; Meldgaard et al. 1963; also Mellart 1975, 70ff.; Hole 2005). The innovation of producing fired ceramic vessels began to spread throughout southern Iran during the seventh and subsequent millennia BC, and further technological innovations in ceramic production developed, were dispersed and progressively adopted in different regions. Although the sequence in which technological innovations appear is broadly similar in different regions, these developments first appear in each at slightly different times. Although there is always the possibility that independent invention occurred, the chronological evidence suggests that in most cases, the major technological innovations typically took place once and were then adopted by populations in different areas, rather than taking place in several different regions. Relatively few detailed analyses of ceramic technology in southern Iran have been carried out, and the exemplary study by Pamela Vandiver (1986, 1987) on the technology of the prehistoric ceramic sequence at Tepe Yahya is the only analysis that provides a detailed assessment of diachronic development. Detailed analyses of ceramics from certain phases in Fars have been conducted by James Blackman (1981, 1989).

The earliest ceramics found at sites throughout southern Iran belong to a very widely spread “soft-ware” tradition (Dyson 1965, 217, Voigt and Dyson 1992, 266; also Vandiver 1987; Beale 1992, 282; Weeks et al. 2006a, b). Vessels are characteristically handmade using chaff-tempered clay, have thick walls, are lightly fired and are very crumbly. There appears to be local variations in the density of the fabric, the size and density of the chaff temper and the degree of surface finishing, but in essence this material belongs to one overarching potting tradition. This may indicate that with the arrival of new technological innovations, there was also scope for experimentation. In fact, Vandiver (1986, 1987, 1995) has shown that early ceramic vessels from the North and Central Western Zagros, Deh Luran, Kerman and into Pakistani Baluchistan were produced using SSC. In Fars and Kerman, it has been noted that baskets were often used in the forming process, and many vessels were subsequently covered with a coating of untempered clay, which was slipped and burnished to form a smooth surface that was often then decorated with monochrome, bi-chrome or polychrome pigments (Matson in Chase et al. 1967, 150; Vandiver 1986; Alden et al. 2004, 36–37; Weeks et al. 2006a, b, 73–74; Alizadeh 2006, 8–10). The added layer of clay is often poorly preserved, and flakes away from the vegetal tempered core (e.g. Alden et al. 2004, Fig. 9.11–9.12). Some vessel types are only wet smoothed and burnished on the exterior and chaff impressions are still visible (Beale 1986, 42). The Late Neolithic (c. 5500–5000 BC) in some regions sees a disappearance of painted surface decoration. The fired vessels are all generally soft, the fabric is fragile and the painted decoration frequently washes off. This suggests that the firing temperature was relatively low, most probably less than 700°C (after Blackman 1989, Table 8.2; Bernbeck 2004).

Table 8.2 Basic chronology for the southern Iranian sequence of technological development

Period	Ware type	Method of manufacture	Hardness and firing temperature
7th–6th millennium BC	Soft/coarse	Hand/SSC	Soft/Low-med (<700°C)
5th millennium BC	Buff-ware	Slow turned	Hard/med-high (850°C >1,000°C)
E. 4th millennium BC	Burnished coarse	Slow turned	Hard/med-high (850°C >1,000°C)
E. 4th millennium BC	Slipped fine	Fast turned	Hard/med-high (850°C >1,000°C)
L. 4th millennium BC	Coarse	Wheel	Hard/med-high (850°C >1,000°C)
L. 4th millennium BC	Vegetal tempered	Wheel/mould	Hard/med-high (850°C >1,000°C)

Given the long-term conservatism in the production process, the low-temperature firing and the varied decorative patterns produced, it might be assumed that these vessels were the product of small-scale and presumably part-time household production systems (following Costin 1991, 2001; see also Rice 1991). However, many of these are not "simple" vessels, and the careful use of a thin untempered layer of clay to produce a fine surface finish, the very high quality slipped and polished finish, and the wide range of often highly elaborate geometric motifs suggests that the potters possessed a high degree of skill in carrying out a relatively sophisticated *chaîne opératoire*. It is also likely that each vessel was both labour- and time-intensive to produce. So, although these potters might have been working at a small and possibly part-time scale, they should probably be considered specialists if only for the skill evident in the production and decorations of the vessels.

With the shift to the Early Chalcolithic in the fifth millennium BC, there were several technological innovations in the process of ceramic production, and these appear to have been adopted progressively in different areas. The first is the choice to use calcareous clays that do not require the addition of chaff-temper and which fire to a buff colour. These calcareous clays largely replace the use of vegetal tempered clay thereafter in most areas. Concurrent with this, there is also evidence for the use of basic turning devices and the application of distinctive black painted decoration, which replaces the bi-chrome and polychrome decoration applied to the Late Neolithic ceramics in some regions. These shifts are accompanied by sharp increases in the temperatures at which vessels are fired, to between 850 and 1,000°C, which results in a notably harder ceramic (following Blackman 1989, Table 8.1). That this Early Chalcolithic firing technology was not completely controlled is attested by the evidence for significant numbers of over-fired vessels, particularly in the early fifth millennium BC, and this phenomenon is evident at sites in both Iran and Mesopotamia. It is, however, notable that by the end of the fifth millennium BC in Iran, potters were producing a wide range of very refined ceramic vessel types that were typically decorated with very elaborate motifs and patterns (e.g. Langsdorff and McCown 1942, Plates 22–80; Beale 1986, Figs. 4.19–4.26; Delougaz and Kantor 1996, Plates 159–192; Alizadeh 2006, Figs. 23–52). This is quite different to the situation in Greater Mesopotamia which witnesses what Wengrow (2001) has described as an "evolution of simplicity", where decoration becomes progressively simplified during the fifth and fourth millennia BC.

We know little about the organisation of ceramic production during the Early Chalcolithic (c. 5000–4000 BC) period in Iran, other than being able to note that there was considerable homogeneity in the technology generally, and in the motifs being used in specific regions. It is clear that potters were able to produce hand-formed vessels of incredible fineness, which were often literally covered with decorative motifs of particular complexity. In talking about Khuzestan, Hole (1987, 91) has made the contrasting observations that this technological homogeneity is indicative of production taking place in only a few places and the material then being widely distributed, but also that the presence of wasters on many sites, suggests that production was more widespread. Sumner (1994, 59) has argued that during this period the advanced production technology, the high quality of the pottery produced and the absence of evidence for ceramic production at the vast majority of sites in Fars indicate that ceramic production was being increasingly centralised and was progressively becoming a more formal specialisation. The fact that the fast wheel was not being used suggests that a significant amount of time must have been expended to produce and paint these vessels. In Kerman, the Early Chalcolithic sees increasing sophistication in the organisation of production (Vandiver 1986), particularly with the introduction of potters marks on beakers. Beale and Lamberg-Karlovsky (1986, 254) have suggested that this might be related to the large-scale production of similar looking vessels and/or the necessity for groups of pots to be identifiable in communal kilns.

By the beginning of the fourth millennium BC, there are a range of further technological innovations in ceramic production, including clear evidence for a dramatic shift towards simplicity and efficiency in both the production process and the finished products. Significant differences in the timing and pattern of adoption of some of these innovations also become apparent. For example, in the lowlands, there is a last flourish of elaborate painted decoration during the late fourth millennium BC, focussed at Susa, which is followed by a wholesale change in the ceramic assemblage that comes to be characterised by mass-produced vessel types, including both wheel thrown and mould-made forms (e.g. Voigt and Dyson 1992; also Delougaz and Kantor 1996; Potts 2001; Potts et al. 2006). These vessel types appear to have been introduced from Mesopotamia, and together with a range of other innovations play an important part in debates about the relationship between Mesopotamia and its neighbours during the later fourth millennium BC (Algaze 1993, 2001; Stein 1999; Potts 1999; Butterlin 2003). By the start of the fourth millennium BC in the highlands, much of the surface decoration disappears and it is replaced by treatments, such as fine slips and burnishing, which continue to be used for many centuries before mass-produced wheel thrown and mould-made forms seen in the lowlands begin to appear (Petrie et al. 2006a, b, 2007). The dynamics of these changes are outlined below, but in general, throughout the fourth millennium BC, there is a progressive increase in the speed of turning until ultimately the fast wheel is adopted. It is also notable that in some areas, the pre-existing production technology was maintained for the production of certain vessel forms. From the mid-fourth millennium BC, a number of what appear to be “disposable” vessel forms are adopted, including the distinctive bevel-rim bowl. These continue being used into the early third millennium BC.

What we know about the organisation of Late Chalcolithic period (c. 4000–3000 BC) is that ceramic production is variable. In Khuzestan, centralised ceramic production appears to have taken place in workshops from where vessels were then widely distributed (Johnson 1973). Kiln sites dating to the earlier fourth millennium BC have been discovered in the Kur River Basin in Fars, suggesting the production remained both specialised and centralised (Sumner 1988, 33). During the later fourth millennium BC, there was a clear shift in approaches to organisation, with the simultaneous operation of distinct production systems to produce vessels from vegetal and grit-tempered clay fabrics (Alden 1979; Blackman 1981, 1989). It also appears that in the Kur River Basin, specific vessel forms were being produced at specific sites, suggesting that production was centralised along product-specific lines (Alden 1979). However, there is no consistent pattern and in the later fourth millennium BC in Kerman, Vandiver (1986, 99) has shown that SSC continued to be used, in combination with the newly adopted technologies of wheel throwing, coiling and mould making. This emphasises that change is by no means uniform across southern Iran, and highlights the strength of the socio-cultural boundary between Fars and Kerman.

The vast sweep of evidence for technological innovations in ceramic production is summarised in Table 8.2. While there are widespread similarities in approach in different regions, it is important to emphasise that these technological innovations are not adopted everywhere simultaneously. The other critical factor is that there were also dramatic regional differences in approaches to decoration and style, which suggests that a range of cultural dynamics were in operation.

Patterns of Technological and Stylistic Innovation in Ceramic Production in Southern Iran

At the broadest scale, it appears that there were variable cycles of innovation in operation in southern Iran throughout prehistory. In most instances, once they had become established in one area, major technological innovations or sets of innovations appear to have been transmitted to neighbouring populations and adopted relatively quickly. This was then followed by protracted periods that were technologically conservative, but which were marked by the elaboration of surface finishes and decorative schemes (i.e. post-forming processes; after Wright 2002), in what equates to changes of style and fashion. This was then followed by another cycle of technological innovation and subsequent stylistic elaboration, and so on. This is in some ways related to Caldwell's (1968, 183) observation that in southern Iran there were processes of nuclear influence followed by regionalism, which was in turn followed by nuclear influence, thus repeating the cycle. The patterns of this process can be most clearly seen by reviewing the sequences of the major regions in southern Iran, beginning with Khuzestan.

The first fired vegetal tempered soft-ware ceramic vessels are used in Khuzestan during the early seventh millennium BC, which is somewhat later than the earliest ceramics from sites in the Central Western Zagros (Voigt and Dyson 1992, I. 124, 129, II. Table 8.2; Alizadeh 2003a). No evidence for the independent invention of fired ceramics has yet been discovered, and we do not know precisely how the invention of firing ceramic vessels made its way to Khuzestan. The existence of obvious precursors in the Central Western Zagros and the fact that the earliest material that has been discovered already shows some sophistication in the technology and approaches to surface decoration suggests that it was not a local innovation. Once ceramics start being used in Khuzestan there is "an unbroken and evolutionary" sequence from the *Formative Susiana* to the *Archaic Susiana 3* phase (Alizadeh 2003a, 8; 2008, 62–66; see Table 8.1). Although there are elaborations, the primary technology essentially remains unchanged until the mid-sixth millennium BC (*Formative Susiana – Archaic Susiana 3*). Nevertheless, during this very protracted period, we see potters create literally dozens of different approaches to surface finish and decoration (e.g. Delougaz and Kantor 1996, 211–247; Alizadeh 2003a, 47–48, 2008, 54–56), which were applied to vessels that show a gradual development and elaboration of form (Alizadeh 2008, Figs. 3a, b). This period also sees the dispersal of both technological and stylistic approaches to other areas, including Fars (see below). In the early sixth millennium BC (*Archaic Susiana 3*), the appearance of distinctive decorative motifs indicates that long-range contact was occurring, resulting in stylistic influence. It is not until the mid sixth millennium BC (*Early Susiana*; see Table 8.1) that grit temper begins to be used in combination with vegetal temper, and this change is also evident at sites in Deh Luran and southern Mesopotamia (Voigt and Dyson 1992, 130; Alizadeh 2008, 9, 66). Although this technological change is widespread, the ceramics from each of these regions continue to exhibit local decorative characteristics (Alizadeh 2008, 9), suggesting that although the adoption of the technological choice was widespread, the need to maintain local approaches to decoration was socio-culturally important. In the *Middle Susiana* phase, we see the introduction of the range of technological innovations related to the production of harder fired buff-wares that have grit and sand inclusions, and we also see early developments in the production of red-wares (Alizadeh 2008, 66–67; see Table 8.1). During the remainder of this phase, we again see a protracted period marked by the progressive development and elaboration of vessel forms and approaches to surface finish and decoration (Alizadeh 2008, Figs. 3a, b), and as with the *Archaic Susiana* phase, the *Middle Susiana* phase sees the dispersal of both technological and stylistic approaches to other areas (see below). The process of stylistic elaboration in the absence of major technological innovation continues into the *Late Susiana* phase, which shows some indications of stylistic influence from the highlands of Fars, primarily based on the use of dots in specific motifs, which are first evident in the *Early Bakun* ceramic assemblage and continued in use into the *Late Bakun* period (Alizadeh 1992, 25–26, 2008, 74–75; see Table 8.1). The production of red-wares also becomes more elaborate (Delougaz and Kantor 1996, 170, Pl. 162.I-AA). Alizadeh (2006, 23; 2009, 134–135) has suggested that the appearance of distinctive *Late Susiana I* pottery in the copper-rich Central Plateau may be linked with the exchange activities of south western mobile pastoralist tribes who were engaging in procuring copper, turquoise and lapis, all of which began to appear regularly in Fars, lowland Susiana and Mesopotamia in the fifth millennium BC. The *Susa II*/*Uruk* phase in Khuzestan marks a period of dramatic cultural and technological influence from southern Mesopotamia, including the adoption of administrative technologies in the form of seals, sealings, and numerical tablets (see Table 8.1). The changes to the ceramic repertoire are dramatic, and include the addition of a large range of Mesopotamian vessel shapes, and the innovative technologies used to produce mould-made bevel-rim bowls and various wheel-made vessel forms (Voigt and Dyson 1992, 130–131). The picture during the *Susa III*/Proto-Elamite period in the late fourth millennium BC in Khuzestan is complicated by the fact that we lack evidence for the transition from the *Susa II* period (Petrie et al. 2010). However, this phase is not characterised by any major technological innovations in ceramic production. There are, however, clear stylistic changes in vessel form, which have parallels in the highlands of Fars (Dittman 1984). This was also the period that saw the development of a fully fledged proto-literate text system commonly known as Proto-Elamite or Susa III, whose use appears to have spread quickly, but in several distinct phases across the Iranian Plateau (Dahl et al. in press).

It is not yet clear when the earliest ceramics were produced in Fars, but all early ceramics were produced using a similar soft-ware production technology. Tantalising hints have come from the discovery on the surface of some sites of ceramics showing "swoosh-pattern" decoration akin to *Formative Susiana* types that date to the early seventh millennium BC (Alizadeh 2006, 7). We do, however, have more reliable evidence for the widespread adoption of technologically and stylistically similar pottery across a wide area during the late seventh and early sixth millennium BC (*Mushki* phase; see Table 8.1). This appears to represent an extensive adoption of innovations that took place elsewhere. It is also likely that this spread originated either in Khuzestan or further north in the Zagros at sites, such as Qaleh Rostam (Weeks et al. 2006a, b, 23). Whether or not we are looking at the dispersal of a migrant population or just the adoption of the innovation of pottery making by an existing and previously aceramic population is not yet clear. Subsequent to this, there is clear evidence for the rapid proliferation of regionally distinct decorative styles (*Jari* + other local Late Neolithic decorative styles; see Table 8.1; Sumner 1977; Alizadeh 2006, 9; Weeks et al. 2006a, b, 13ff.), and little evidence for technological innovation over a protracted period. In the shift from the Neolithic to the *Bakun* period in Fars, we see almost a complete abandonment of the technologies used to produce soft-ware ceramics, and the adoption of the innovations related to the production of harder fired buff-wares that have grit and sand inclusions. Alizadeh (2006, 11) has argued that the black-on-buff pottery that appears in highland Fars in the *Early Bakun* (*Middle Fars*) phase has no known antecedent in Fars, and was most probably introduced from lowland Susiana through a migration of people and/or specialised potters. It is, however, notable that in Mamasani, several motifs have been isolated in the Late Neolithic repertoire at Tol-e Nurabad that have close parallels to motifs seen in the *Early Bakun* levels, suggesting that in the Mamasani region, at least, there are some indications of continuity in approaches to motifs from the Late Neolithic (c. 5500–5000 BC) (Weeks et al. 2006a, b). The protracted *Middle* and *Late Bakun* periods (see Table 8.1) see little change in terms of the technology used to produce painted buff-ware ceramics, but considerable elaboration in the approaches to surface decoration and vessel form. Although the decorative style used during the *Middle Bakun* period in Fars is distinct, there are several motif combinations that have a very wide ranging distribution in southern and western Iran, which suggests the operation of specific types of interaction between the populations living in different regions (Alizadeh 2006, 11). It is not clear whether this was the product of the movement of people or the movement of material. The *Late Bakun* period sees a culmination of the elaborate surface decoration in Fars, and there appears to be some highland influence on the decorative styles used in Khuzestan (Alizadeh 2006, 23; 2008, 74–75; see below). In the shift from the *Bakun* to the *Lapui* period, we see a complete shift in both the types of clays that were being exploited and the approaches to surface decoration to the point where figurative and geometric decoration are essentially abandoned at more or less the same time (Sumner 1988; Blackman 1989). This is all the more marked as it follows the extremely vivid combinations of geometric and figurative motifs used during the *Late Bakun* phase. The key technological shift in the *Lapui* period seems to be the increased use of rotation to produce particularly refined rim forms and quite regular circular vessel apertures, which were often lacking in the preceding period. At least in the Mamsani region, a shift from the production of red fine wares to the production of buff fine wares with a red slip takes place during the *Lapui* period (Petrie et al. 2006a, 2007, in press), but this is the only apparent stylistic innovation. With the shift to the *Early Banesh* period, the bevel-rim bowl appears for the first time in the highlands, but this form is one of the few indicators of contact with the lowlands at this time (Alden 1979, 1982, 2003). It is with the *Middle Banesh* phase (c. 3300–3000 BC) that we see an even more dramatic shift towards simplicity and efficiency over refinement, with the use of the fast wheel and moulds to mass produce vessels. This happens somewhat before the adoption of sophisticated administrative technologies and proto-literate texts (Alden 1979, 1982). Some degree of regional variety in approaches to fabric preparation and surface finishing suggests that these innovations did not completely supplant existing technologies as they had in previous periods (Petrie et al. 2006a, b).

In Kerman, the innovation of fired ceramic vessels occurs in the mid-sixth millennium BC (*Yahya VIID – Tepe Gaz Tavila*), many centuries after this process took place in Fars. The similarities in approach suggest that the technology has moved, but as in Fars, it is not yet clear whether we are looking at a dispersal of a migrant population or the adoption of the innovation of pottery making. In the early fifth millennium BC (*Yahya VI/Iblis 0/I*), we see the local production of a hard fine-ware with grit inclusions (*Soghun/Bard Sir Painted* ware) that has some stylistic parallels with *Early Bakun* wares from Fars. However, the technological innovations required to produce these wares may have developed independently in Kerman, as they were produced using SSC and clay from local sources (Beale and Lamberg-Karlovsky 1986, 256; Vandiver 1986). It is also interesting that the production of these fine wares seems to coincide with precocious metal working innovations that appear to have taken place at the site of Tal-i Iblis. Although the pyro-technologies required for each process are very different (Frame 2004), we are nonetheless seeing sophisticated control of high temperatures in both instances. Are we seeing craft practitioners in Kerman engaged in a broad range of innovative experimentation during this period? In the mid-fifth millennium BC (*Yahya V/Iblis II*), there is clear evidence for the importation of black-on-buff ware vessels from Fars to Kerman (Beale 1986, 86–87), and the related production technologies appear to have been adopted and elaborated upon in the late fifth and early fourth millennia BC with the local potters first producing black-on-buff ware and then black-on-red ware (Beale 1986, 67–82, 257, Fig. 4.1). These wares appear together with what are referred to as *Lapui-related* red-ware vessel forms, which are more characteristic of the early fourth millennium BC in Fars, suggesting that this ware type and its associated technological traditions and innovations appear earlier in Kerman than they do in Fars, and potentially originated in the east (Beale 1986, 87; Voigt and Dyson 1992, 145, 149). There appears to be further elaboration of the local ceramic assemblages during the early-mid fourth millennium BC with the production of the various *Aliabad* wares (Chase et al. 1967, 79, 184). It is, however, during this period that the use of the fast wheel and moulds to mass produce vessels like the bevelled rim bowls, shoulder spouts and wheel-made vessels with string cut bases occurs (Caldwell 1968, 182). The reoccupation of Tepe Yahya (IVC) during the late fourth millennium BC appears to be a foreign initiative (Lamberg-Karlovsky 1978; Potts 2001, 198) and involves the consolidation of the technologies used to produce bevel-rim bowls, trays and conical cups, as well as the use of Proto-Elamite/Susa III tablets.

Innovation and the Dynamics of Transmission, Adoption and Elaboration

Although technological innovations in ceramic production technology appear to have spread relatively quickly between neighbouring regions, the pattern appears to have been staggered and inconsistent. This very much conforms to McGlade and McGlade's (1989, 288ff.) suggestion that innovation is a dynamic yet evolutionary force, and the process of transmission/transfer being likely dictated by constraining and facilitating social processes. These constraints operate independently in each region, and are dependent upon local contexts and factors. Beale and Lamberg-Karlovsky (1986, 263–264) have noted that at Tepe Yahya, there appears to have been little change during *Yahya VII*, but during *VI-VC* and *VB-VA*, the rate of change accelerates (Beale and Lamberg-Karlovsky 1986, 263–264). They suggested that the apparent manifestation of long periods of conservatism punctuated by shorter periods of rapid change in cultural development at Tepe Yahya finds interesting parallels in the theory of "punctuated" evolution in biology (Beale and Lamberg-Karlovsky 1986, 264 citing Gould 1982, 184; see also Eldredge and Gould 1972; Gould 1977). This observation broadly conforms to the principles of macroevolution as outlined by Zeder (2009), where change is a punctuated process in which periods of rapid transition are followed by long periods of relative stasis. This paper proposes that a nuanced model that distinguishes intermittent punctuated innovation in technological processes taking place within the context of relatively continuous innovation in stylistic processes can be applied to the prehistoric archaeology of southern Iran as a whole.

There are two critical questions that ideally must be answered in order to understand this process, firstly why does innovation operate in this manner in southern Iran, and secondly, what transmission processes existed that facilitated the process. There is a lack of strong archaeological evidence for both, but these are nonetheless questions of archaeological interest, so it is worth offering some speculation drawing on the evidence at hand.

The process of transmission has been more clearly addressed in the southern Iranian context. The archaeological record is such that it appears as though technological innovation had the potential to disperse quickly once it had occurred, and this implies that specific types of communication and interaction were taking place between populations inhabiting the different parts of southern Iran. However, the fact that there are periods of technological conservatism and different regional trajectories suggests that the mechanisms that facilitated (and also constrained) the dispersal of innovations are unlikely to have remained static over time. The somewhat nebulous complex and variable mechanisms by which innovations were dispersed and/or transmitted that were mentioned in the introduction are perhaps the most difficult elements to characterise, as they are perhaps most likely to be dictated by local conditions and contexts (Sherratt 1997; Shennan 1989, 1996; McGlade and McGlade 1989, 282), and the role of individual action and choice (Layton 1973, 1989). There are two critical and interconnected factors that are significant in the context of southern Iran. The first is the geography of the Zagros, which constrains the way people can live and move through this region, and it is likely that the landscape of southern Iran played a specific role in the formulation of ethnic identities throughout prehistory. The second is the likelihood that the regions of southern Iran were more or less continually linked by people moving through this landscape. In the diverse geographic context of southern Iran, mobility is potentially the key factor for explaining the transmission of innovation and the specific distributions of distinctive ceramic forms and motifs throughout prehistory. However, the way that mobility was manifested is very significant. In each period or phase, it is possible that we are dealing with the mobility of objects, and both innovation and material might have moved as a result of the trade and exchange of particular vessels and/or their contents. Such movement of material can also result in imitation and emulation. It is also possible that the distribution of certain forms and styles represents the mobility of people, ranging from entire populations to individuals, such as itinerant potters or marriage partners with technological knowledge, and the vocabulary of distinctive decorative motifs which move as a result of exogamous marriage traditions. It is most likely that this would have been variable in both time and space, and in all of these instances, specific socio-cultural behaviours would have been involved, particularly the processes of acceptance and resistance.

In discussing the Neolithic period in Fars, Weeks et al. (2006a, b, 20ff.) have emphasised the role of seasonal mobility, raw material exchange and other more social factors, such as exogamous marriage, as mechanisms for facilitating interaction and the spread of ceramic technology between communities during the *Mushki* phase. They have also suggested that the regionalisation in ceramic decoration might be a product of the growth in populations within individual valley systems, which reduced the need for mobility in order to maintain viable populations (Weeks et al. 2006a, b, 22). This model sees mobility in the later Neolithic phase primarily in human terms, and does not account for the ongoing role of pastoralism. In an extensive body of research comprising both research papers and monographs, Abbas Alizadeh (1988, 1992, 2003a, b, 2006, 2008, 2009) has advocated specific connections between mobility and processes of cultural change in southern Iran at various points throughout prehistory, and he sees change primarily being a result of the actions of mobile pastoralists. As noted above, he has argued that the black-on-buff pottery that appears in highland Fars in the *Early Bakun* (or what he calls the *Middle Fars*) phase has no known antecedent in Fars, and was most probably introduced from lowland Susiana through a migration of people and/or specialised potters (Alizadeh 2006, 11). The similarities between the distribution of fifth millennium BC black-on-buff pottery and the areas of the highlands used by the modern Qashqa'i have also been emphasised (e.g. Alizadeh 2006), although the validity of this correlation has been questioned (e.g. Potts 2008; Askari Chaverdi et al. 2008). Migration as an explanation for culture change is not straightforward, however, and leads to questions like "where did the pre-existing population go?" and "how were the migrants dealt with by the incoming population?" Are we seeing processes of demic or cultural diffusion or a combination of the two? In the southern Iranian context, it is more than likely that a straightforward movement of a population from one region to another is too simple an explanation. Where there is evidence for the introduction of a range of innovations related to ceramic production, such as that witnessed in the *Early Bakun* period, then it is almost certain that complex socio-economic processes were in play, including the need to assimilate new technologies and also potentially new populations. There is also some possibility that such obvious technological changes might reflect cultural responses to changing socio-economic requirements, such as increases in population that necessitated a means of producing ceramic vessels more efficiently.

When thinking about such a broad range of technological developments and the processes of innovation and transmission, it is important to keep several significant parameters in mind. Although ceramics would have been used by every family if not every person within a given community, the nature of the vessels being used in southern Iran is such that it is highly unlikely that every family in every town, village or even household were involved with their production, even in the earliest Neolithic. Most of any prehistoric population would have been involved in some fashion with day-to-day activities, such as subsistence farming and animal management, whereas ceramic production is likely to have always been some sort of specialised process, involving a particular knowledge base. This is specifically the case with the inter-connected processes of the production of ceramic fabrics, decorative styles, and the control of firing temperatures. Although direct evidence is lacking, the specific skills involved and the quality of the material produced suggests that from as early as the ceramic Neolithic in southern Iran, there were relatively few potters in any one village, community or region. This makes it likely that virtually all ceramic vessels at any one settlement were the products of a relatively small proportion of the population that then used those products. Innovations in ceramic production technology thus need only have been passed between relatively small numbers of people in any two regions. The likelihood that face-to-face contact was taking place between potters in individual regions is interesting when we consider the fact that many of these technologies are very widely distributed (e.g. Wright 2002, 410–414). This suggests that some mechanism must have existed that facilitated both contract and information transfer. As this was most likely occurring between populations living at specific sites and in particular regions, it might be best described as an interaction network. Developments like the introduction of increasingly faster rotation and ultimately the fast wheel, and the gradual minimisation of surface decoration represent changes to the way ceramic production was organised that are likely to have seen even fewer people involved in the production process through time. This would have further reduced the number of individuals involved in processes of transmission. Perhaps the key point is that while we are observing processes of culture change, in the case of ceramics, the innovations are actually in the hands of a small number of people, but have an impact on entire populations. If this reconstruction is correct, then it suggests that although the innovations in ceramic production were widespread and the products that resulted were widely used, the transmission and transfer of technical knowledge and innovation were restricted to practitioners and producers. This suggests that the dispersal and transference of those innovations between groups were taking place within an open system, but this system was restricted to certain people within groups, so in respect to the detailed knowledge being transferred, the system was closed. Nevertheless, for the innovations to be accepted they need to be recognised as being beneficial by consumers. It is therefore perhaps valid to think of it as both an open and a closed system, depending on where one is looking from. The question as to what mechanisms or interaction networks existed to facilitate this transfer between groups is at present unanswerable.

Cutting across the trend towards regional distributions of specific motifs and motif combinations, there are also instances when specific vessel forms and/or decorative styles are particularly widespread. For example, Alizadeh's (1992, 2006, 11) comparative analysis of the *Middle Bakun* pottery assemblages from Fars and those from contemporaneous sites in Susiana, Behbehan and the Central Zagros has showed that this period witnessed the widespread distribution of a number of shared decorative motifs that did not exist previously. It is possible that this reflects imitation of styles by potters living in different regions, but it could also be indicative of the existence of a reciprocal social system involving the trade and/or exchange of vessels and their contents as gifts to gain access to foreign lands (Alizadeh 2006, 23; following Earle 1994; Gregory 1982). Alizadeh (2008, 74) argues that "the similarity between the ceramic assemblages of the lowlands and the highlands may be attributed to a shared tradition among craftsmen and interregional marriages rather than to mere imitation", but the specific reason for this is not clearly stated. It is important to remember that vessels may also have social significance, and the widespread distribution of specific decorative motifs may also indicate a similar distribution of specific practices related to those vessels. Possibilities abound. One major unresolved factor in all of this is that it is not at all clear where the pottery was being made in most instances. Although it is theoretically possible to investigate this, it has not yet been attempted on a wide scale. There are clearly major differences between the simultaneous production of distinctive vessels and motifs in different areas and the production of these objects in one area and their subsequent distribution, and until such dynamics can be characterised, elaborate discussion of the significance of the distribution of specific motifs is likely to be futile.

It is not entirely clear why the punctuated pattern of innovation occurred in southern Iran. The archaeological evidence from each region suggests that major technological innovations in ceramic production appeared in clusters, and subsequent to this there was a tendency not to engage in technological innovation for protracted periods. These innovations would likely have had various social, cultural and economic effects, and the mechanism or mechanisms by which they were dispersed and/or transmitted must have been both complex and variable. Kohl (2008) has argued that the spread and adoption of innovation across broad areas reflects shared developments, which are facilitated by the existence of "shared social fields". In most cases, it is not clear whether technological changes were the cause or the result of broader socio-economic change, and while a concept of punctuated technological innovation might describe the patterns that are evident in southern Iran, it does little to explain why this takes place. McGlade and McGlade's (1989) range of constraining and facilitating parameters provide a useful explanatory framework in this respect. Shennan (1989, 1996, 289) has suggested that when innovation occurs, it is at some level a conscious decision made in response to a particular situation where people are either prepared or required to suspend their usual routines. If this is correct, then it should also hold true for the resistance of innovation. If people are neither prepared nor required to suspend their routines, the resistance of innovation is more likely, and presumably it will either not take place, or if it does occur it may not be adopted.

Caldwell's (1964, 143) concept of the "interaction sphere" posits a correlation between interaction and innovation such that when different cultural traditions meet, new approaches are introduced to each group, and what he describes as new arrangements of forms – innovations and inventions – can be built. This has been followed by Lamberg-Karlovsky (2009, 75, 82), in his emphasis on the importance of interaction as a facilitator and motivator for change, and the role of agents in the process of trade and exchange. He suggests that certain institutions and incentives existed to allow for innovation and economic growth (Lamberg-Karlovsky 2009, 75, 82). Roux (2003) has argued that technological innovation occurs when there is both demand and enabling conditions. Major innovations are thus most likely to appear and be adopted when societies are predisposed to accept change and/or have a need that must be satisfied. Drawing on classical sources, D'Angour (2000) has argued that for innovation to succeed, it must appeal to existing individual and social perceptions about what is valuable. If it fails to do this, it is liable to be resisted. In terms of ceramic production, it appears that throughout prehistory in southern Iran, major technological innovations spread quickly over short distances, and often across social boundaries. This suggests that at certain points in time, potters were open to innovation and populations were ready to accept these new approaches. However, in general, there were phases of protracted technological conservatism, which indicates that there were long periods during which there was no imperative or requirement for change. All the while, however, there appears to have been a constant need to reaffirm the existence of socio-cultural boundaries through the generation of regionally distinct decorative styles. It is also important to remember that although ceramics are but one element in a much broader cultural milieu, the technological innovations related to ceramic production were taking place concurrent with innovations in metal production, approaches to subsistence, and administrative technologies in the form of seals, sealings, tablets and ultimately the development of a fully fledged proto-literate text system whose use spread across the entirety of the Iranian Plateau. These innovations also appear to follow a similar trajectory to the punctuated pattern seen for ceramic innovations, which is significant as they all contribute to the increases in socio-economic complexity that culminated in the appearance of the first urban cities in Iran (e.g. Tal-e Malyan). Lamberg-Karlovsky (2003) has argued that the innovation of literacy was rejected on the Iranian Plateau in the early third millennium BC, and the subsequent phase of socio-economic development during that millennium was essentially illiterate.

The *Lapui* phase ceramics that are used in Fars between c. 4100–3500 BC are an interesting example of the role of cultural assemblages in the past, and patterns of innovation and adoption. The *Lapui* assemblage comprises two types of red-wares: a coarse-ware, which typically has an irregularly burnished surface; and a fine-ware, which either has a red or buff fabric and a polished red slip (Sumner 1988; Petrie et al. 2006a, b). Vessels in both wares appear to have been turned using a slow wheel or tournette (Blackman 1989; Petrie et al. 2006a, b). During the preceding *Bakun* period in Fars (4800–4100 BC), the ceramic assemblages are completely dominated by painted buff-ware ceramics made from calcareous clay. The contemporaneous ceramic assemblages from Khuzestan (*Middle-Late Susiana* – 5100–3900 BC) are likewise dominated by painted buff-ware, although a small but significant part of the assemblage is made up of coarse burnished red-wares in shapes that are akin to the *Lapui* vessels subsequently used in Fars. Similarly, in Kerman, the ceramic assemblages of the later fifth millennium BC are dominated by black-on-buff or black-on-red wares, but also include examples of a burnished red-ware that has been referred to a *Lapui* or *Lapui-related* ware (Beale 1986, 87; Voigt and Dyson 1992, 145, 149). The absolute chronologies of these assemblages suggest that burnished red-wares were being used contemporaneously and were small but significant components of the ceramic assemblages in use in both Khuzestan and Kerman. Although Fars is situated in between these two regions, red-wares do not appear with any frequency until the very end of the fifth millennium BC, at which time they completely replace painted buff-wares. This does not happen in either Kerman or Khuzestan. This can be interpreted in several ways. In the first instance, there must have been some specific reasons as to why the innovation of producing coarse burnished red-ware and red-slipped ware was resisted in Fars until the late fifth millennium BC. It is then perhaps all the more surprising that once these technological and stylistic innovations were adopted, they completely replaced the existing technologies and decorative styles. Blackman (1989, 104–105, 106) proposed that the observed changes are unlikely to have been the result of the introduction of new, previously unknown technology, through diffusion, innovation or migration, but rather are more likely to be the result of cultural responses to changing socio-economic requirements. Sumner's (1988) initial interpretation of the survey data from the Kur River Basin suggested that the *Lapui* period marked a drop in the regional population, whereas Alizadeh (2006, 26) has suggested that it actually saw a population increase. There is also evidence from the Mamasani region that *Lapui* fine and coarse wares were each being produced from distinct sources of raw materials and then distributed to multiple sites. The disappearance of painted decoration suggests that potters were choosing different approaches to decoration. That this material became so widespread suggests that this shift was accepted by a population that was either receptive to or demanding change. Until we know more about the role of the decorated *Late Bakun* vessels that were being replaced, it is difficult to comment on which option is the more likely. The dynamics in the mid-late fourth millennium BC are also interesting, as in most cases, the new material culture elements, such as wheel and mould-made vessel forms, are nested within the existing local material culture assemblages (see Potts 2001; Petrie et al. 2006a, in press). This contrasts with the situation of the earlier fourth millennium BC, and shows that in some instances, technological innovations completely replace existing practices, while in others, they are added to those practices. Which of these options transpires in any circumstance is likely to be dictated by a range of variables, including the nature of both the innovation and the local social context into which it is transmitted.

Many of the technological changes that we see in ceramic production are related to the minimisation of energy expenditure and the maximisation of the rate of production of individual vessels. While vessels continue to be decorated with elaborate schemes until the beginning of the fourth millennium BC across much of the region, it is conceivable that pressures from population growth resulted in a progressive need to produce ceramic vessels more simply in order to satisfy demand. Larger populations are also likely to have required different social behaviours, not least because of increased potential for social differentiation and there is also the potential that ideological factors played a role (e.g. Zeder 2009). Given the clear changes towards the use of more efficient production techniques through time, it seems likely that the role of ceramics themselves also underwent change between the early ceramic Neolithic and the Late Chalcolithic phases. This is most clearly evident in the disappearance of painted decoration during the fourth millennium BC in Khuzestan (*Susa II*) and Fars (*Lapui*), concurrent with the adoption of technologies that enable mass production of utilitarian vessels.

Each of the major regions discussed here appears to present a distinctive sequence of material culture characterised by idiosyncratic painting styles, decorative motifs and vessel forms. Drawing on the ethnographic evidence of Gosselain (1999), Wright (2002, 413) has noted that potters are willing to incorporate new techniques in post-forming manufacturing processes, and suggests that these are most readily transmitted between potters. She also argues that the small-scale transfer of technological approaches preceded more intensified interaction involving the transfer of raw and finished materials (Wright 2002, 414). It is interesting that in each phase of elaboration in the highland areas, regionally distinctive material assemblages appear to recur in more or less the same areas after each cycle of technological innovation. This suggests that there were also repeated cycles of correlations between the geographically proscribed regions of southern Iran and assemblages of material culture that were used. Although this runs the risk of being deterministic, it is important to reiterate that the geography of southern Iran was almost certainly a critical constraint on the processes of interaction, communication, innovation and transmission during prehistory. The geographic distribution of the cultural material between c. 6500 and 3000 BC is quite specific, so the patterns of similarity and difference between regions, and the evidence for the spread of innovation is suggestive of a constantly developing and changing dynamic, where there is a close relationship between the landscape, mobility and interaction between peoples across that landscape, and the recurring patterns of stylistic distribution.

If we accept that material culture is a maker of culture (Hodder 1982, 1986), then it is relatively straightforward to understand the existence of relationships between material culture and the signalling of social boundaries (Wobst 1977; Jones 1997; David and Kramer 2001; Wright 2002). Gosselain (1999, 2000) and Stark et al. (2000) have discussed the relationship between ceramic technology and social boundaries, with Gosselain drawing specific attention to the relationship between specific *chaînes opératoire* and linguistic distribution. In the southern Iran context, we are faced with a significant degree of commonality in approaches to ceramic production technology over protracted periods of time, which is evident across a wide area. However, there is also evidence of a tendency towards increased regional elaboration of decorative motifs. It is entirely possible that there might be distinctive local variations in approaches to ceramic production and distinctive *chaînes opératoire* that have not yet been identified. Detailed studies focussing on identifying variation in the density of the fabric, the size and density of the chaff temper and the degree of surface finishing, etc. might be enlightening, but until such analyses are completed, we can only confirm that this material belongs to overarching potting traditions. Nonetheless, the evidence that there were stylistic innovations that led to the production of regionally distinct decorative styles after the dispersal of the new technological innovations affirms that in some situations, there were socio-cultural mechanisms in place that necessitated the creation of small-scale stylistic innovations in order to produce regionally distinct material culture. Taken together, this indicates that although there may well have been various boundaries and frontiers at different times during prehistory, these had a degree of permeability. It is important to remember that these processes were in the hands of the potters, i.e. those who produced this material. At one level, it seems clear that the populations of each region of southern Iran possessed a robust sense of cultural identity that was reflected in the material culture being produced by their potters. Although the potters of each region were susceptible to technological conservatism, they were at the same time capable of and open to innovation.

Acknowledgements This paper originated as an overlong presentation at the *Workshop on Early Iranian Metallurgy* held at the Department of Archaeology, University of Nottingham, from 19 to 21 September 2007. It was also partially inspired by conversations with Prof. Andrew Sherratt who constantly challenged me to think of the bigger picture. This version has had a slightly tangled gestation, and I would like to thank the editors of this volume for their patience and excellent advice. I would again, as always, like to thank Prof. D.T. Potts and particularly Dr. Lloyd Weeks for stern comments and sage advice. I would also like to thank Drs. Sheila Kohring and Chris

Thornton for their pointed and useful observations and Prof. C.C. Lamberg-Karlovsky for extensive and insightful comments that drew attention to a number of gaps that have hopefully now been filled. Although those who have read this paper have tried to temper the interpretations and opinions expressed, any errors or misconceptions remain those of the author.

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Chapter 9
Culture, Tradition and the Settlement Burials of the *Linearbandkeramik* (LBK) Culture

Daniela Hofmann and Penny Bickle

The Problem

"Culture" has had a rough time recently. It has been denounced by archaeologists and anthropologists alike, either because it has been simplistically opposed to nature (e.g. Thomas 1996, 13–15; Ingold 2000, 29–31), or, more importantly for this paper, because it creates false expectations of uniformity or cultural authenticity in a group's social life. For instance, Clifford's (1988, 10) definition of culture as "a deeply compromised idea I cannot yet do without" is followed by an eloquent challenge to the view that links culture to tradition, persistence and collectivity and opposes it to art, history and the particular. The normal state of culture, it is argued, is to be contested, to have permeable boundaries, and to never stand still. In the messiness of daily existence, where different interest groups with shifting memberships appropriate and strategically deploy symbols, it seems overly abstract to speak of a unity of meaning or purpose (e.g. Kuper 1999, 121; Barnard and Spencer 1996, 141; Ingold 1994, 330; Turner 1993).

However, in spite of these vitriolic attacks, culture has refused to go away. This is as true for archaeology as it is for anthropology. For the latter, Sahlins (1999, 2000) has repeatedly come to the defence of culture, characterising it as a set of shared understandings which make social action possible. Culture furnishes the conventional categories and concepts which are then made actual and referential in the course of the situated actions of people (Sahlins 2000, 283–91; see also Giddens 1984). This allows ample room for different perceptions, but "not everything in the contest is contested" (Sahlins 2000, 488) – there must be a minimal shared basis of mutual intelligibility for "contestation" to work. To paraphrase Ingold (1994, 330), people may not live in bounded cultures, but they still live culturally, they navigate their way through the world in a specific style. Culture lives in the actions of its participants, not in a set of abstract rules that can be challenged at will. It is because

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B.W. Roberts and M. Vander Linden (eds.), *Investigating Archaeological Cultures: Material Culture, Variability, and Transmission*, DOI 10.1007/978-1-4419-6970-5_9,

of this interpenetration between shared practice and novelty that change and a certain fuzziness of boundaries are part and parcel of a culture, without this implying a total lack of coherence (Sahlins 2000, 290; see also Rosaldo 1989).

This is a rather selective glimpse of a vast anthropological discussion, but it shows that the concept of culture there, at least, is not yet obsolete. It is not some universal and abstract standard of behaviour and more of a pool of resources that is fluid, but not limitless. It is the set of shared categories which enable meaningful action, and can be altered as it becomes implicated in specific projects. With its focus on instantiation in specific, materially grounded actions, this definition of culture could be made to work in archaeology. Yet in our experience at least, this is not the way the culture concept has been employed.

The following paper introduces the way culture has been discussed in our chosen case study, the Linearbandkeramik (LBK; c. 5600–4900 cal BC; Fig. 9.1), the first Neolithic culture over large areas of Central and Western Europe. Here, culture is often used as an abstract benchmark against which certain kinds of practices can be compared, generally unfavourably. In the long run, this has perpetuated the interpretation of the LBK as a somewhat static and unproblematic entity, internally coherent and with clearly defined beginnings and ends. Using settlement burials from two LBK regions, Lower Bavaria and the Paris Basin, we argue that to classify such practices as low status or marginal is to miss their impact in the communities in which they are carried out. However, burial practices like any other form of social action are not mechanically reproduced according to static codes and their salience to the investigation of culture lies in the way LBK settlement burials speak to both broader traditions and local practices. While drawing from a shared set of possible forms of expression, the burials are made to matter at an intimate social scale, which introduces variation and local trajectories. It is only once we come to terms with this fact that we can begin to rethink how culture can retain interpretative significance in the kinds of archaeologies we are trying to write.

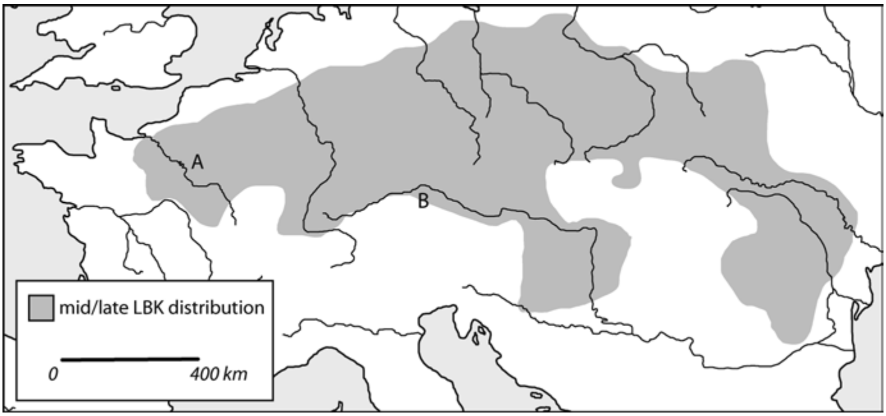

Fig. 9.1 Distribution of the LBK across Europe. Case study areas are (**A**) the Paris Basin; (**B**) Lower Bavaria (after Jeunesse 1997, 10)

Archaeological Cultures: Performing the LBK

The LBK is perhaps the classic archaeological culture, as its material repertoire consists of a certain style of houses, economy, burial, pottery, stone tools and so on "constantly recurring together" (Childe 1929, v–vi). Although it is generally accepted that in its later phases, the LBK becomes increasingly regionalised (cf. Modderman 1988; Gronenborn 1999; Sommer 2001), this phenomenon has still effortlessly been subsumed in universalising narratives. The LBK begins as very homogenous in its earliest phase (e.g. Sommer 2001) and then progressively fragments, giving rise to the geographically more circumscribed cultures of the Middle Neolithic. As a general trajectory, this is valid everywhere in the LBK. There is little discussion of how, or even whether, this process would have been perceived and evaluated on the ground by the individuals and communities involved. For this reason, narratives derived from one area of the LBK, be they about the symbolic dimensions of the house (Bradley 2001), personhood and the body (Jones 2005) or the violent end of the LBK in the face of climatic instability (Golitko and Keeley 2006; Gronenborn 2007), are assumed to be valid throughout. Therefore, while it seems we can deal with differences in material culture as a classificatory tool, we are less good at coping with difference in historical trajectories of change.

As a result, "LBK culture" has increasingly become something almost metaphysical. Somewhere, there is an ideal LBK pot, or house, or burial against which regionalisation or chronological change can be defined as a deviation. This ideal material does not exist, yet it exerts considerable power. It is used to marginalise some areas or practices, to construe them as somehow out of line. Often, this is combined with a focus on "big questions", such as the Mesolithic–Neolithic transition, where it becomes crucial to identify just how "real LBK" a given practice is (for a critique, see Robb and Miracle 2007).

This can, for instance, be seen in the ways in which two ceramic types contemporary to the LBK, La Hoguette and Limburg, are utilised in the discussion of the transition. These ceramic traditions are known almost exclusively from their presence on LBK sites (or entirely so in the case of Limburg) (Constantin 1985; Jeunesse 1987, 2000; Lüning et al. 1989; van Berg 1990; Constantin and Blanchet 1998; Manen and Mazurie de Keroualin 2003). Considered as representative of terminal Mesolithic groups by virtue of their difference from LBK ceramics, when these pots are found they remain resolutely separated from the rest of the LBK assemblage in the archaeological report (see also Thomas 1996, 114). Similarly, the presence of wild animals on LBK sites continues to be regarded as a transitional practice or a Mesolithic throwback. Thus, hunters are considered to have a different identity and a lower status compared to the more LBK herders (Hachem 2000). This is seen as part of a long-term tension, resolved only in later Neolithic contexts when hunting is finally seen to give way to herding and to retain only a symbolic significance (Sidéra 2000; Tresset 2005). In the case of recent isotopic studies, non-locals in burial assemblages have sometimes been identified as hunter-gatherers, an interpretation then hypothetically confirmed through the accompanying grave goods (Price et al. 2001; Bentley et al. 2002, 2003; Price and Bentley 2005; see also Bickle and Hofmann 2007).

The difficulty with this approach is that variations in the archaeological assemblages become deviations from an imagined norm, which are reified as either regional or inauthentic cultural practices. Narratives of the Mesolithic–Neolithic transition are thus reduced to explaining what particular patterns of material represent, with some aspects of the material world being seen as more informative than others. It is as a reaction to these kinds of narratives that, in our research so far, we have chosen an alternative focus, writing mainly about the construction of identities and communities in daily practice and intimate settings (Hofmann 2006; Bickle 2008; Bickle and Hofmann 2009). It seemed easier in those instances to trace the specific histories of the "multi-tradition communities" (Gronenborn 2007, 84; see also Zvelebil 2004; Whittle 1996, 2003) that are now increasingly seen to characterise the LBK. Writing about the small-scale meant paying attention to difference and valuing it.

In these kinds of narratives, archaeological traces should not be seen as a passive reflection in the material world of an idealised culture existing only in Neolithic people's heads. Rather, as Barrett (2001, 156) argues, material remains take a far more active role in the constitution of past societies, providing "the material condition which necessarily and actively facilitated certain strategies of social practice". This is to encounter the material remains of the past through how it is inhabited, or in our terminology, performed. The notion of performance as understood here is largely founded on Bourdieu's (1977, 1990) practice-based approaches to social life.

Practice and performance are an essential part of Bourdieu's (1990, 96–7) conception of the *habitus*, in which physical action in the world is not a mere "execution" as if performing a character from a play, but rather it is "that active presence in the world … which directly commands words and deeds without ever deploying [the performance] as a spectacle". This means that, rather than *habitus* being the rules within which communities live, creating the boundaries of social possibilities, it is the framework which enables action in the world. Therefore, performance is at once both the producer and regulator of discourse in the world (Butler 1993; for archaeological discussions, see Meskell 1996; Pearson and Shanks 2001). While these discussions show a convergence with some of the anthropological arguments rehearsed above, their application to the LBK specifically remains limited.

The challenge is therefore to address the role of bodily remembered practices in carrying forward the performances which form LBK daily life, ultimately creating our archaeological entities. In this kind of framework, LBK materials are not a direct record of either a perfect or imperfect performance of LBK culture, but rather the contexts in which life occurred. We need to consider how the assembled evidence facilitated the continuation of social relationships and led to regionally diverging trajectories in how material culture was employed. The focus of this paper, therefore, is the tension between the existence of culture as similarity of action within a social grouping and the material remains which constitute our archaeological knowledge base. We expect differences and similarities in the associations of practice and material objects but the challenging and interesting questions lie in the different social actions and mentalities which led to their creation. For this purpose, we focus on the interpretation of burial practices.

The Dead on Settlements

The classic LBK burial rite is inhumation in cemeteries with a specific range of grave goods, including stone tools, pots and shell beads (Jeunesse 1997). This remains the benchmark against which other kinds of burial, for instance, interment on settlements, cremation or fragmentation and secondary burial, can be compared. For cemeteries, the presence of grave goods and the normative tendencies to choose a specific position and orientation for the deceased (crouched on the left side with the head to the east) are generally interpreted as a sign of piety and care. Other kinds of burials are defined by the lack of one or more of these attributes and hence valued negatively (e.g. Veit 1992, 1996; Jeunesse 1997; Lüning 1997). Repetition, it seems, shows care while variation implies a lack of it. Again, the terms of this discussion encourage the definition of culture-wide norms.

Using examples from Lower Bavaria and the Paris Basin (Fig. 9.1), we wish to examine further the variations observable even within each region, let alone across the whole of the LBK distribution. We are explicitly focusing on settlement burials, partly to challenge the idea that they are the graves of the unimportant dead, but similar points could also be made in an investigation of cemeteries (see e.g. Hofmann 2009, 222–23). It is our aim to explore the specific meshing of the "LBK" as a widely shared perspective on the world with small-scale, face-to-face engagement of a specific set of people in the world.

Double Burials as a Local Tradition at Otzing

The largest number of settlement burials from a single site in Lower Bavaria comes from the mid to late LBK settlement at Otzing near Deggendorf. Rescue excavations uncovered 45 burials scattered between roughly 30 house plans (Schmotz 2000, 2002; Schmotz and Weber 2000). Few of the burials can be assigned to a particular building. Many are located at roughly equal distance between two houses, others are loosely scattered on free spaces between buildings. There is also a tighter cluster of seven badly preserved inhumations near the north end of the site. Schmotz (2002, 267) mentions two isolated skulls, but gives no further detail.

On one level, the interments at Otzing correspond to the general characteristics identified as typical for LBK settlement burials (cf. Veit 1996; Orschiedt 1998). Many of the pits containing burials are general refuse pits, and many of the deceased receive few or no grave goods. This is especially true for children and juveniles who, in line with *archaeologists*' expectations, constitute the majority of burials (25). The bias towards female burials identified on other sites (Veit 1996) is, however, not repeated here (Schmotz and Weber 2000). The position and orientation of bodies is also less standardised than on cemetery sites. Moreover, there is a particularly high incidence of double burials, and these form the focus of discussion here.

Double inhumations can occur on cemeteries (cf. Peschel 1992), but are generally more common among settlement burials. At Otzing, their proportion is even higher than usual, and this can form the starting point for drawing out performative links and contrasts. This is all the more pertinent since double burials have in the past been interpreted as merely a labour-saving device to dispose of the least important members of a community, mostly children (Veit 1996, 204). This makes sense within the general and rather abstract LBK-wide models of status and prestige presented above, but it can be challenged when we focus down to the tableaux created in the course of the rites and on the performances occasioned by these deaths. This can reveal a much subtler interplay between wider norms and local traditions.

One striking contrast at Otzing is between burials whose occupants are facing away from each other and those which share the same orientation. The resulting picture is quite different in each case, and we may speculate that the relationships that existed between the deceased may be responsible for this. For example, the grave of a mature person and small child, probably both female, gives a cramped impression (Fig. 9.2). Although there would have been ample room for the girl to the right of the older woman, their bodies were not arranged side by side. Rather, the woman's head has been squeezed tightly against the edge of the cut and her legs have been folded back onto her thighs to create room for the girl. The girl is even more tightly crouched and is facing away from the woman, even avoiding touching her knees. Thus, while the bodies share the same grave pit, direct physical contact seems deliberately minimised.

Grave 19, containing two children, gives a very different impression (Fig. 9.3). The two bodies are not only buried in the same position and facing in the same direction, but the older child is also embracing the younger, suggesting a relationship of intimacy or even tenderness. This arrangement is also observed in the few double burials from Lower Bavarian cemeteries, such as Aiterhofen and Sengkofen (Nieszery 1995). Hence, only one of the possible variations on double burials evidenced at settlements was replicated in cemeteries. Rather than a complete contrast between the two contexts, we can perhaps suggest a focus on more stereotypical practices in cemeteries, perhaps linked to a different, wider audience present at the time of burial.

Otzing's grave 27 again drives home the point of variability on settlement sites. The two children buried here lie on the sherds of a smashed coarse ware pot. Their heads are in opposite directions, but their legs overlap, creating tension between the intimacy of touch and the antithetical positioning. The closest parallel comes not from another double burial, but from the sequential interment of two children in the same pit complex, in close proximity, but with their heads facing north and south, respectively.

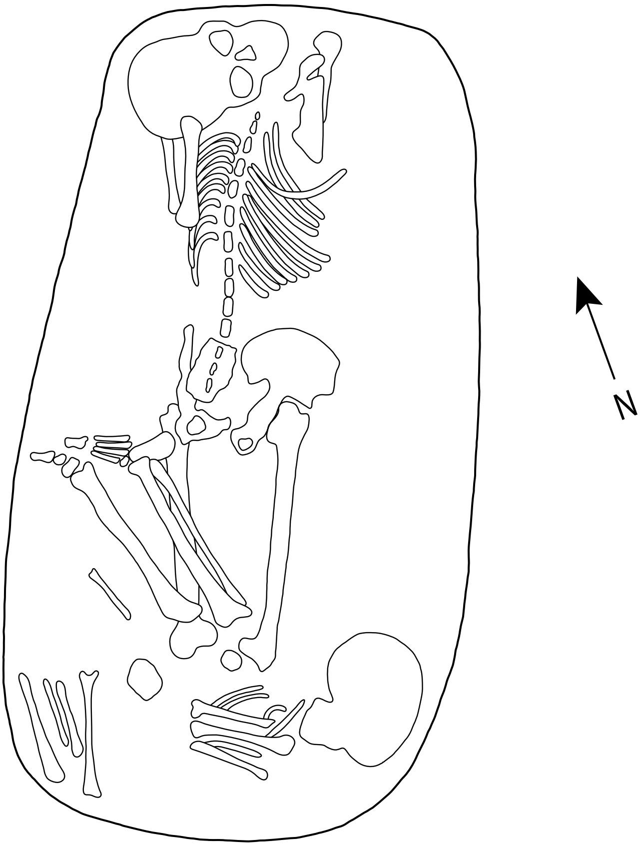

Fig. 9.2 Otzing, grave 10: double burial of an older adult woman and child (after Schmotz and Weber 2000, 29)

Graves 22 and 29 may show evidence of later manipulation, which is also implied by the isolated skulls reported from Otzing (see Schmotz and Weber 2000, 25). The two older children/juveniles in grave 29, for instance, were buried successively in irregular positions. It is not clear how much time elapsed between the two interments, but the first burial may have been disturbed by the second, resulting in the displacement of the head and the removal of the arms. It seems unlikely that this is solely due to the rescue conditions of the excavation. The meaning of juxtaposing the two bodies in this way, at almost right angles, is unclear, but may well dramatise the specific circumstances of the deaths or a particular relationship.

The differences observed between these burials militate against a single explanation, such as carelessness or labour-saving devices. What we are seeing is a set of practices – including the positioning and orientation of two bodies relative to each other, the selection of a specific spot on the site and the potential for further manipulation at a later date – being selectively deployed on different occasions. Idealised versions of relationships or more idiosyncratic dramatisations can both occur.

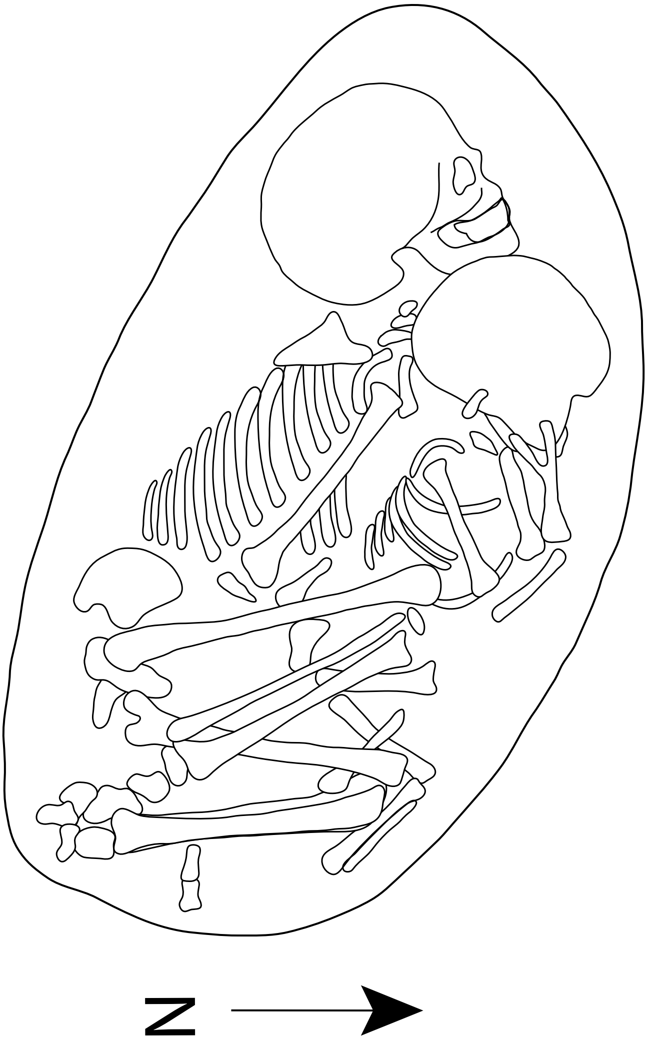

Fig. 9.3 Otzing, grave 19: double burial of two children (after Schmotz and Weber 2000, 29)

We can even begin to discuss the possibility of local traditions, although ideally this would require more detail on the relative sequence of the burials and the overall duration of the site. In contrast to nearby cemeteries, such as Aiterhofen (Nieszery 1995), the burials at Otzing form few distinct clusters or groupings, and none of a size comparable to burial grounds. We hence cannot really apply the idea of family groups returning to specific plots (cf. Nieszery 1995, 66). Yet, graves reference each other in subtler ways, through tableaux and practices. How individuals are positioned relative to each other, for instance, links graves from different parts of the site: children in antithetical orientations, bodies arranged at right angles or parallel to each other provide recurrent choices. The practice of manipulation is again relatively frequent.

It seems likely, then, that specific performances and dramatisations were remembered, perhaps keenly for a while, and these provided the template against which other rites were performed. In contrast to cemeteries, places set aside for the dead, the remembrance of settlement burials would rely on encountering grave sites in the course of everyday routines and on the repeated choice of certain elements of performance. This meshing of significant places and actions means that, while individual settlement burials may have been forgotten over time, the community at Otzing could develop a micro-tradition in which some practices were considered more effective and appropriate, and were hence repeated more often. It is these idiosyncratic and unquantifiable factors that result in the observed pattern of burials being at once similar to LBK-wide norms – for instance, in demographic composition or provision with grave goods – and at the same time different, for example, in the frequency of double inhumations (see also Sangmeister 1999). The burials at Otzing are a unique set of theatralisations designed to cope with specific, emotionally charged events. They draw on a certain pool of practices, but to judge them by how well they conform to static norms is to miss the point of their embeddedness in a specific local sequence.

The Performance and Context of Child Burials in the Paris Basin

In the case of the Paris Basin, a number of significant differences in the context of burial and the associated rites can be identified (Jeunesse 1997; Constantin and Blanchet 1998; Constantin et al. 2003; Pariat 2007). Inhumation in cemeteries was not practised and the gendered division in grave goods was not as strongly marked; instead, burials are found in settlement contexts, and there is a strong sense of performance associated with the time of interment (Bickle 2008).

Furthermore, the placing of child burials close to longhouses, a highly varied aspect of inhumation rites across the LBK, is also found in the Paris Basin (Veit 1996). This practice has often been commented on (Veit 1996; Whittle 1996; Jeunesse 1997; Bradley 2001; Constantin et al. 2003; Jones 2005; Pariat 2007), though it is usually discussed away from the context of the longhouse. Bradley (2001, 53) has attempted a connection between burials and architecture. However, the lack of detailed consideration of the actual place of burial around the house, the demographic variability of the persons thus treated in different areas of the LBK and the different practices that constituted an inhumation has led to an overly broad connection between some of the dead and architecture, which in this form does not hold true for the whole of the LBK.

The problem with this approach to the archaeology is that such practices become homogenised as one particular category of evidence. Rather than comparing the child burials to an idealised form of burial, it is far more productive to think about their context in the settlement and the performances associated with the moment of interment. For instance, child burials in the Paris Basin are actually very varied. In two cases, at Berry-au-Bac, *Le Chemin de la Pêcherie* and Cuiry-lès-Chaudardes,

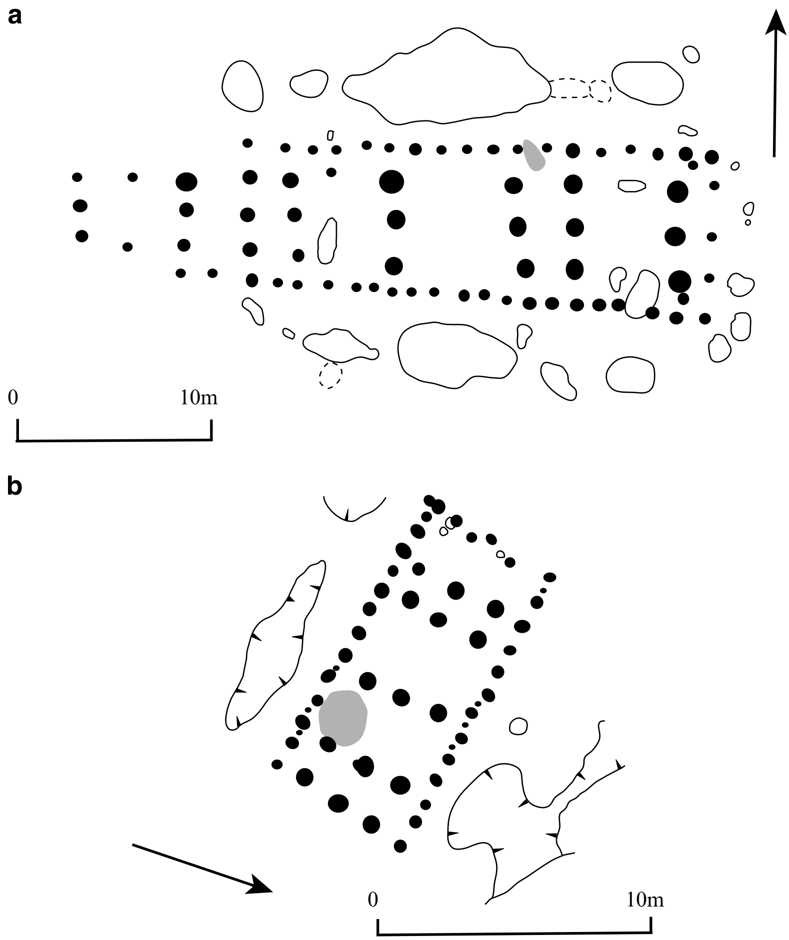

Fig. 9.4 The child burials (in grey) found inside longhouses in the Paris Basin. (**a**) Burial 308 in house 300 from Berry-au-Bac *Le Chemin de la Pêcherie*, Aisne (after Dubouloz et al. 1995, 29). (**b**) Burial 315 in house 330 from Cuiry-lès-Chaudardes, Aisne (after Ilett et al. 1980, 32)

Les Fontinettes, the burials were placed in pits inside the houses (Fig. 9.4; Farruggia and Guichard 1995; Ilett et al. 1980). There have been suggestions that child burials may have been placed in the loam pits next to houses, which also received waste from daily life at the settlement, because they were of little value or were given little attention in burial (Jeunesse 1997, 98). This assumption has been made partly because they have received far fewer grave goods than adult burials, but this lack of grave goods conceals the significant effort that goes into child burials. Frequently, burials have their own grave cut and even when placed in the loam pit, they are in an area apparently set aside. For example, the child interred in the northern loam pit of house 245 at Cuiry-lès-Chaudardes *Les Fontinettes* is provided with its own area, which is prepared for the burial by the sprinkling of ochre on the bowl of the cut (see Fig. 9.5; Coudart and Plateaux 1978). Each burial, therefore, had its own particular location around the house, whether inside, by the walls or in the loam pits.

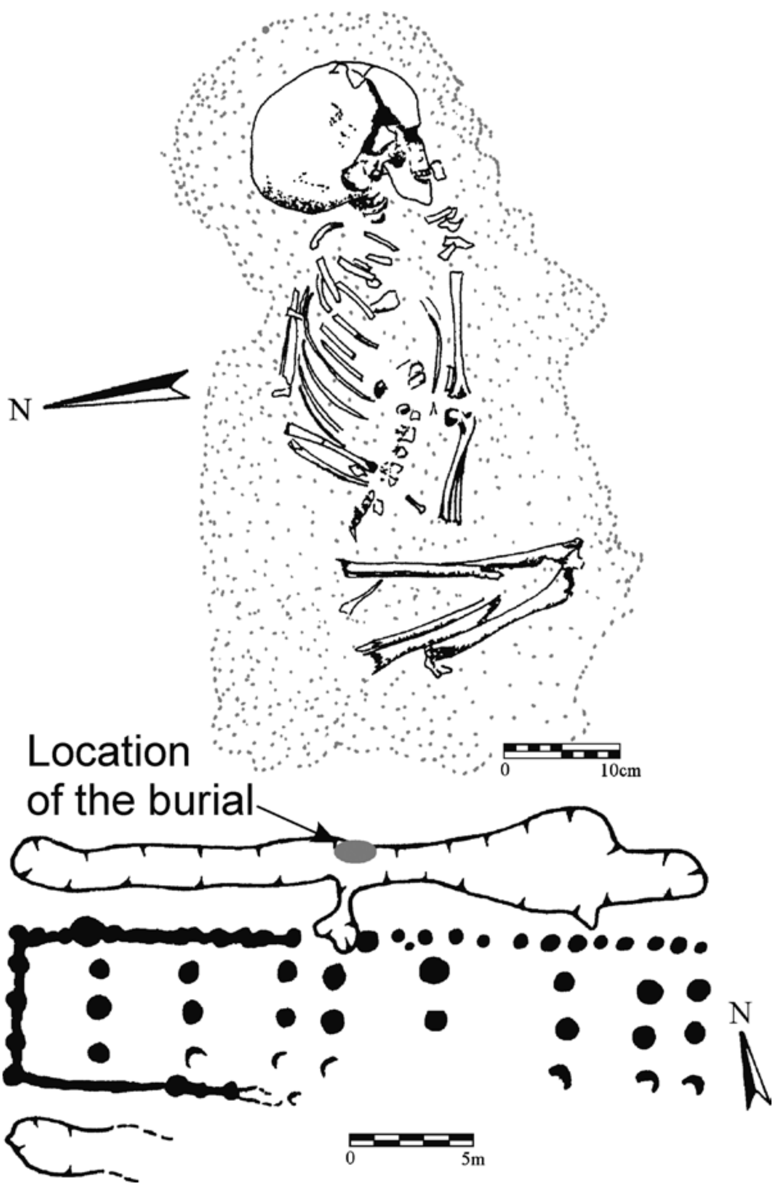

Fig. 9.5 Burial 271 from the northern loam pit of house 245. The grey shading around the skeleton indicates the presence of ochre (after Soudský et al. 1982, 75)

Not only were the burials given a particular space in the settlement, but the rite of burial may have been fairly dramatic. The natural soil into which the burials were placed is alluvial silt and frequently creamy white or yellow in colour (Ilett et al. 1982; Chartier 1991). Therefore, the presence of reddy orange ochre would have stood out particularly well, distinguishing the space of the burial from the rest of the soil. Burials are also occasionally furnished with beads, which were frequently white (or grey) in colour, as they were made from limestone, shell (including *Spondylus*) and bone (Jeunesse 1997; Constantin et al. 2003; Bonnardin 2003). These colours may have metaphorically stood for bodily fluids (such as blood or semen) or, through the associations of particular colours, drawn on complex relations between material substances and the body of the deceased (Borić 2002, 39; Jones and MacGregor 2002, 11), thus playing a significant part in the range of possible performances at the grave side.

The particular efficacy of this event is local, immediate and within the knowledge of those who threw ochre, placed the body in the grave cut or stood and watched. However, these rites were not repeated every time, but rather were part of the possibilities present when each burial was made. Therefore, the household or the community chose the appropriate place for the deceased, made time and space in the daily round and chose to follow or ignore tradition. The implication is that each burial is not an impartial representation of social order or culture, but a place in time and space in which emotion, memory and intention meshed together with the expectations of childhood in the Paris Basin.

The onus on the archaeologist is not to explain this particular practice as a means of identifying the extent to which communities in the Paris Basin conformed to general LBK rules, but rather to explore how these practices were inhabited (Barrett 2001). With this approach, the connections between child burials and architecture become more interesting. Bradley (2001, 53) has previously suggested that the presence of child burials by houses may imply a link between houses and the dead. However, rather than simply arguing that houses represent the ancestors, Bradley (2001) implies that they are part of a connected world-view in which the orientation of burials and houses forms an orientation for LBK life on its origins, built around the direction along which the first farmers migrated out of central Europe. The discussion of the child burials above can now elaborate on this point, illustrating that childhood may have been in some way tied into the architectural space of the house and the practices of building and using longhouses. The longhouse would have provided a particular forum for daily life and the formation of social relationships; the mediation of death in this setting may have evoked the solidarity of community in the space of the settlement. However, even within the Paris Basin this is subject to manipulation and creative responses, in which it would be difficult to define an essential practice that could be identified as meaning one thing or representing one identity.

These creative responses to the interplay of social relationships and architecture will have had a considerable temporal dimension at the settlement. LBK longhouses are generally considered to have lasted for just 20–30 years or one generation, with abandoned houses left to decay *in situ* (Coudart 1998; Last 1996; Whittle 1996; but see Rück 2009, 179–80). Settlements were thus composed of tangible material reminders of past generations that could be engaged with on a daily basis. In this sense, time was "thick" (Borić 2003, 48) at all LBK sites, but the responses to such an engagement would have been tempered by the shared memories held by the community. These have the potential to have been both oral and material (Bloch 1998, 109).

Harrison's (2004) study on the relationship between former Aboriginal inhabitants of the settlement of Dennawan and its archaeological remains focuses on the relationships between shared memories and the interactions between people and objects. Specifically, Harrison (2004, 199–200) emphasises the importance of making physical contact with the site during visits through touch, which inspires particular emotions and physical responses. Thus, Harrison (2004, 214) states that "such memories materialise only with re-enactment" as individuals tell stories in reaction to their bodily engagement with the site. Burial near houses would have drawn upon such acts of collective rememberings, building local narratives around the house. These, as much as any perceived rule, may have encouraged the repetition of particular ways of doing things. The striking association between children and pits very near or in the house, which is not repeated in all areas of the LBK (see Hofmann 2009, 222), is the product of recurrent practices that had come to make sense locally, built up through the micro-chronology of individual episodes of grief, burial and commemoration. Small-scale and intimate, each child burial would have blended living memory and tradition together. Therefore, the social interactions around longhouses were not passive representations of a single LBK identity, but rather a mediation of the complex interplay of daily life, memory and identity, together building up the time depth of settlements and their specific biographies.

Conclusion: Anchoring Culture in the Local

Looking at different aspects of funerary rites in different regions blurs the associations between different identities and burial practices. It shows overlapping, but also diverging trends within LBK communities, both among groups at the different ends of its distribution and those living in the same place (Hofmann 2006, 2009; Bickle 2008). The study of burials is, therefore, at its most interesting and productive when it is considered as part of the formation of various scales of identity, community and temporality at the settlement. Social life is a complex interaction between people, materials and environment, and we only do justice to these patterns when the boundaries between different categories of evidence are viewed as permeable and variety in practice is allowed visibility in the archaeological dialogue. The apparent orthodoxy of the LBK is, therefore, undermined by close and detailed attention to its archaeological remains. If a united LBK is assumed, then the variations become problematic and require considerable explanation by us before we have even begun to ask questions of LBK life itself. However, this is a problem of our own making: we have mistakenly assumed that unity in human behaviour is produced as a result of fixed cultural rules (Bourdieu 2002).

Modderman's (1988) conclusion that the LBK was characterised by "diversity in uniformity" manages to capture some of the qualities of LBK traditions. Both at Otzing and in the Paris Basin, burial practices were guided by an interplay of variation and more widely held ideas. Working within and upon the traditions provided by shared living, communities in the LBK were not passively repeating static identities or senses of belonging. The act of sprinkling ochre before the burial in the Paris Basin or smashing pots at Otzing were events caught up in the mediation of appropriate ways of acting and feeling. In this sense, the creation of tradition comes about through collective memory work by a group of people.

Remembering is not a solitary activity (Middleton and Edwards 1990). A relation to the past is given in the participation in recurrent practices, as well as in more formal instances of recollection. Both are rooted in a wider field of interaction, which influences the content, context and occasion of remembering, drawing out some aspects as central to the identity and integrity of a community (Middleton and Edwards 1990, 10–17). Linked to daily practice and to dialogue with others, remembering is partial and subject to change. It is here that tradition is transformed, whether accidentally (Mizoguchi 1993) or through selectively emphasising some aspects at the expense of others. This link with practice also accords objects and places a crucial part in grounding memory work in daily experience and investing it with emotional salience (e.g. Radley 1990; Küchler 1987, 1993; Battaglia 1990, 186). Therefore, traditions are not just a repetitive representation, but an open-ended "practice of remembering" (Ingold 2000, 148), significant at various levels of social interaction. These practices selectively draw upon shared items of material culture or ideas of appropriateness to play out specific instances of situated actions.

For us, the interest in studying culture hence lies in the way in which certain kinds of materials and their deployment in practice create something akin to a pool of resources, which are in turn drawn upon and transformed in specific instances. The significance and emotional salience of these materials and practices is of necessity local, but as a medium of expression they are more widely shared. In contrast, the material definition so often adopted for culture leads to a system of strict rules, which then limits the archaeologist to focus on the explanation of difference. This approach has diverted attention away from the significant questions of how the LBK way of life found coherence within both the local scale and the widely shared network and how different scales of social action can best be meshed in our accounts of the past.

Admittedly, and partly as a consequence of these limiting research priorities, the ways in which "shared pools of resources" could have been created are so far rather vague. For the LBK, we have shown that it is at the local level of this network that the *habitus* or "LBK world-view" must first be addressed. The insights provided by localised case studies, however, go beyond the local, as broadly shared practices find their meaning at this scale. It is here that we feel research and theoretical effort in LBK studies should concentrate. We must get beyond using culture as a divisive entity for the classification and evaluation of practices, a tool to measure conformity, and come to terms with its messy involvement at various social scales.

Culture should become an enabling concept in our narratives, a way to discuss networks, connections and similarities between specific projects and practices carried out at different times and places. In this guise, the concept of culture can become once more a challenging and fruitful starting point in the more nuanced archaeologies we seek to write.

Acknowledgements We would like to thank the editors for inviting us to write this paper and for their comments and suggestions. Thanks also to Stevie Miller and Ian Dennis for their help with Figs. 9.2 and 9.3.

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Chapter 10
Constructing Social and Cultural Identities in the Bronze Age

Kristian Kristiansen

Introduction

The Bronze Age has sometimes been presented as the first period of 'globalisation' or 'world system' in Europe. I propose that in order to apply such terms onto the past, we first need to understand the meaning of culture and how it is constituted. I wish to propose that the concept of culture has been employed in two different ways in archeology: from 1860s to 1960s, culture was predominantly used in an instrumental way, as a means to classify the past in time and space. Typology was the method. As there existed no theory on the meaning of culture, early attempts to equate culture and people were flawed, as we know.

Ian Hodder and post-processual archeology introduced a new understanding of culture. Here, culture is socially and symbolically constructed and, therefore, carries meaning. This may be linked to a variety of social traditions, from ethnicity to cosmology (Hodder 1982). This approach was taken one step further by Thomas Larsson and myself when we suggested that a recurring set of material symbols may form a symbolic field that corresponds to an institution (Kristiansen and Larsson 2005, Chap. 1). I apply this approach in the following in order to demonstrate how social identities were constructed by selectively using material culture to define different institutions with different roles chiefly among males during the period 1500–1100 BC in northern Europe.

My paper is organised around two dialectic relationships: between material culture and materiality, and between social and cultural identity. I propose that it is only by linking the two that a more complete, historical understanding of the role of material culture can be achieved.

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B.W. Roberts and M. Vander Linden (eds.), *Investigating Archaeological Cultures: Material Culture, Variability, and Transmission*, DOI 10.1007/978-1-4419-6970-5_10,

From Material Culture to Materiality

In prehistoric and pre-state societies, there exists an intimate relationship between people and things, as expressed 25 years ago by Ian Hodder in saying that culture was meaningfully constituted. Later social anthropologists, such as Alfred Gell and Marilyn Strathern, explored this relationship and these insights from anthropology have gradually been taken onboard by archeologists. It is proposed here, with reference to Marilyn Strathern's and Alfred Gell's works, that materiality embodies a form of personification of material culture (Gell 1998; Strathern 1992; see also Tilley 1999). It is often derived from ritualised and sometimes divine relations between gods and humans; humans and nature; and humans and animals, where material culture acts as an intermediary that encapsulates and symbolises supernatural properties. In this way, specific objects, such as the images and symbols of gods, can be empowered through various forms of rituals. They attain what Gell called secondary agency and are infused with supernatural power and personal properties that respond to human actions. Such power may also be acquired by certain forms of prestige goods through their links to outstanding individuals such as chiefs, warriors, or priests, and the deeds they performed with the objects (e.g., famous swords, the kula rings, and shells). The consequence is that the objects become loaded with personal biographies and names (e.g., Appadurai 1986; Strathern 1992; Kristiansen 2002). Through this process, things and persons create each other and become one, and therefore, the exchange of gifts also becomes partially personal, as has been argued by Strathern (1992).

Based on this perspective, I propose that the context and the distribution of such highly powerful and personalised objects and monuments can inform us about social institutions, and the way they interacted in time and space. A case in point is the constitution in the Nordic Early Bronze Age of ritual chiefs who were characterised by a certain recurring set of objects and symbols, and warrior chiefs who had another set of recurring objects (Fig. 10.1).

The ritual chief is characterised by a special package of objects, such as campstools, and drinking vessels with sun symbols at the bottom, so that the sun would rise when lifting the cup that contained mead. Razors and tweezers are often linked to this group of ritual chiefs, which are defined by the exclusive use of spiral decoration which was the symbol of the sun cult and of Nordic identity. Their swords would often be full hilted and used for parade rather than for warfare. They are rarely sharp and rarely damaged (see Kristiansen 1984 for an empirical documentation of the use of different sword types).

The warrior chief, on the contrary, would have a highly functional, undecorated flange-hilted sword, an international type whose distribution stretched from south-central Europe to Scandinavia. It was the sword of the professional warrior, always sharp edged and often re-sharpened from damage in combat. The warrior chiefs would rarely have any of the ritualised symbolic objects of ritual chiefs, suggesting that they were not in charge of rituals. They shared with ritual chiefs a burial tradition in an oak coffin under a barrow, and a chiefly dress consisting of a cape and a

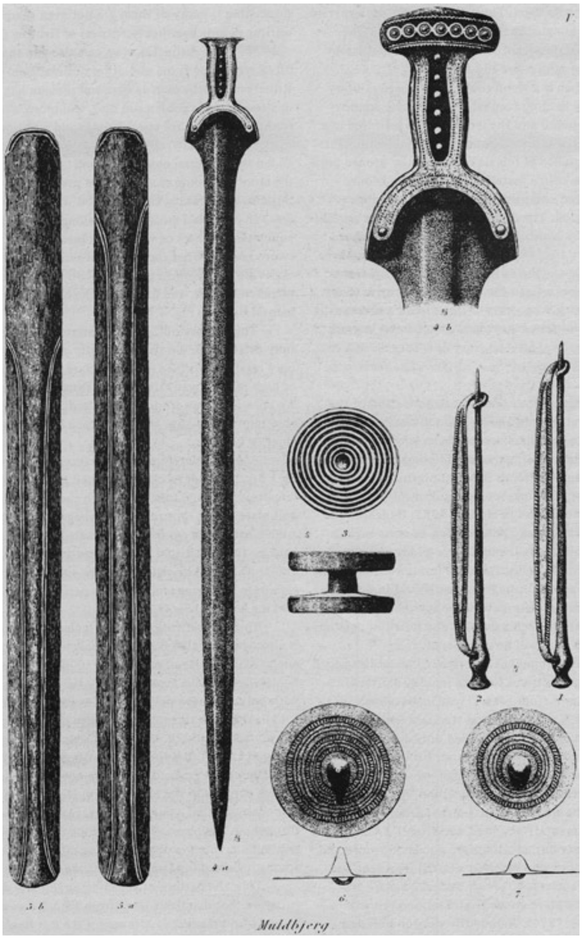

Fig. 10.1 Oak coffin burial of ritual chief with spiral decorated objects and a ritual sword of Nordic origin and a warrior chief with plain undecorated functional sword of international (non-Nordic) origin

round cap, both of which were socially distinctive of the free man of chiefly lineage. They also shared a burial tradition in a barrow, which is the corresponding ritual definition of 'free men' who owned cattle and farms, in opposition to those who had smaller houses without stalling for cattle (Kristiansen 2006).

Finally, we have a third group defined by octagonal hilted swords of south German origin, but which were also produced in Denmark by migrant smiths, as they employed a specific casting method different from the Nordic smiths (Quillfeldt 1995). Like the warriors, they do not have any of the paraphernalia of the ritual chiefs, and they share the same international distribution as the flange-hilted sword. They represent a group of people who might be linked to trade and smithing (Kristiansen and Larsson 2005).

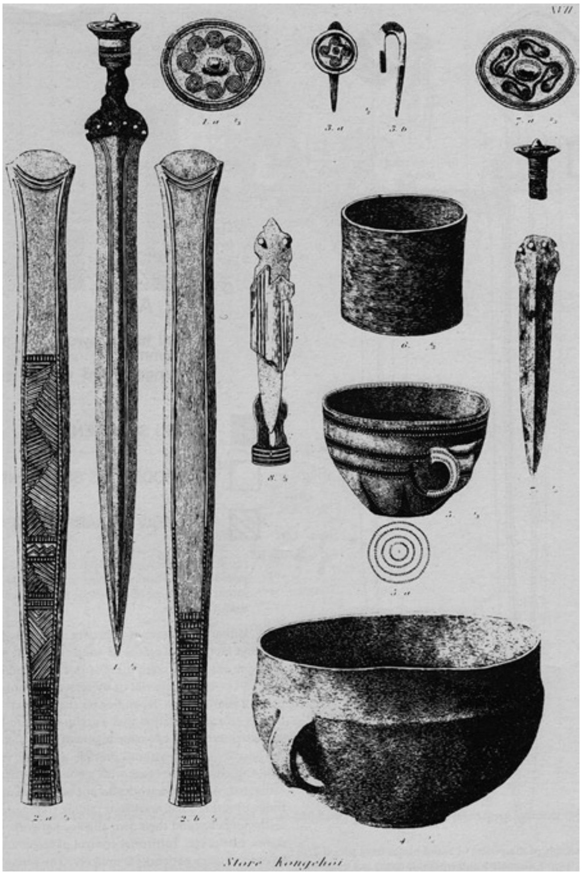

Fig. 10.1 (continued)

These three groups are represented by several hundred burials and serve as a prime example of Ian Hodder's dictum that material culture is meaningfully constituted. In a rather straightforward way, they demonstrate that different sword types in the Bronze Age were meaningfully linked to different social and ritual institutions and social identities. Hundreds of other object types from prehistory are waiting for a similar contextual interpretation of their social and institutional meaning.

Once we are able to delimit symbolical fields and their institutions, it becomes pertinent to raise the issues linked to the formation of social and personal identity such as personhood and agency. Here, a theoretical discourse from psychology and philosophy that examines personhood and embodiment meets with a theoretical discourse in anthropology and archeology that examines their social and cultural conditions (Strathern 1992; Gell 1998). The works of Michael Shanks and Paul Treherne are early examples of this theoretical trajectory that employed materiality to explore the cultural construction of body, the self, and their embodied praxis (Shanks 1999;

Treherne 1995). Other theoretical approaches to materiality were developed by Chris Tilley in *Metaphor and Material Culture* (Tilley 1999), and by Colin Renfrew, Elizabeth DeMarrais, and Chris Gosden in their edited book *Rethinking materiality: the engagement of mind with material world* (Renfrew et al. 2004). More recently, Joanna Brück has critically revised the concept of materiality and personhood and suggested that it was constructed through social relations and, therefore, cannot be equated with a modern perception of the individual (Brück 2006; see also Sørensen and Rebay 2008). However, such an embedded understanding of the self and social identity moves the interpretative focus to the meaning of these larger institutional and social relationships. This takes us on to the next level of analysis: the relationship between social institutions and cultural identity.

From Social to Cultural Identity

We have long since recognised the social complexity of prehistoric societies, but not the derived complexity of culture and its employment in producing and reproducing this complexity. While we are able to delimit social institutions by a contextualised analysis of their symbolic and cultural fields of meaning, such as that which defined ritual chiefs and warrior chiefs, the next step in the analysis is to move from institutions and the constitution of social and personal identities to cultural and ethnic identity. Did the symbolic fields of meaning that constituted the institutions of ritual chiefs and warrior chiefs also carry a wider collective meaning of identity? We are here encountering the relationship between the formation of the self through a social identity and its dialectical relationship with collective identities, from social groups/classes to polities/ethnicity. While ethnicity undoubtedly played a central role in all human societies as part of a common origin and shared historical identity creating a tradition, its material expressions have been an underdeveloped field of study (however, see Bürmeister and Müller-Schessel 2007; Fuhrholt 2008). I propose that it is possible to delimit various forms of social and ultimately ethnic identity through a careful analysis of the geographical distribution of social institutions and the symbolic meaning of their material culture.

Thus, the two institutions of ritual chiefs and warrior chiefs have radically different distributions, and this informs us about their different roles in the reproduction of a complex set of regional and inter-regional identities, some of which formed a collective ethnicity and some a political identity. The ritual chiefs maintained the ritual and cosmological order of society, defined by a symbolic package of objects and by the spiral decoration. It signaled Nordic identity and a shared religious cosmology, and probably also a shared cosmological origin. They were in charge of rituals and controlled the huge corpus of religious and legal texts vital to the correct performance of rituals and to the maintenance of order. Therefore, Nordic ritual chiefs never, or rarely, moved outside the cultural boundaries defining this 'ethnic' identity. I define ethnic identity here as a shared symbolic world of cosmological origin (Jones 1997). However, the Nordic identity displayed in the spiral style of chiefly objects refers back to a distant Mycenaean template of high culture that was not shared with other central European Bronze Age groups.

The warrior chiefs, on the contrary, were culturally defined as ‘foreign’, which allowed them to travel and maintain political connections outside the symbolically defined ethnic world of Nordic Culture. Therefore, they maintained and carried the inter-regional networks that constituted the flow of bronze and of foreign relations. They were part of a central European/north European international network, with a shared material culture of central European origin (Fig. 10.2).

Ethno-historical evidence of warrior cultures supports such an interpretation of warriors and traders on the move. Warriors often formed special group identities (sodalities) that linked them in a spatial network defined by rules of special behavior and etiquette. This could be employed both for recruiting war bands and for traveling to more distant chiefs to earn fame and foreign prestige good, as evidenced in Africa among the Masai, among the Japanese Samurai, and a recurring feature in the literature on warriors and warfare.

In this way, the institutions that existed took care of separate needs that were vital to Bronze Age societies: the internal maintenance of a shared cultural and cosmological world, and the external maintenance of political and commercial relations. Returning to the question of personhood and social identity, the sheer number of sword burials and the regularity they display in burial rituals and burial goods suggest that we are dealing with well-defined bounded institutions and social identities. Although small-scale variation exists, there is nothing to support Brück’s (2006) suggestion of a divided, relational personhood in the Bronze Age. Social relations were imperative, but they operated within a well-defined set of normative rules linked to the long-term reproduction of political institutions with their own blueprints for social actions and heroic deeds (Kristiansen 2008). We are far beyond a New Guinean perception of personhood and ‘dividuals’, whose relevance for any prehistoric period may indeed be questioned (Spriggs 2008).

It should also be observed that the relationship between ritual chiefs and warrior chiefs could become strained and competitive if foreign relations collapsed. Also, in periods of warfare, the warrior chiefs would be able to amass more power; but if they could aspire to become ritual/political chiefs through their deeds, then the strain could be eased. However, this would also be dependent upon the numerical relationship between the two groups. During the centuries from 1300 to 1100 BC, flange-hilted swords become more numerous, whereas Nordic full-hilted swords become less numerous. It suggests that the warrior group could threaten the role of ritual chiefs. But it may also indicate that ritual chiefs had strengthened their power and created larger political entities with lesser opportunities for warriors to achieve high office as ritual leaders.

This double institution also represented a clever division of power that we meet in many societies, both in the anthropological literature and in early historical texts (Dumézil 1988). The later Spartan double kingship may be taken to represent an inheritance from the Bronze Age where it was widespread, as my example from northern Europe suggests. During Mycenaean times, this dual leadership was designated by the term ‘wanax’ (the political/ritual leader), and ‘lavagetas’ (the

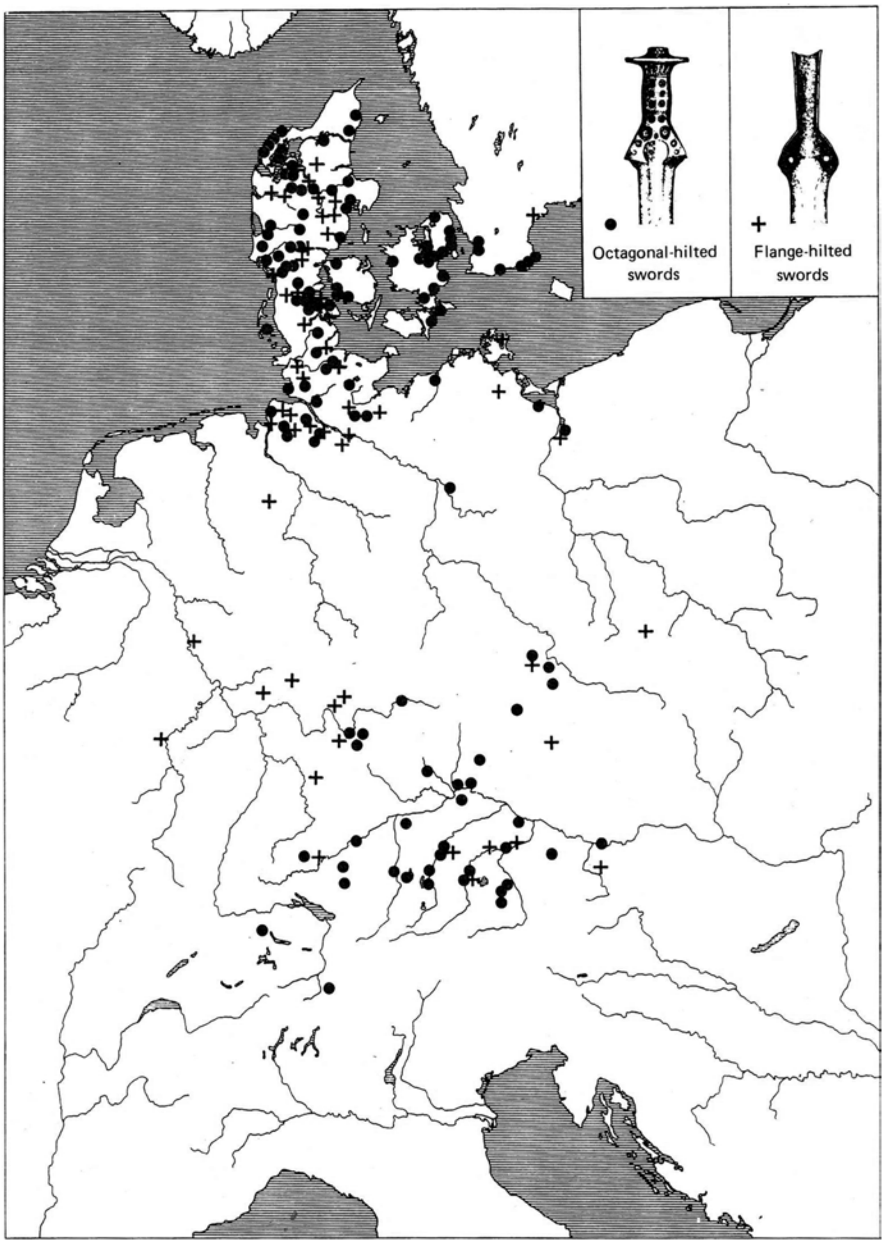

Fig. 10.2 Distribution of foreign swords connecting south Germany and Denmark, versus the distribution of Nordic full-hilted swords

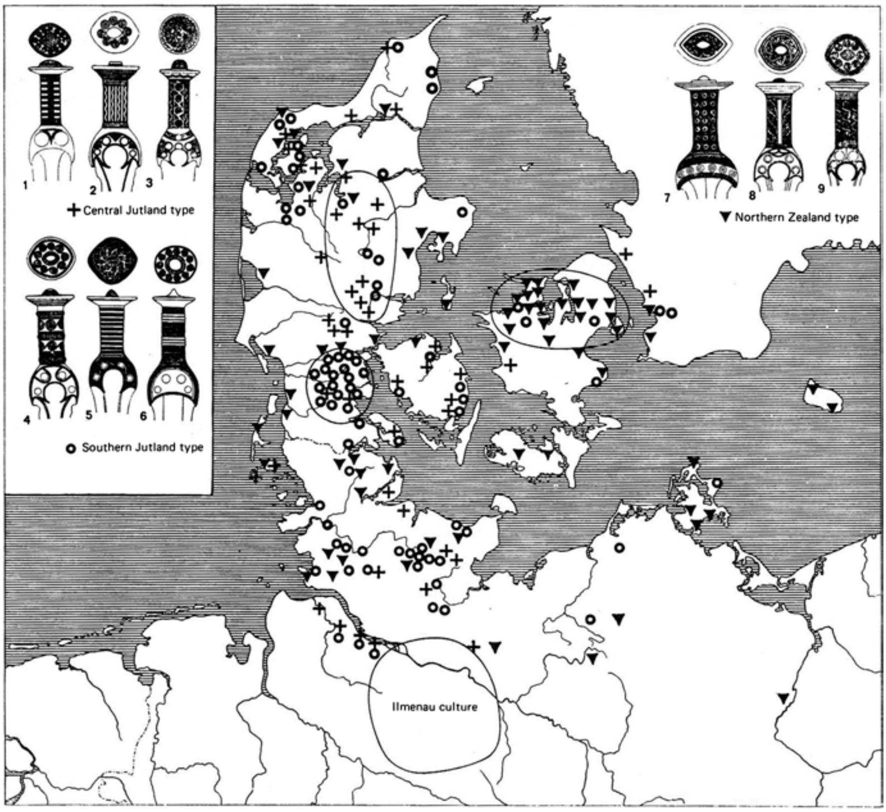

Fig. 10.2 (continued)

war leader). In the Iliad's catalog of ships, two leaders represent each kingdom/ethnic group. Neither in Greece nor in Scandinavia, however, is there any doubt that the ritual/political leader was over and above the war leader, but that did not imply that tensions could not arise between them, as indeed exemplified in the Iliad by the opposition between Agamemnon and Achilles.

Conclusion

I have demonstrated that in the Bronze Age, there existed symbolic fields that corresponded to institutions with different roles and geographical distributions. It speaks about societies that were highly complex, with a capacity to maintain parallel, coexisting forms of identity, some linked to a larger 'foreign' political world and some linked to a more ethnic and ritual world of 'national' identity. In this, the Bronze Age is not vastly different from what we know from slightly later periods, such as Archaic Greece, which exhibits similar developed forms of identity and ethnicity, also testified in written sources (Finkelberg 2005, Hall 1997, 2002, Renfrew 1998). Although the jury is still out as to the existence of larger, shared ethnic identities in the past, our example suggests that by the Bronze Age, we see the emergence of new forms of more bounded ethnic commonalities. They were based upon a shared cosmology and shared institutions, which would, in all probability, also imply some measure of a shared language. Thus, prehistoric material culture holds the potential to unfold social institutions, political and ethnic identities, if unlocked with proper historical and anthropological insights and interpretative strategies.

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Chapter 11
Fine if I Do, Fine if I Don't. Dynamics of Technical Knowledge in Sub-Saharan Africa

Olivier P. Gosselain

Introduction

In our "global village," things and practices are currently diffused over such large areas that few, if any, relationships seem to exist anymore between their spatial distribution and salient cultural boundaries. Global products, such as powder milk, canned fish, or digital watches, are found everywhere, from the fringes of Greenland to the heart of the rainforest, as are cities congested with Japanese cars, boys impersonating the football star of the day, or adults greeting each other with a handshake. These elements have given rise to a form of "world cultural landscape," so pervasive in our daily experience that we do not pay attention to it anymore.

Such patterns of distribution have the propensity to make us feel elated or threatened, depending on our political stance. More importantly, it compels us to pay better attention to the way in which we envisage the relationships between material culture and social boundaries. Is this "blurring" really a new phenomenon? And if so, does it really proceed from the large-scale distribution of cultural traits? Are there conditions under which the spatial distribution of material elements would coincide with salient boundaries? After all, the quest for material correlates of social identities may be just another one of those chimeras pursued by archeologists and anthropologists. A quest that feels especially attractive for those who are confronted with material documents, but a groundless quest all the same. Well-advised historians and art historians, for example, have already warned us against the ineptitude of these "tribal styles" that are highlighted in museums or luxury publications (e.g., Bravman 1974; Frank 1998; Ravenhill 1976; Strother 1998). Archeologists and geographers have also underlined the difficulty of trying to connect things, people, and territories (e.g., Bromberger and Morel 2001; Jones 1997; Stark 1998).

Yet, when pushing the question a step further, one gets the feeling that the issue should not be discarded too prematurely. In particular, part of our difficulty in finding

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B.W. Roberts and M. Vander Linden (eds.), *Investigating Archaeological Cultures: Material Culture, Variability, and Transmission*, DOI 10.1007/978-1-4419-6970-5_11,

relevant relationships between material culture and social boundaries could be due to the fact that we tend to consider "objects" and "practices" indistinctly, and pay too much attention to transmission processes in culture dynamics, at the expense of appropriation and practice. Let us consider football, which is an element of the Western "culture" that has been exported all over the world and that casual observers perceive as (boringly) homogeneous. Indeed, the basic modus operandi and rules are the same everywhere, as is the equipment of players, thanks to warlike marketing strategies. When paying closer attention, however, one observes important differences in postures, gestures or the collective construction of the play. These differences allow, for instance, the distinguishing of the Super Eagles of Nigeria from the Squadra Azzurra of Italy or the Red Devils of Belgium. That is they enable us to identify a series of micro "football cultures" whose spatial distribution may match that of national institutions. How these distinct "cultures" arose is a question that may prove more important in historical and anthropological terms than finding the original locus of football practice and the geography of its diffusion. Naturally, things had to be initially transmitted and mastered for the diffusion to take place, which involved interactions between people. As the nature of these interactions, as well as the identity of the people involved, is highly variable, differences occur in the scale and morphology of spatial distributions (see Bocquet-Appel et al. 1996; Zeebroek et al. 2008) that, in turn, inform us about the history of the diffusion process. But the story does not end there. Once introduced, innovations are inevitably submitted to a process of appropriation, which means both inserting them in preexisting logics and generating new logics from their use (e.g., Miller 1997; Wenger 1998; Zeebroek et al. 2008). Of particular importance in that regard, it is the way in which people use the newly introduced elements in social strategies. Tangled in the ever-changing world of social relationships, diffused items start a new "life trajectory" that profoundly alters their nature and allow them, despite large-scale distributions, to become accurate indexes or social boundaries. Studying the dynamics of transmitted elements, therefore, is not only a way to explore historical processes, but also a way to gain a better understanding of the social dimension of technical practices and material culture – as splendidly illustrated by Lave and Wenger (1991).

In this chapter, I illustrate the dual nature of culture dynamics through the example of pottery techniques in Africa. Relying on observations that I have made for the last 2 decades, as well as a considerable database of ethnographic observations made since the beginning of the twentieth century (Gosselain 2008a), my aim is to focus on the context and process of knowledge acquisition. This means, first, documenting the conditions under which people are introduced to craft, and second, the conditions under which the acquired knowledge is put into practice. This second aspect is fundamental. It allows us to shift from a perspective that eschews contingency and reduces the analysis of culture dynamics to that of transmission processes, to one that sees traditions as situated practices that are not just acquired at a precise moment in time but are continuously reassessed as people engage in daily practice (Bowser 2002; Bowser and Patton 2008; Dobres 2000, 149–52; Gosselain 2008b; Lave 1996; Lave and Wenger 1991; Wenger 1998). As we see throughout this chapter, "reflexivity" on technical actions (see Lenclud 1997) is a key to understanding their dynamics.

Acquiring Knowledge and Skills

As is often emphasized, pottery making is mainly a family and female activity in Sub-Saharan Africa (Drost 1968; Gosselain 2002, 21–31). This means that knowledge is handed down first and foremost among female relatives and very often within the nuclear family. Overall, the proportion of people undergoing apprenticeship outside the sphere of the family is minor, but it may turn out to be high among certain populations. This is due to modifications of the socio-economic context within which the activity is practiced (e.g., Gosselain 1999), or to particular social practices. In northern Cameroon, for example, Delneuf (1991, 72) observed that the choice of actors for the transmission of potting knowledge is due to family lifestyles: continuous education with the mother in non-Islamic and non-Fulani ethnic groups, and education outside the family sphere among Islamic women and especially among the Fulani. Among the Luo of Kenya, women generally marry outside the locality where they were born and are subjected to a resocialization process under their mother-in-law's supervision. If the mother-in-law is a potter, the newlywed will learn the trade at her mother-in-law's side to show that she is ready to integrate into her new family (Herbich 1987).

Another characteristic of pottery making is its accessibility to everyone in most Sub-Saharan populations. In theory, those who desire may learn and practice the trade, as long as they find someone who agrees to pass on her/his knowledge to them. If in practice the activity remains in the hands of certain families or certain groups of individuals, it thus takes place outside of any institutional monopoly. The situation differs dramatically in a series of societies from West Africa, the Lake Chad Basin, the Darfur region of the Sudan, and the Horn of Africa. Here, pottery making is the prerogative of a small number of specialists, who practice endogamy and benefit from a particular social and symbolic status (Barley 1984; Drost 1968; Frank 1998; Gallay et al. 1998; Lyons and Freeman 2009; Sterner and David 1991). This type of restriction does not necessarily have an impact on the identity of the people involved in the learning process. As in other societies, the initial transmission generally concerns relatives, and it may not even be mandatory. What matters is that the number of "specialists" is sometimes very low in certain localities or in certain regions, which, due to strict endogamy rules, can force an artisan to travel long distances to find an appropriate spouse. Such a phenomenon obviously has an effect on the spatial dispersion of traditions (e.g., Haaland 1978; MacEachern 1998).

Whatever the social context within which the activity is practiced, apprenticeship most often takes place during childhood, between approximately 6 and 12 years old. Those who acquire their knowledge outside the family sphere generally do so as adults, but field observations indicate that the belated character of the apprenticeship has no fundamental influence on the mastery of knowledge and expertise. People interviewed in the field stress the quality of the relationship between the person passing on knowledge and the apprentice: they must get along with each other to ensure a successful apprenticeship. If this is not the case with the apprentice's mother, father, or close relative, then (s)he will seek somebody else.

Available data indicate that the actual process of learning must be broken down into at least two phases. During the first phase, the apprentice assists established artisans during certain stages of the manufacturing process: clay extraction, clay processing, fuel collection, setting the firing structure, and removing and treating the vessels after firing. If need be, an apprentice may be given responsibility for operations considered tiring but uncomplicated – for example, extracting clay or crushing shards for grog. This participation is important because it allows the apprentice to become familiar with materials, collection sites, recipes, and the physical characteristics of clay. (S)he also becomes acquainted with the symbolic and social prescriptions linked to certain stages of pottery making. Few people, however, consider this participatory phase as a "true apprenticeship," since it is not explicitly directed toward the acquisition of knowledge. They do not know either when it actually begins or ends, and hardly mention it when asked to describe how they learned their trade. Another important aspect of this first learning phase is that the operations that apprentices participate in are usually led on a communal basis, which means that what they learn correspond to the shared norms of a particular group, be it a family, a local socio-professional grouping, the potters of a whole district, etc. Apprentices are thus initially trained to conform to local norms, which may have important consequences at a later stage of their life if they relocate in a new community. Lastly, there is no particular order to what apprentices learn during the participatory phase. As illustrated by Lave and Wenger (1991, 96): "[p]roduction activity-segments must be learned in different sequences than those in which a production process commonly unfolds, if peripheral, less intense, less complex, less vital tasks are learned before more central aspects of practice."

Clearly, the "more central aspects" of pottery making pertain to the shaping operations, usually subdivided into the "roughing out" and "preforming" stages. Here, the acquisition of relevant skills leads the apprentice to enter into a much more formal phase – which many consider as the true *moment* of apprenticeship. Field observations and potters' testimonies indicate that the change first becomes evident in the protagonist's attitude: up until then, the apprentice had a mainly playful relationship with shaping pottery; (s)he played with clay, but did not really seek to make a vessel. If (s)he is sufficiently motivated[1] and "gifted" (notions that crop up constantly in interviews), the teacher redirects the game toward the acquisition of expertise and adopts a much more active role with her/his pupil. There is clearly a shift of status at this stage, which some potters signify by submitting the apprentice to an initiation (e.g., Hauenstein 1964; Knops 1959; Quarcoo and Johnson 1968) or giving her/him an emblematic tool. Among the Nama blacksmiths of Dia (Mali), for example, young female potters receive a terracotta tournette, made by the person who takes them into apprenticeship. They keep it for life. Similarly, female Songhay, Zarma, and Bella potters in Niger, who use the pounding technique for

[1] The notion of "motivation" covers a great number of factors as demonstrated by Wallaert (2000, 2008).

shaping the vessels, often receive a small terracotta hammer when they begin their apprenticeship. They later inherit their mothers' hammers – treasured objects that potters hand down from one generation to the next.

Whatever the context, the apprentice first endeavors to rough out small wares, miniature models of those her/his instructor makes,[2] or wares for particular purposes, such as saucepans, piggy banks, and incense holders. These first attempts rarely meet with success: the walls collapse, the pressure exerted is too weak or too strong, etc. To help the apprentice overcome these difficulties, the instructor must go beyond the role of a simple model: (s)he works alongside the apprentice, correcting errors and ill-executed movements and, quite often, holding the apprentice's hands so that (s)he can physically sense the correct movements and hand positions. Those questioned stress the importance at this level of the relationship between the instructor and the apprentice: for knowledge to be passed on correctly, there must be respect, patience and, from the point of view of the one passing on the knowledge, a mixture of severity and benevolence.

At the end of this phase, which can last from a few months to a couple of years, the apprentice has assimilated all the movements and postures linked to shaping, but it is only very progressively that (s)he goes on to make bigger wares. Most of the people questioned explain that what happens afterward is a "matter of practice." They especially emphasize the stability of their technical behavior: "I do as my mother/father did," they say, no matter where they came to live after learning the craft or what their life trajectory was. Some even stress that change should not occur at all, as any modification in the manufacturing process may jeopardize its outcome.

Scales and Asymmetries in the Distribution of Technical Behavior

The emphasis put by potters on the stability of technical behavior is an interesting situation for archeologists. Indeed, if, on the one hand, the transmission of pottery traditions usually occurs between closely affiliated individuals at a particular moment in their lifetime, and, on the other hand, such traditions are not submitted to postlearning modifications, they should thus propagate as whole packages through space and time, along familial networks and according to individual movements. Given that female individuals essentially move for matrimonial reasons in rural Africa, and that marriage mostly occurs between people who belong to the same social group, the distribution of pottery traditions should then coincide with major social boundaries, such as languages, political units, or socio-professional subgroups.

The problem is that they do not. There are rather few coincidences with such boundaries, be it at the level of techniques, tools, materials, or finished products

[2] These wares are sometimes marketed as toys, which provides an incentive for apprentices to pursue in the learning process (Etienne-Nuge and Saley 1987; Owusu-Ansah 1973; Traoré 1985).

(see illustrations in Berns 1989, 2000; Frank 1998; Gelbert 2001, 2003; Gosselain 2008b; Sall 2001, 2005). Moreover, one does not observe any coherency between the elements that constitute each pottery traditions. In fact, the technical characteristics pertaining to *each* stage of the manufacturing process tend to be distributed according to their own mode and to evolve at their own pace.

Clay extracting and processing techniques differ usually from one region, village, district, or even family to the next, regardless of other existing boundaries. Among Baatonu-speaking people of Northern Benin, for instance, potters knead the clay with a pestle in a raised wooden mortar, as do their Boko- and Pila-speaking neighbors. We are thus faced with a regional tradition whose distribution crosses salient cultural boundaries. Within this area, however, I recorded seven different recipes for preparing the clay paste, some being used in a series of villages, others in a single community or even a single family. A similar situation has been recorded in Yorubaland (Fatunsin 1992), Northern Cameroon (Livingstone Smith 2000), and many other parts of Sub-Saharan Africa (see examples in Gosselain 2002, 75–77). In fact, the spatial distribution of clay processing recipes is seen to operate simultaneously at two scalar levels (see discussion and examples in Gosselain 2010; Gosselain and Livingstone Smith 2005; Livingstone Smith 2000). The first pertains to the ingredients used as "temper," such as grog, cereal husk, dung, crushed stones, etc., or basic processing operations, such as drying and crushing, sieving, foot tramping, or hand kneading, etc., whose variations are seen to spread over areas that generally exceed several hundred kilometers. The second scalar level pertains to the singular combination of particular "tempers" and processing operations, that is, the actual processing "recipes." Here, variations may be regional or subregional, but they mainly develop within micro spaces, such as a district, a village, or a string of villages.

In regard to firing and postfiring operations, technical variants are also distributed in a very heterogeneous way. Potters from the same district, village, or string of neighboring villages may use similar fuel materials, structures, and/or tools, while others who speak the same language and belong to the same subgroups use other materials, structures, and tools. In the Hausa village of Jiratawa, Southern Niger, for example, male potters of the Roumawa district fire the pots with millet stalks in large ovens, while those of the Dakawa district fire them in the open with a combination of straw, dung, and wood. Interestingly, they all produce the same highly standardized water pots and use similar techniques at other steps of the manufacturing process. Other examples of intra-village variations exist in the ethnographic literature (e.g., Kientega 1988; Manessi 1960; Schott 1986). But variations may also occur at a micro-regional level, such as the "elevated bonfire" in the northern part of the Great Lakes region (Gosselain 2002, 157–158), within ethnolinguistic boundaries (e.g., Lawton 1967; Priddy 1971; Strybol 1985; Thiam 1991), or according to gender (e.g., Kientega 1988; Kreamer 2000; Zouré 1999).

Ornamental traditions are even more complex in terms of spatial distribution. Some motives may be the consequence of a small number of individuals or neighboring communities (see the classical examples of Balfet 1965 for North Africa or Herbich 1987 for the Luo of Kenya) while design structures and tools are usually shared by a larger number of people and sometimes distributed at a subcontinental level.

Tools, such as fiber roulettes, for instance, are used in around a half of African populations in a geographically bounded area extending throughout the Sahelian belt from Senegal to the horn of Africa, and southward into the Great Lakes region (Gosselain 2000; Livingstone Smith 2007). Within this huge area, which does not coincide with existing cultural boundaries, even at a macro level, roulettes are used to make a variety of motives which are themselves organized according to local design rules. Although large-scale comparisons of other ornamental techniques still have to be made, a similar situation seems to prevail for painting, incising, or impressing. As in the case of clay processing recipes, we are faced with a repertoire of techniques and motives that may spread over very large areas, but whose particular combination allows for the identification of more salient boundaries, such as language or grouping of communities that share a common history (see Berns 2000 for an illustration of a meaningful regional distribution of pottery designs in Northern Nigeria). One must note, however, that the sharing of a similar ornamental repertoire does not necessarily blur social boundaries. In his comparative study of two neighboring pottery-producing centers in the Cameroonian Grassfields, Argenti (1999) shows that diverging representations have developed in each community regarding the meaning and use of shared figures. As a consequence, differences are recorded in the way they are executed, the size and morphology of the vessels to which they are associated, as well as the gender and status of the individuals to whom they are associated.

The sole step of the manufacturing process whose variations do more frequently and obviously coincide with salient boundaries is shaping – or, more precisely, the rough out operation. At that level, variations in gestures and in the way clay elements are deformed and/or joined together may coincide closely with ethnolinguistic boundaries, linguistic groupings, ancient political boundaries, or the spatial extension of socio-professional subgroups (among many examples, see Frank 1998; Gallay et al. 1998; Gelbert 2001, 2003; Gosselain 2000, 2002; Kanimba 1996; LaViolette 2000; Nicklin 1981; Pinçon 1997; Pinçon and Ngoie-Ngalla 1990; Priddy 1971; Sall 2001, 2005; Thiam 1991; Woods 1984). This does not mean that variations in shaping techniques always match that of meaningful boundaries in Africa, far from it. As already observed at other levels of the manufacturing process, particular variants may be distributed over huge areas, independently from language and/or social affiliation (e.g., Gosselain 2002; Huysecom 1994; Sterner and David 2003), or over areas that, although much smaller, cross salient boundaries (Gallay et al. 1998; Gelbert 2001, 2003; Langlois 2001) or do not seem to bear any relationship with them (Gosselain 2008b; Lyons and Freeman 2009. They may even vary according to the gender of the potter (e.g., Kreamer 2000; Priddy 1971; Roy 1987). The fact remains, however, that when comparing the spatial distribution of shaping techniques to linguistic, social, or political boundaries, including ancient ones (see Gallay 1994; Livingstone Smith and Van der Veken 2009, one usually gets a better match than for any other step of the manufacturing process. As I previously concluded at the outset of a cross-continental comparison, shaping techniques *tend* to reflect those most rooted and enduring facets of identity in Sub-Saharan Africa, and hence to give us information on a category of social networks built upon cultural or even kin affiliation (Gosselain 2000, 210).

This all indicates that pottery traditions correspond to a heterogeneous collection of elements whose spatial and temporal evolution follows different lines, and whose variations allows us to approach different facets of people's identity. This is already an interesting conclusion, but we need to go a step further in exploring the underlying reasons of this phenomenon. Why is there such a distortion between what potters say about the origin and development of pottery traditions and the picture that one gets when comparing those traditions at a micro or macro level?

Between Practice, Socialization, and Economy

In order to understand the situation described above, we must go back to the moment when individuals complete what they describe as the actual process of learning; that is, when they have mastered the skills required for fashioning the vessels. Regardless of the time at which they have completed that learning, many potters continue to practice the craft within the same social and spatial context. The youngest stays under the supervision of their relatives, while the oldest, who entered the craft at a later stage of their life, may work more casually with their former teacher, but nevertheless practice the craft under the same conditions as the ones that prevailed at the time of learning.

Yet, most apprentices do not spend their life where they have learned the craft. They go to live in other places, some nearby and some more distant according to marriage, divorce, or for a series of economic and personal reasons. Some artisans also set themselves up seasonally in regions where they may be confronted with other traditions (Simmonds 1984; Gelbert 2003; Gosselain 2008b; Tobert 1988). Whether permanent or temporary, these moves have several implications from the point of view of the dynamics of potter cultures (for a few case studies see David and Hennig 1972; Frank 1998; Gallay 1994; Gosselain 2002, 2008b, 2010; Huysecom 1994; Pinçon, 1997).

First, artisans must locate new clay sources and identify zones, where they can collect other raw materials involved in the manufacturing process. Some of these materials are easily found; others require more arduous searches. This is particularly true if their usage is specific to pottery making and the incoming artisan has no opportunity to mix with other specialists. In addition, certain materials can simply be unavailable in a region, as happens for certain plant species used in postfiring treatments (Gosselain 2002, 194–195).

Next, the artisan might need to target a new clientele and satisfy other requests and tastes. One immediately thinks of decoration in this respect (e.g., Sall 2001), but artisans may also be confronted with particular requirements as regards the form, color, and even physical properties of the wares. For example, several Zarma potters of Southern Niger explained to me that they used three different clays when making pottery that they intended to sell on neighboring markets, and a single one when making pottery for themselves or friends and relatives. The reason, they said, was to maintain their reputation on marketplaces, since it was widely acknowledged that "good pottery" was made with three clays. In Senegal, Tukulor potters who settled in the upper valley of the Senegal River have started to use vegetal fibers rather than dung for processing the clay because their Soninke clients consider dung an impure material (Gelbert 2001, 82). Customers may also consider that vessels are stronger when black and shiny, as among Doayo of Cameroon (Vander Linden 2001), or orange red, as among Gurensi of Northern Benin. Specific firing and postfiring techniques are consequently used to obtain such properties.

A third implication for incoming potters is that they are led to interact with a new group of colleagues when settling in a different community. This is a crucial element that brings us back to the social dimension of learning and to the meaning attached to potting practices. We have seen that potters are initially socialized into the craft through participating in the work of confirmed artisans. Starting with less complex and less vital tasks, they are progressively drawn toward more central aspects of the craft through a process of "legitimate peripheral participation" as coined by Lave and Wenger (1991) in their pioneer study of communities of practice. An essential aspect of this participatory process (see Wallaert 2008 for a detailed example) is that skill and knowledge acquisition combines with the development of an identity of a "member," as the apprentice increases her/his participation in the community and progressively reaches a more central position. In this context, the shared repertoire of practices acts together as a binding element, which reinforces the link between members of the community and their sense of group identity, and as a vehicle that helps newcomers negotiating their insertion within the community (see especially Bowser and Patton 2008; Corniquet, in press). What matters here is that such negotiation does not stop at the outset of the learning process. Seen from the perspective of individual actors, the learning process never ends, insofar as the "social world of activity" (Lave 1996, 5) is continuously evolving: potters may join new communities, as stated above, but their own community may also be modified due to the insertion of new participants, changing relationships between older participants as they shift status through their life trajectory, or new connections with other potting communities.

Far from being a "closed package" that the apprentice sticks to and brings along throughout her/his whole life, the repertoire acquired during initial learning is an open aggregate whose individual components are both constantly liable to be reassessed and modified, and enrich the repertoire of other practitioners. Of crucial importance is the fact that the evolution of a repertoire does not depend on the nature of its constituting elements, but on the meaning attached to them at the time they are put into practice. As this meaning is strongly dependent on the social world of activity, there is no way to tell, a priori, whether an element is reproduced, borrowed, modified, or abandoned. What is sure, however, is that meaning is continuously reconstructed by individuals through their lifetime and expressed in a variety of ways, be they technical, social, or economical. In that regard, individuals are continuously engaged in a process of reconciling past and current experiences and unifying elements that often prove to be contradictory (Kaufmann 2004). It is a "fine if I do, fine if I don't" kind of tension that one is frequently confronted with in the field: on the one hand, potters emphasize the inherited nature of their behavior, "I do as my mother did, and as did her own mother," on the other hand, they obviously tune this behavior in order to fit with local ways of doing. What follows are some examples of the conditions under which pottery traditions may be either reproduced or transformed.

Dynamics of Technical Knowledge: Some Ethnographic Examples

In southern Niger, the spatial distribution of clay processing recipes follows two distinct patterns. In the west, recipes often vary from one village to the next and sometimes within the same village community. If shared by distinct communities, they usually cluster in micro areas. An important aspect of the craft throughout this western region is that it is carried out by people bearing distinct, and often competing, socio-professional status. In the east, pottery making is (mostly) open to anyone and carried out by people who do not bear any particular status. As for clay processing recipes, they tend to group within large and bonded areas. I have shown elsewhere (Gosselain 2010) that where pottery making is constitutive of the potter's identity, individuals take great care in avoiding blurring social boundaries through using inappropriate processing recipes. They do so according to what is known locally about other ways of doing and what are perceived as meaningful boundaries, which translates into microscale processes of technical homogenization. When an incoming potter brings with her another recipe that is locally inappropriate, it is either abandoned or used as a secondary recipe.[3] Conversely, where a potter's identity is not at stake, such as when pottery making is simply a source of income, the processes of homogenization occur at a much larger scale. Here indeed, the absence of "social filters" creates conditions under which processing recipes propagate progressively according to "classic" factors, such as personal mobility, marriage networks, communication routes, or the density of settlements (see also Livingstone Smith 2000).

The shaping stage offers us other illustrations of the way representations attached to technical procedures are liable to alter its evolution. Of particular interest here is the strong connection between shaping techniques and the deeply rooted facets of identity – as opposed to more situational ones. In Cameroon, for example, I met a Gbaya potter in Yoko, Central Province, who practiced the drawing of a lump technique, but also mastered the coiling technique, learned from a neighbor in a former village. She had chosen to teach the latter to a young Hausa neighbor in her new village community because, as she explained, "I'm a Gbaya and she's a

[3] For example: "One may add dung to the clay if the amount of grog available is not sufficient"; "One may add millet husk to the clay if it is too wet." Note that if local representations change, secondary techniques may regain a primary status.

Hausa. She needs her own technique." Among Niger's Songhay blacksmiths, I met women who had acquired the molding technique from their Bella neighbors, but who had chosen to pass on the pounding technique to their daughters, pounding being regarded as the "true Songhay technique" (see details in Gosselain 2008b). In the same region, women from a village deemed the main pottery production center proudly explained that "for more than five generations, only pounding has been practiced here" – the equivalent, all in all, of a seal of quality based on the notions of "tradition" and "soil." Conversely, in the central region of Niger, potters of Tuareg origin, who occupy the lowest social position in their society, have been transforming themselves into Hausa, a population associated with Muslim orthodoxy, urbanity and wealth (Nicolas 1975), within which artisans do not bear any status. Besides adopting the Hausa language, clothes, and architecture, they seem to have "purified" their technical repertoire, shifting from the pounding technique, that local people associate with a Tuareg identity, to molding, which is locally associated with Hausa.

We are now in a better position to understand why shaping techniques seem to change at a slower rate than other steps of the manufacturing process, and why their variations frequently coincide with social boundaries, such as language, socio-professional affiliation, or gender. Rather than being due to the combined effect of motor habits and the spatial extension of matrimonial networks, as I previously emphasized (Gosselain 1998, 2000), such situations may result from a deliberate conservatism among the potters. How to shape a pot is not a trivial issue as it relates both to group affiliation and the psychological bonding of teacher and apprentice during the second phase of learning. Acting usually as a strong stabilizing factor, this bonding creates also the conditions for sudden shifts in techniques, such as when artisans are engaged in a redefinition of their identity.

Although I formerly thought that decoration would be more likely to reflect more superficial and situational facets of identity, another example from Niger shows that the preoccupations developed about ornamental designs may parallel those observed at the level of shaping. In the River Region, polychrome painted vessels are currently the most appreciated pottery. Produced mainly on the eastern bank of the river by the Bella, former Tuareg slaves, it is sold on both banks of the river and throughout Zarma and Songhay country. Many female Zarma and Songhay potters consider the Bella's painted pottery more beautiful and more prestigious than their own. Numerous earthenware jars from the eastern bank may thus be found in the homesteads of Zarma and Songhay potters and in those of other members of their community. When asked why they do not adopt this style, which would increase their sales on marketplaces, Songhay potters reply, "To each her own." By discussing in more detail, it becomes apparent that Songhay potters refuse to adopt the polychrome style in order to continue differentiating themselves from the Bella. Belonging to the socially stigmatized but (according to them) less lowly group of the blacksmiths, their decision is all the more important. That being said, much of the dynamics observed at the level of ornamental practices relate especially to the emergence of new fashions, a process in which customers play a central role, to the arrival of a new clientele, and to competition between potting communities and individuals. For instance, Corniquet (in press) documents how a new pottery style has recently appeared in the Arewa Region of Niger, and how it has been subsequently incorporated in the repertoire of a series of local potting communities. Its pattern of distribution coincides with that of marketplaces frequented both by members of these communities and by the middlemen who initially brought exemplars of this new style. Schildkrout et al. (1989) provide another example of the effect of the consumption sphere upon local practices. At the onset of the twentieth century, in north-eastern Congo, Mangbetu potters started to produce a new category of vessels, anthropomorphic jugs, that were bought by notables as gifts for European colonists. This style was not a complete innovation, as it was built upon elements borrowed from neighboring populations and other media. Although still displayed as an emblem of "Mangbetu culture" in museum collections, it disappeared after 1 or 2 decades, when other types of political and economical relationships started to develop between Congolese populations and Europeans.

Postfiring treatments give us a last example of the way technical practices may be adapted to fit with local practices and representations. In northern Cameroon, Koma Ndera women only started producing pottery two or three generations earlier. The techniques that they use at the various levels of the operating chain are similar to those used by neighboring populations, from whom the techniques have obviously been borrowed. Two aspects nonetheless diverge: the prohibitions linked to certain production stages and the ingredients used for preparing the organic coating applied at the end of firing. With regards to the latter, it is striking that, on the one hand, the new ingredients are used locally for medicinal or ritual purposes; and on the other hand, that the same functions are filled, among neighboring populations, by the "rejected" ingredients.[4] There seems to have been some sort of technical adjustment, making it possible to ensure the compatibility of technical practices and certain symbolic representations (Gosselain 1999).

Conclusion

The data discussed in this chapter show that pottery traditions comprise a heterogeneous collection of elements that, while initially acquired as a whole by individuals over a short period of time, are constantly reevaluated during practice and may be manipulated accordingly. A good part of these manipulations result from interactions with new social actors. Setting up in another environment, negotiating a position in the community of practice to which one belongs, or adapting to the changing tastes of customers can have a significant impact on individual practices. In this respect, pottery traditions are strictly comparable to any other cultural

[4] Both are available in comparable quantities in the region.

assemblage, or even to what we call "culture" in general, that is, inherently unstable, situated, and historicized configurations (see Brumann 1999).

With regards to pottery cultures, African artisans theoretically have at their disposal an extraordinary panoply of appropriate practices for realizing their objectives (Drost 1967; Gosselain 2002, 2008a). In reality, however, they consider only a limited number of possibilities, both because they simply ignore the existence of alternative ways of doing, and because they filter their choices when alternatives become conceivable. In other words, pottery cultures are not constructed chaotically, according to the whim of people's interactions or the mechanical diffusion of components, as water would flow in a system of pipes, but arise from a strong channeling of elements at both the collective and individual levels. Kaufmann (1997, 37) talks in this respect about "control processes" and "leeway restrictions," while stressing especially the role of individual constructions. The reason for this is that pottery making does not exist independently from other practices and value systems. As repeatedly shown over the last decades (e.g., Dobres 2000), numerous representations are mobilized during each technical act; representations which, from the actor's point of view, are completely inseparable from other types of knowledge. Corresponding to what is usually called "world views," these representations allow artisans to classify, without too much difficulty, what it is locally appropriate to use, make, and produce. Alongside these "collective" representations, there exist others that are more personal and more diversified as to their origin. Following Lave and Wenger (1991; see also Bowser 2002; Bowser and Patton 2008; Corniquet in press), I have illustrated the effect of both the learning process and the functioning of communities of practice on the development of individual representations. We have seen, for example, how the way in which the protagonists of learning engage during the second phase of skill acquisition, when training to shape vessels, leads them to view shaping technique as both an "inheritage" and an index of the most rooted facets of their identity. Conversely, the way in which they are socialized into the craft during the first learning phase leads them to view (and use) another part of the technical repertoire as a vehicle for ascertaining social ties and negotiating one's position into a community of practitioners.

Thus, there exists an inherent tension in every potting practice between a desire to maintain and reproduce the link with those from whom the knowledge was initially acquired, and the unavoidable adjustments imposed by the social and economic contexts within which individuals carry the craft throughout their life trajectory. Far from being mere procedures, transmitted and reproduced mechanically from one generation to the next, the components of technical repertoires are meaningful and deeply invested in daily experience. In this regard, the scale at which they are distributed is only a part of the question pertaining to the identification of meaningful social boundaries in the material world. Be they large-scale phenomena, as those evoked in the introduction of this chapter, or more modest ones, spatial distributions simply ensure the *availability* of elements liable to be incorporated in local repertoires. What matters, from that point on, is the meaning that people give them, that is, how they give them a social life and define the conditions of their evolution.

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Chapter 12
The Transmission of Technological Skills in the Palaeolithic: Insights from Metapopulation Ecology

Terry Hopkinson

Introduction

In a series of seminal publications in which he developed his celebrated typological scheme for the stone tool industries of the French Lower and Middle Palaeolithic, François Bordes argued powerfully that there was no chronological dimension to lithic industrial variability in the Middle Palaeolithic of southwestern France. Instead, he saw the variability present in stratified sites, such as Combe Grenal and Le Moustier, as the outcome of discrete episodes of occupation through time by five culturally distinct Neanderthal groups or "tribes", each of which was characterised by a culturally specific technological and typological repertoire that remained unchanged for perhaps as long as 80 millennia (e.g. Bordes 1953; Bordes and de Sonneville-Bordes 1970). Today, the argument for long-term technological stasis in the Middle Palaeolithic of France, or indeed anywhere else, is no longer tenable. Mellars' observation of chronological patterning in the Mousterian of the region (e.g. Mellars 1967, 1996) has been confirmed, at least in its broader outlines, by the construction of a robust regional Middle Palaeolithic chronology synthesising multiple site stratigraphies, biostratigraphical correlations and radiometric dating (e.g. Mellars 1986a, b, 1988; Mellars and Grün 1991). Similar developments in other regions of Europe, for example, the Upper Danube (Weiβmüller 1995; Richter 1997, 2002), have demonstrated comparable, but specifically different, shifts in chipped stone technology through the Middle Palaeolithic of Marine Isotope Stages 5 (130–71 kyrs BP), 4 (71–60 kyrs BP) and 3 (60–24 kyrs BP).

At the same time, however, the standard developmental chronology of the Levantine Middle Palaeolithic, based on Tabun Cave, Israel, has been strongly challenged by work which indicates that the stratigraphic sequence of industries at Tabun is in fact unique to that site and has little or no regional chronological validity

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B.W. Roberts and M. Vander Linden (eds.), *Investigating Archaeological Cultures: Material Culture, Variability, and Transmission*, DOI 10.1007/978-1-4419-6970-5_12,

(Munday 1979; Marks and Monigal 1995; Goren-Inbar and Belfer-Cohen 1998; Hovers 1998). It is now clear that stone tool fabrication practices in the western Eurasian Middle Palaeolithic varied significantly through time and in space, but that variability cannot readily be explained either in terms of long-term evolutionary developmental trajectories at the regional scale or by reference to a multiplicity of static stone tool cultures.

The issue of hominin behavioural change prior to the Upper Palaeolithic has been addressed by a number of authors over the last 10 years or so. Typically, this work has adopted a holistic approach to behaviour in the Palaeolithic, going beyond lithic industrial taxonomy to create syntheses of hominin behaviour which also incorporate factors, such as mobility, the spatiotemporal structure of landscape use and subsistence practices. In consequence, it now seems irrefutable that significant shifts in hominin behaviour occurred around 300 kya across both the African Early–Middle Stone Age (McBrearty and Brooks 2000) and European Lower–Middle Palaeolithic (Hopkinson 2007a) transitions, and that new behaviours continued to emerge thereafter throughout Africa and western Eurasia (papers in Hovers and Kuhn 2005).

Of course, the tripartite subdivision of the Palaeolithic into Lower, Middle and Upper phases reflects major, persistent and spatially extensive behavioural developments, principally in material culture typology and technology, apparent in the archaeological record. The transition from the Lower to the Middle Palaeolithic in western Eurasia, for example, is associated with the rise of Levallois technology, as well as with the systematic occupation of climatically seasonal regions and of regions of high topographic relief (Hopkinson 2007a). Yet Levallois technology seems to have appeared locally and sporadically throughout the European Lower Palaeolithic after 600 kya (Hopkinson and White 2005). Similarly, prismatic blade technology was practised by Neanderthals in northern Germany and in Northeastern France some 90 millennia prior to the onset of the Upper Palaeolithic (Conard 1990; Ameloot-van der Heijden 1993) and then simply disappeared. A comparable story of short-lived local or regional innovation anticipating behavioural shifts that only subsequently become widespread and persistent is visible in the African Middle Stone Age. The Howiesons Poort (HP) industry of South Africa, notable for its precociously "modern" geometrical, microlithic and backed pieces and datable to 60–80 kya at Klasies River Mouth (Miller et al. 1999) and Border Cave (Grün et al. 2003), is a case in point. Recent detailed investigations of the HP and succeeding industries at Rose Cottage Cave have confirmed that "The HP was a very innovative industry; but it did not persist and did not give rise to the LSA" (Soriano et al. 2007: 700). The authors suggest that "…the lack of persistence of the HP innovations is in need of an explanation" (Soriano et al. 2007: 681).

Indeed it is. Sporadic, localised and ephemeral outbreaks of behaviours and practices that become institutionalised and fixed in hominin behavioural repertoires only many millennia later are very difficult to explain in terms of currently dominant paradigms that refer such institutionalised behavioural shifts to evolutionary biological (and therefore innate) advances in hominin cognitive capacities.

The key to understanding possible solutions to the problem of behavioural change in the Palaeolithic lies in going beyond a conception of such change as simply enabled by the evolution of enhanced cognitive capacities. Neither is it sufficient to understand technological developments in narrow mechanistic terms, whether that be as optimal adaptive solutions winnowed from a stochastically generated array of behaviours by the blind forces of natural (or economic) selection or as culture-system responses to changes in environmental pressures (e.g. papers in Torrence 1989). Although these factors should not, of course, be denied, their utility as explanators of technological change visible in the archaeological record depends on an understanding of technology and material culture as the *social* products of *social* individuals located in particular *social* worlds (Hovers 1998: 158). Technological change in the Palaeo-lithic is, therefore, best understood as necessarily originating in innovative new practices perpetrated by skilled, knowledgeable and purposeful hominin agents. The question then becomes one of why innovations at certain times and places disseminate through the wider social realm only to a limited extent and then disappear, but at other times and places disseminate widely through social space and become persistent, long-term social practices.

However, our understanding of the social and system dynamics of knowledge dissemination and transfer in the Palaeolithic remains in its infancy. Increased population density has been identified as one, possibly critical, factor in promoting innovation and its social dissemination (Shennan 2001). Bordes himself raised the same possibility 40 years ago (Bordes 1968; Bordes and de Sonneville-Bordes 1970: 72). Yet, one cannot accept Zilhão's assertion (Zilhão 2007: 39) that one need not go beyond a simple population increase model to explain major behavioural change in the Palaeolithic, even if we limit, as he does, our concern to the events of the European Middle–Upper Palaeolithic transition. Population increase at which scale? Local, regional and continental-scale population sizes and densities need not rise and fall together; local increases in hominin population sizes do not necessarily entail regional increases in population densities. In any case, it is the dynamic relation between the local, at which scale innovation originates, and the regional, to which level innovative behaviours must disseminate if they are to become persistent and widespread, which is the point at issue.

This problem has perhaps been seen most clearly by Hovers, who explains non-linear chronological patterns of lithic technological variability in the Levantine Middle Palaeolithic by reference to the transmission of technological practices between potentially innovative local populations distributed variably within a wider regional social, cultural and ecological space (Hovers 1998: 157–158). Similarly, Peck and Ives (2001: 185–187) have brought ethnographic accounts of numerical, social and cultural fluidity in native American bands to bear on the problem of bifacial point stylistic variation in the late pre-contact North American Plains. Neither, however, has at their disposal a theoretically and empirically grounded framework for understanding how local and regional population structures and dynamics, and the relation between them, might impact upon the transmission and persistence of innovative technological practices in time and space. It is suggested here that the principles of *metapopulation ecology*, allied to a social network approach to the dissemination of culturally specified practices, provide a basis for a first-order approach to this problem.

Metapopulation Ecology and Palaeolithic Hominins

Metapopulation ecology is a theoretical and methodological approach to the ecogeography of terrestrial animals. It is based on the premise that just as local populations of any species are composed of many individuals, so regional populations are composed of many local populations. This regional "population of populations" is the *metapopulation* (Levins 1970). Local populations are chronically vulnerable to extinction or extirpation and are more or less ephemeral. The species persists on longer timescales only at the regional metapopulation level (Levins 1970), providing that every local population extinction event be, over time and space, balanced by a colonisation event in which the territory vacated is reoccupied by a new local population comprising individuals derived from a neighbouring population. Metapopulation ecology, therefore, shares many structural features in common with shifting mosaic models in landscape ecology (e.g. Bormann and Likens 1979) and with island biogeography (MacArthur and Wilson 1967). In a synthetic review, Hanski (1999) has identified several key conditions which must be met if a metapopulation approach is to be valid and which must, therefore, apply if such an approach is to be reasonably applied to Pleistocene hominins:

1. The species must be discontinuously distributed in space with localised concentrations of population
2. The environment must be structured heterogeneously in space as habitat patches
3. Each tract of suitable habitat must be occupied by one local breeding population
4. Individuals must be mobile and potentially liable to migrate between local populations

The first of these criteria is clearly met by hominins as social organisms. With respect to the second, the northern mammoth steppe certainly conformed to this requirement (Guthrie 1984, 1990; Gamble 1995; Lister and Sher 1995; Hopkinson 2007b: 31–35). In Africa too, palaeoenvironmental evidence clearly indicates that the Pleistocene hominin occupation of the continent is associated strongly with patchy environments. Indeed, the occupation of patchy mosaic landscapes seems to have been the central feature of hominin ecology at least since the emergence of the obligate terrestrial biped *Homo erectus/ergaster* around 1.8 mya. The requirement that each tract of habitat be occupied by just one local breeding population or group appears more problematical, since it seems probable a priori that regional hominin metapopulations comprised local populations whose territorial or range boundaries were, at least sometimes, fluid and defined socially rather than fixed environmentally by the limits of habitability. However, the point of this condition is that separate breeding populations of a species should not live sympatrically if a metapopulation approach is to be valid. If we take a local hominin group's foraging territory or range to constitute a "tract" for current purposes, and providing we can assume that local territories or ranges were essentially discrete and were not substantially shared at any point in time by several local populations, the condition is effectively met. Finally, the mobility of Palaeolithic hominins is not in question, although the migratory behaviour of individuals is not understood. However, individual movement between groups is well attested both in chimpanzees and in modern peoples. Indeed, juvenile male migration is widespread among mammals generally and is probably a consequence of inbreeding avoidance (Dobson 1982). In summary, it is reasonable to investigate Middle Pleistocene hominin ecogeography from a metapopulation perspective.

Hominin Metapopulations as Knowledge Networks

The potential relevance of metapopulation ecology to our understanding of behavioural change in the Palaeolithic lies in the fact that human metapopulations constitute regional-scale social networks through which knowledge, information and values are liable to flow. From this point of view practices, including technical, mobility and subsistence practices, represent enacted knowledge capable of disseminating through metapopulation scale social networks. Indeed, social network theorists (a taxon that today includes physicists and population biologists) are developing models of the flow of values and practices through such networks in which local populations function as nodes (e.g. Borenstein et al. 2006).

It is, therefore, central to the current argument that hominin behaviours in the Palaeolithic are seen as *practices* necessarily entailing both knowledgeable individuals perpetrating skilful acts for particular purposes in social contexts, and socially mediated cultural repertoires of practical knowledge that pre-date any particular individual and which furnish individuals, through socialisation, with a knowledge base that enables their purposeful action (Hopkinson 2007b: 20–30; Hopkinson and White 2005). The emergence of novel behaviours must therefore originate with innovation, understood as the purposeful application of already existing skills in new ways by knowledgeable individuals. What is more, these innovative practices must then be practised not only by the innovator individual or individuals, but must also be taken up by others so that the new practice itself becomes incorporated into the cultural repertoire. It is this critical transition in the history of an innovative practice from individual idiosyncrasy to widespread, persistent cultural institution that metapopulation ecology can illuminate.

For present purposes, it is assumed that Palaeolithic hominins were organised in small (probably between 10 and 100 individuals; but see Dunbar 1998 for a qualified higher estimate, at least for hominins with brains of fundamentally modern size), mobile bands thinly distributed in the regional landscape at population densities that were, by any contemporary standard, very low. These mobile bands were the local populations or groups that together constituted the regional metapopulation.

It is also assumed that connections existed between local populations within a regional metapopulation, whether in the form of whole-group encounters, encounters between individuals from different local groups, or as the migration of individuals from one local group to another. At the very least, the requirement for a breeding population of a critical minimum size if reproductive viability was to be maintained would certainly have necessitated reproductive exchanges between local groups too small to provide low-consanguinity mates for all their adult members. It is precisely in these encounters or movements between local populations that the possibility of novel practices culturally endemic within an innovator population being transmitted to other local populations is immanent.

The question of whether one should talk about innovator individuals or innovator populations requires some clarification. The centrality of the purposeful, knowledgeable individual to the innovation process has already been recognised. At the same time, one must also recognise the weight of evidence that small-scale social entities are strongly conformist and tend to suppress "deviant" behaviours; this has been recently been demonstrated in studies both among modern people (e.g. Baerveldt and Snijders 1994; Efferson et al. 2008) and in captive chimpanzees (Whiten et al. 2005; Hopper et al. 2006). On the other hand, local populations themselves must be considered not as homogeneous entities but as clusters of very small-scale "cliques" (as "metacliques", if you will) with social commitment and conservatism at their strongest within, rather than between, cliques (Dunbar 1998). In view of these considerations, it must be considered highly likely that, throughout the Palaeolithic, individuals devised innovative behaviours that failed to be taken up by their group peers and which more or less quickly fell into disuse. On the other hand, on those occasions when novel practices, for whatever reason, were indeed adopted within a local population, the same tendency to conformism would serve to preserve them even beyond the lifespan of the innovator individual. It is in this sense that we can speak of innovator populations, embedded in a metapopulation in which the novel practice is otherwise unknown, and which can potentially function as a source from which the practice might spread to other local groups within the metapopulation.

If regional hominin metapopulations in the Palaeolithic are conceived in this way as composed of local populations that are chronically short-lived, then this has important implications for the transmission of knowledge between local groups. The key principle here is that the likelihood of transmission of a novel practice from one local group to another is a function of the number and character of contacts and connections between them. Consequently, the longer a local group practising an innovative behaviour persists, the greater the probability that it eventually is taken up by a neighbouring group. Conversely, the more frequently local groups suffer extinction, the less likely it is than an innovation arising in and practised by one local population will spread to their neighbours before the innovator population itself becomes extinct and the innovation dies with it. Sporadic, ephemeral and localised outbreaks of novel behaviour is just the kind of pattern one would expect to see in a long-lived regional metapopulation of hominins composed of innovative but short-lived local populations. Equally, the long-term and widespread behavioural shifts associated with major transitions in the Palaeolithic can be understood as predictable outcomes of an increase in local population longevity. Any such increase would promote the transmission of novel practices between groups and permit them to persist in wider, metapopulation scale social networks and continue to be available for further transmission even after the disappearance of the group in which they originated. What follows is an examination of the metapopulation factors that impact upon the longevity of local populations – in this case, hominin local populations. Metapopulation ecology identifies three major and interrelated factors: local population size, territorial range and between-group individual migration.

Local Population Size

The size of a local population (i.e. the number of individuals of which it is comprised) in any animal species is determined by four variables: natality and immigration, both of which increase population size; and mortality and emigration, which decrease it. Local population extinction occurs when its size falls below a critical lower viable limit and its continued survival becomes impossible. The most significant cause of local population extinction seems to be stochastic variation in population size through time (Hanski 1999: 28). Disregarding the effects of external environmental factors, the time to extinction for any population is given by its current size, its upper ceiling size (at which point the population fissions or density-dependent processes inhibit further growth), its mean growth rate and the variance in that growth rate. The larger the current population size, the longer the population is likely to persist before extinction (Foley 1994, 1997). In addition, factors, such as the age structure of the local population and its ratio of reproductive-age females to males, are more prone to stochastic variation in small populations. In hominin groups, the premature death of a few prime-age adults can be a fatal blow for a small population, whereas a larger population might retain sufficient such individuals to remain viable. Similarly, small groups are more likely, through stochastic processes alone, to find themselves with too few reproductive-age females to maintain the levels of natality necessary for continued viability. What is more, small local population size promotes emigration as individuals seek membership of larger populations (Kuussaari et al. 1996) and discourages individual immigration (Smith and Peacock 1990). Conversely, large local populations attract immigrants, either because they offer more mating opportunities or because individuals interpret large group size as evidence of resource abundance. These considerations compound the strong tendency for local group longevity to be linked closely and positively to local group size, and together constitute the *Allee Effect* – reduced and even negative per capita growth rates in small populations (Allee et al. 1949). Allee et al. also recognised that this effect might be exacerbated by sociality effects in highly social species. In hominins, these might take the form of a prohibition on breeding with close relatives and mothers' reliance on others for the provisioning and/or care of children.

External factors, including environmental events and changes in resource abundance and availability, can amplify temporal fluctuations in local population size.

The effects of time lags and interactions with other species can amplify them yet further and increase the risk of a population falling below a minimum threshold size and becoming extinct. Larger populations are less prone to extinction through these processes, and also because it is more likely that some individuals will do well even if others are doing badly. It is important to note that although the *Allee Effect* and its amplifications discussed here lead to small populations experiencing a high risk of extinction and reduced longevity, the same processes contribute to the persistence of the wider metapopulation. Being in large part stochastic, oscillations in local group size across the metapopulation tend to be asynchronous; populations experiencing extinction and others close to or at their upper population ceilings, therefore, coexist at any one time within regional metapopulations. The extinction of one population creates "empty" terrain available for recolonisation through the fissioning of a larger nearby population (Ruxton 1996).

The critical point here, however, is that, in time, all local populations become extinct. A high extinction rate produced by stochastic oscillations in population size, while contributing to metapopulation persistence, necessarily means short average local population life spans. Of course, in large-bodied *k*-selected mammals, such as hominins, these processes unfold over much longer time spans than is the case with, say, small mammals. Nevertheless, the principle holds: the longer-lived local hominin populations were, the greater the probability that any behavioural innovation arising and becoming fixed in one local population would be transmitted to other populations before the extinction of the originating group, and the greater the likelihood that, following a local group extinction event, the terrain would be recolonised by a new local group practising the innovative behaviour. Metapopulation ecology shows that larger populations persist longer than smaller ones, so hominin metapopulations comprising relatively large local populations are more likely to facilitate the dissemination of novel behaviours than those comprising relatively small local groups. In one sense, this supports Shennan's (2001) suggestion that increased hominin population sizes promote innovation; what the metapopulation perspective adds is the realisation that *local* population increase can deliver this effect *even if the regional metapopulation experiences no increase in total size* but is instead organised into fewer but larger local populations. Of course, this does not preclude the possibility that population growth at the regional level, if it entails an increase in local population numbers as well as in local population size, could also promote the transmission of novel behaviours through the increased number of inter-group encounters that ensue from the tighter packing of local groups in space.

Local Population Territorial Range

Environmental change through time also has an important spatial component. The spatial structure of landscape mosaics is coupled with environmental periodicities operating on multiple wavelengths (Woodward 1987) from annual fires

(Zedler et al. 1983) and floods, storms and pest outbreaks to volcanic eruptions (Rogers et al. 1994), as well as with climatic periodicities operating on decadal, centennial and millennial wavelengths (King 1996). In addition, landscape mosaic ecology today understands vegetation succession not in Clementsian terms of "the climax community" (Clements 1916, 1936), but in terms of patch upgrade–downgrade dynamics in which patches at every stage of succession are present in a shifting mosaic (Bormann and Likens 1979). In consequence, the spatial structure and species diversity of mosaic landscapes are in constant flux, and it is impossible to identify any ideal equilibrium state (Connell and Sousa 1983). Instead, different components of terrestrial ecosystems are coupled with different temporal wavelengths of limiting factor. The result is landscape asynchrony. This was especially true of the Pleistocene, during which high-amplitude millennial-scale Dansgaard-Oeschger climatic oscillations inhibited the establishment by competitive exclusion of optimally adapted, spatially extensive, low-diversity plant and animal communities and instead promoted persistent fine-grained mosaic landscapes that were highly fluid (Hopkinson 2007a, b) and sustained high levels of species diversity (Lister and Sher 1995) in close spatial proximity.

Since the ecosystem effects of temporal environmental stochasticity are distributed unevenly and asynchronously in space, the risk of local population extinction can be reduced, and their average longevity increased, not only by increased population size but also by expanded foraging range. Larger areas are likely to be more heterogeneous, and to contain a wider array of resources, than smaller areas. Within-range spatial environmental asynchrony also tends to increase with range area. Consequently, the catastrophic disappearance of a key resource from one part of a larger area does not mean that it will disappear everywhere within that area, and even if it does a suitable alternative resource is more likely to remain available in a larger than in a smaller territory. In metapopulation ecology, this principle has been termed the *vegetation mosaic* hypothesis (Short and Turner 1994).

Developments in hominin mobility strategies in the Palaeolithic should, therefore, be seen not only as behavioural innovations in themselves, but also as a causal factor in conditioning the likelihood of novel behaviours disseminating from an originator population through the wider metapopulation. Expanded territorial and foraging ranges tend to increase local population lifespan since a wider-ranging strategy spreads in space the environmental risks that can lead to local population extinction. It follows that a logistically organised mobility strategy (Binford 1980) can also deliver an *effective* increase in territorial range without any *absolute* increase since, unlike whole-group mobility, it allows the group to be, in effect, in more than one place at the same time and permits resources to be harvested on a larger spatial scale within a specified time window. More speculatively, it is possible that, under logistically organised mobility strategies, small task specific parties from different local populations might converge upon the same particular environmental patch at the same time to harvest its specific and briefly available resources; if so, that circumstance would create a systematic basis for regular encounters between local populations, and therefore for enhanced transmission of ideas and practices between them.

Immigration and Emigration

This third key factor governing local population longevity is, in principle, the most problematical in terms of archaeological visibility. Nevertheless, it is of some importance and deserves to be built in to models of hominin metapopulation social and ecological dynamics, including knowledge transmission. Two kinds of migration can be identified: the migration of individuals between local populations, and the occupation of "empty" terrain by a newly formed population following fission of a larger population. Both are important in understanding local population longevity and metapopulation persistence, and are considered together here.

A local population will become extinct if the net emigration rate (rate of individual emigration minus rate of individual immigration) exceeds its rate of population growth, even in the absence of determining environmental events, such as resource exhaustion or catastrophic habitat destruction. Conversely, a local population can survive even if its mortality rate exceeds its natality rate, provided its net immigration rate (rate of individual immigration minus rate of individual emigration) is sufficiently high. This can be particularly important when a local population inhabiting a high-value habitat experiences a high intrinsic growth rate, persists at or close to its ceiling population size and thus produces many emigrants and daughter populations that repeatedly maintain local population levels in surrounding lower-value habitats – the so-called *source–sink* dynamics (Pulliam 1988, 1996). A corollary of this is that population size is significantly affected by isolation. The greater the distance between a local population and its nearest neighbours, the smaller it is likely to be because isolation obstructs immigration more than it obstructs emigration (Hanski 1999: 17). As we have seen, this in turn impacts on the population's risk of extinction.

Migration is promoted and inhibited by a range of factors:

1. *Inbreeding avoidance.* If breeding with close relatives reduces fitness, it is selectively advantageous to move away from the parent population to another, or to drive away some or all of one's offspring. This is thought to be the basis of the evolutionary development of intrinsic, male-biased juvenile emigration in mammals (Dobson 1982). We have no direct archaeological evidence for the range of sexually structured migratory behaviours practised by Palaeolithic hominins, but all living people and apes show some kind of systematic tendency for one or more classes of individual to migrate in search of low-consanguinity mates. In a hominin local population that is so small, it is close to its lower limit of viability and is liable to increase net emigration and therefore to accelerate the population's extinction or dissolution. Culturally specified practices that mitigate inbreeding, such as an incest taboo or exogamous marriage, would also promote individual migration even between larger local hominin populations. Individual migration of this character, therefore, potentially affects the transmission of knowledge and skills in regional social networks not only directly, through the flow of knowledgeable individuals between populations within the broader metapopulation, but also through its effect (either positive or negative) on local population size and thus longevity.
2. *Population size.* Emigration rates also tend to increase with local population density, especially in mammals (Baker 1978). For social hominins, this corresponds

to local population size. Emigration rates will increase with group size if this is associated with ecological limiting factors and with elevated levels of interpersonal conflict. Indeed, these can be understood as density-dependent processes that also contribute to group fissioning at population sizes at or close to the viable ceiling. Ecological factors have been shown to limit group size in primates (Chapman et al. 1995), but large group size has also been shown to promote fissioning because it becomes increasingly difficult for individuals to service a prohibitive number of interpersonal relationships (Henzi et al. 1997). Emigrants from a large group form a pool of immigrants to other groups. With respect to hominins, where one or several populations in a metapopulation occupied favourable habitat and experienced high growth rates, then the source–sink dynamics mentioned above predict that populations in marginal habitats would have been maintained by a steady input of individual immigrants from larger populations (considerations of isolation taken into account) even if their mortality rate exceeded their natality rate. Alternatively, if emigration primarily took the form of daughter-population formation through parent-population fissioning, then new populations derived from a high-value terrain source would have driven continued metapopulation presence in marginal sink regions, where local population longevity was low and extinction rates high. So, given a metapopulation in a region characterised by one or a few high-value territorial ranges, hominin migration patterns must have been strongly asymmetrical with a significant net emigration (of individuals or daughter populations) from the high-value "source" ranges to the low-value "sink" ranges. The likelihood of an innovative practice or behaviour being transmitted from the innovator population to others in the metapopulation was, therefore, directly related to the particular part of the region in which it occurred. A new practice arising in a population inhabiting marginal terrain (where, one might speculate, functionally advantageous new behaviours might preferentially be expected to emerge) was, therefore, unlikely to disseminate through the metapopulation since the innovator population would either produce few emigrants, or would be structurally prone to a short lifespan, or both. Of course, climate and environment change over time might shift the location of favoured source habitats, and thus of source populations, within a region.

3. *Migration cost.* Theoretical (Olivieri and Gouyon 1997) and empirical (Ims and Yoccoz 1997) studies have shown that mortality in migration is its major cost. Structurally high migration mortality will inhibit emigration and depress immigration. Structural factors affecting migration mortality include the habitat between groups and the likelihood of a would-be immigrant being rebuffed, or even killed, by members of a group to which they are seeking admission.
4. *Migration distance.* A critical structural cost of migration is the distance to be travelled; the longer the migration distance, the greater the cost. When local populations are thinly distributed in the regional landscape, the isolation effect inhibits migration so that small isolated local populations are less likely to have the capacity to stave off extinction through immigration.

For hominins, there are probable additional costs to migration that metapopulation ecologists do not generally consider, concerned with sociality and landscape knowledge. An individual leaving their group and seeking admission to another must weigh the benefits of doing so against the loss of social position this will entail and the need to construct a new set of social relationships. A daughter population occupying "empty" terrain following parent group fission, and possibly individual migrants in the period between leaving their parent group and gaining admission to another, must cope with the unfamiliarity of the landscape and the spatiotemporal distribution of resources within it.

Some Preliminary Conclusions

If a hominin metapopulation palaeoecology is to be developed, advances need to be made in the estimation of local and regional population sizes in the Palaeolithic. Identifying individual migration and population fissioning events seems very far off at present. However, we do have evidence for expansions in hominin territorial ranges, at least around 300 kya in both Europe (Hopkinson 2007a) and Africa (Barham 2000), which are consistent with the consolidation of previously sporadic and ephemeral practices from that time in the Middle Palaeolithic and Middle Stone Age through the enhanced knowledge transfer between local populations that increased territorial range facilitates. Logistically organised mobility practices are apparent in the African MSA (McBrearty and Brooks 2000) and at least in the later European Middle Palaeolithic (e.g. Hopkinson 2007a). Insofar as the evidence currently exists, it is both capable and supportive of interpretation from a metapopulation viewpoint. Certainly, the modelling of hominin subsistence, sociality, ecology and behavioural history needs to incorporate the likely "on the ground" realities that metapopulation ecology reveals.

Cursory as it is, this trawl through the principles of metapopulation ecology challenges us to think past the comfortable paradigms of evolution in which we have all too often wrapped ourselves as students of very deep human history. Novel behaviours, including technical practices, might (or might not) have been functionally adaptive in that they equipped their practitioners to optimise their risk and energy cost–benefit calculations more effectively (either literally or figuratively, if natural selection is understood to have carried out that calculation for them); but a metapopulation approach to knowledge transfer counsels that functional-adaptive utility alone is certainly not sufficient to guarantee the necessary spread of a new practice from its point of origin to a wider hominin social realm and its fixation in that realm until its own supercession by a yet more functionally adaptive practice. Useful new ways of doing can fail to transfer from the originator population and then disappear with the extinction of that population, for reasons entirely unconnected with their utility or with directional advance in human biological and cultural evolution. As has been observed, the Palaeolithic record is replete with examples of precocious practices that then disappear, and gives us no cause to understand the evolutionary history of human behaviour as a one-way ratchet. Metapopulation ecology can provide the basis for understanding how new practices might arise and then suffer the various fates – rapid loss, partial diffusion, regional penetration persisting for a few millennia, or long-term persistence on continental or even larger scales – that we actually see in the archaeology of the Lower and Middle Palaeolithic. It also offers an explanation for the mosaic-like character of some key transitions, such as that from the African Early to Middle Stone Age (MSA) in the Kapthurin Formation, where late Acheulean and a range of early MSA industries (including Fauresmith and Sangoan) within the Bedded Tuff depositional complex, dated to between 235 and 284 kya, appear to have been contemporaneous (Tryon and McBrearty 2002). This pattern is just what would be predicted from a metapopulation perspective in which locally arisen innovations percolated differentially through a regional metapopulation as environmental and social dynamics at different spatial scales facilitated or obstructed their transmission. One might even apply the same reasoning to the European Middle–Upper Palaeolithic transition if one accepts its "mosaic" character (e.g. Svoboda et al. 1996).

Equally, a metapopulation approach suggests that appeals to gross evolutionary developments in hominins' heritable cognitive capacities are inadequate as explanations of major transitions within the Palaeolithic. Metapopulation ecology is much better placed than is biological essentialism to explain the sporadic outbreaks of apparently precocious, even "modern", behaviour that have been described, such as Neanderthal prismatic blade production or the Howiesons Poort phenomenon. McBrearty and Brooks (2000) provide an exhaustive survey of the piecemeal accretion of "modern" human behaviour in the African MSA and conclude that the shift to a biologically specified modern human condition occurred in Africa at the Acheulean-MSA boundary around 300 kya. Certainly, the gradual accumulation of innovations over the 250 millennia of the MSA is compatible with the metapopulation dynamics considered here. Perhaps more importantly, metapopulation thinking raises the possibility that some of their "markers" of modern human behaviour – range extension, long-distance raw material procurement and exchange networks, and scheduling and seasonality, for example (McBrearty and Brooks 2000: 492) – reflect expansions in territorial ranges and logistically organised mobility, and as such might better be understood as *conditions for*, rather than markers of, modernity. Without, of course, wishing to deny the reality of human cognitive evolution, it should nevertheless be recognised that understanding behavioural developments in the Palaeolithic from the perspective described here leads us to an understanding of "modernity" (or indeed of any other "grade" of hominin behaviour) not as an essential, biologically specified condition, but as an emergent property of environmental, social and demographic processes operating on multiple scales.

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Chapter 13
Steps Towards Operationalising an Evolutionary Archaeological Definition of Culture

Felix Riede

Introduction

There is considerable debate among anthropologists and archaeologists about the ontological status of human "culture". A plethora of definitions have been offered (Kroeber and Kluckhohn 1978; Kuper 1999), the vast bulk of which are anchored in ethnographic accounts or foreground cognitive dimensions of human experience. Hence, they are of limited utility to archaeologists who, by and large, have to contend with a patchy and discontinuous record that consists exclusively of more or less durable material culture. Although of limited analytical utility to archaeologists, many definitions of culture nonetheless recognise that the social transmission of information is at its core (for a recent review of the North American literature, see Lyman 2008). Pitt Rivers (1875, 298), for instance, noted that "hereditary transmission" of cultural traits underpins our ability to recognise series of cultural transformations. These observations were later formalised in the typological method (Montelius 1903) and subsequent seriation approaches (O'Brien and Lyman 1999; Riede 2006b, 2010a).

In this paper, I argue that an alignment of "culture" with processes of information transmission allows the development of a specifically archaeological definition of culture under the umbrella of Darwinian theory. Such an approach rests on the rejection of typological concepts of culture, which remain widespread in archaeology. Instead, it is argued, culture may be understood as a materialist, population-level phenomenon that is generated through the actions of individuals and that it takes archaeological shape through the consistent socially learnt repetition of such actions across generations. In a historical perspective, this approach lends itself to tree-like exploratory models – cultural phylogenetics – and this may aid in not only classifying a given set of archaeological remains into culture-like groupings, but also in answering long-standing questions about processes of change in material

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B.W. Roberts and M. Vander Linden (eds.), *Investigating Archaeological Cultures: Material Culture, Variability, and Transmission*, DOI 10.1007/978-1-4419-6970-5_13,

culture, action and behaviour. I present a brief case study from the Southern Scandinavian Late Glacial illustrating how such a specifically evolutionary archaeological method of defining cultures can be operationalised.

Information Transmission and Material Culture Evolution

The transmission of information is at the core of Darwinian models of evolution (Jablonka and Lamb 2006). First formalised with respect to genetic information transmission, there is now a widespread recognition that salient information transmission covers a much greater number of domains, such as the epigenetic, the social and behavioural, as well as the ecological (Jablonka and Lamb 2005; Nielsen 2007; Odling-Smee 2007; Odling-Smee et al. 2003; Wells et al. 2006). Noted already by Lewontin (1970) a long time ago, Jablonka and Lamb (2006, 237) reiterate that Darwinian evolution can emerge in any information transmitting system because…

> … the transmission of information between generations, whether through reproduction or through communication, requires that a receiver interprets (or processes) an informational input from a sender who was previously a receiver. When the processing by the receiver leads to the reconstruction of the same or a slightly modified organization-state as that in the sender, and when variations in the sender's state lead to similar variations in the receiver, we can talk about the hereditary transmission of information. This typically occurs through reproduction, but it can also occur through communication if communication leads to a trait of one individual being reconstructed in another. Clearly, if the hereditary transmission of information is seen in this way, there is no need to assume that all hereditary variations and all evolution depend on DNA changes.

The archaeologists Eerkens and Lipo (2007, 246) underline that "it is more productive to conceive of a general case in which genetics, culture, language, and the like are simply versions of generic inheritance systems, structured means in which information is passed between sources and destinations. These systems differ greatly in their implementation, dynamics, and degree of fidelity…but this is irrelevant to their information-theoretic structure". In sum, when the transmission of information between generations – by whatever means – displays the properties of trait *variation* between units, *heritability* and *differential representation* of these traits from one generation to another, some form of Darwinian evolution is the result. Note that in such formulations of Darwinian evolution, crude selection and survival only play minor roles. The agents creating the material culture variation act purposefully and intentionally. They are knowledgeable, yet they are not omniscient (Mesoudi 2008) and the picture of cultural evolution invoked here is demonstrably not the kind of evolution caricatured by post-processual theorists (e.g. Shanks and Tilley 1993).

That Darwinian theory holds some promise for understanding long-term material culture change was recognised early on by pioneering scholars, such as the Swedish antiquarian Oscar Montelius. Although it took some time for Darwin's ideas to become widely disseminated in Scandinavia (Kjærgaard and Gregersen 2006), references to his works become more common in archaeological texts shortly after translated works first become available. In the early 1870s, the Swedish scholar Hans Hildebrandt (1873, 17) stated that "if any science at present needs its Darwin, it is comparative archaeology".

Recognising the evident similarities in the palaeontological and archaeological records, and the challenges faced by workers in both fields, he went on to draw explicit analogies between archaeology and palaeontology (e.g. Hildebrandt 1880). Oscar Montelius further elaborated this point by making the case for the similarities between cultures in an archaeological and species in a palaeontological sense (Montelius 1884, 1899). Beyond this basic insight, he (Montelius 1903, 20) argued that

> It is in actual fact rather amazing that Man in his labours has been and is subject to the very same laws of evolution. Is human freedom indeed so limited as to deny him the creation of any desired form? Are we forced to go, step by step, from one form to the next, be they ever so similar? Prior to studying these circumstances in depth, one can be tempted to answer such question with «no». However, since one has investigated human labours rather more closely, one finds that clearly, the answer has to be «yes». This evolution can be slow or fast, but at all times Man, in his creation of new forms, needs to conform to the very same principles that hold sway over the rest of nature.

Montelius clearly recognised that the evolution of culture was historically constrained, that the creation of new forms was contingent on their predecessors, and that the transmission of information is vital in shaping material culture expressions. His student Nils Åberg (1929, 508) reiterated that "typology is the application of Darwinism to the products of human labour". However, the notion that cultures are analogous (as natural or analytical units) to species as they were thought of in the late nineteenth and early twentieth centuries is fundamentally flawed, precisely because both entities were defined typologically. The species concept in biology is still controversial (e.g. Ereshefsky 1992; Mayr 1957; Rieppel 2007), but the essentialism of typology has long been abandoned for "population thinking" (Ghiselin 1974; Hull 1965; Mayr 1959). This epistemological revolution came about at a time when archaeologists had roundly rejected the application of Darwinism to human works (e.g. Brew 1943) and had instead turned to ecologically and sociologically inspired approaches (Riede 2006b, 2010a; Trigger 1989), despite the fact that Mayr brought this issue to the attention of anthropologists at the time (see Mayr 1959). As a consequence, many archaeologists employ types for building diachronic sequences and for making arguments about change over time, something for which such entities are profoundly ill-suited (Lyman and O'Brien 2003, 2004; O'Brien and Lyman 2000). From the adoption of population thinking flows a focus on *variation* and the need to use quantitative techniques (Mayr 1976, 27–8):

> The assumptions of population thinking are diametrically opposed to those of the typologist. The populationist stresses the uniqueness of everything in the organic world. What is true for the human species, that no two individuals are alike, is equally true for all other species of animals and plants… All organisms and organic phenomena are composed of unique features and can be described collectively only in statistical terms. Individuals, or any kind of organic entities, form populations of which we can determine the arithmetic mean and the statistics of variation. Averages are merely statistical abstractions; only the individuals of which the populations are composed have any reality. The ultimate conclusions of the population thinker and the typologist are precisely the opposite. For the typologist, the type (eidos) is real and the variation an illusion, while for the populationist the type (average) is an abstraction and only the variation is real. No two ways of looking at nature could be more different.

Eerkens and Lipo (2005, 2007) and Eerkens and Bettinger (2008) have similarly argued that variation in material culture should be the focus of archaeological enquiry, at least in so far as it is concerned with the social transmission of information.

Population thinking alone, however, cannot lead to a readily operationalised definition of culture in an archaeological sense. While helpful to ethnographers working with contemporary populations (Bloch 2005; Sperber 1996), and indeed some anthropologists pursuing an evolutionary approach (Mace and Holden 2005), these definitions of culture are predicated on having comprehensive linguistic information. Cultures, in this view, are socio-linguistic entities and mutual intelligibility is understood as a proxy measure of interaction akin to Mayr's (1957, 2000) interbreeding criterion (Fig. 13.1). For archaeologists lacking linguistic information,

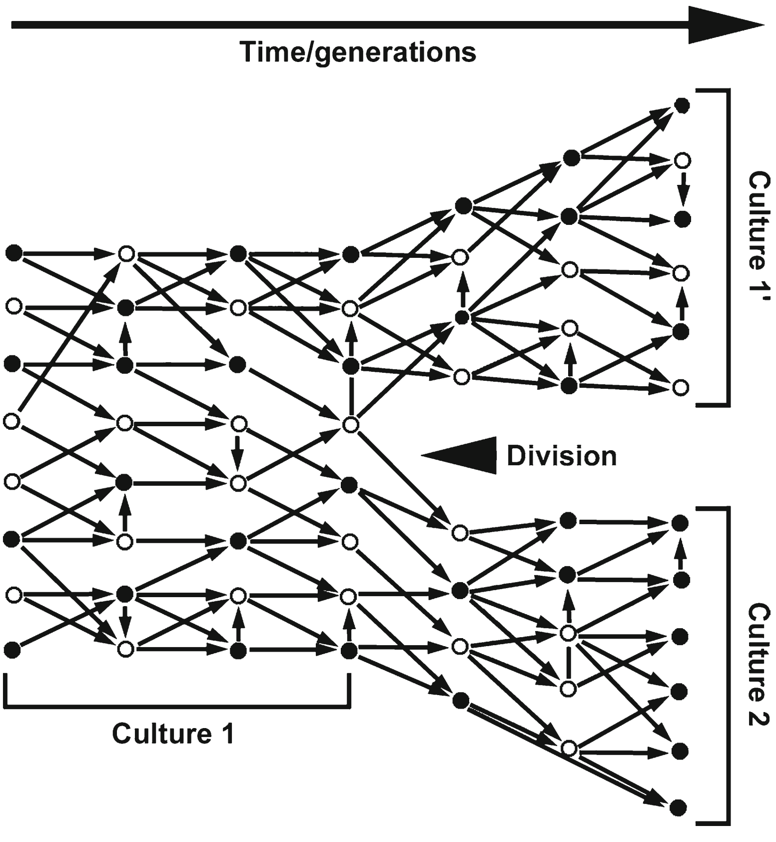

Fig. 13.1 An archaeological culture (Culture 1) as a heterogeneous population of teachers/learners (*open circle* and *filled circle*) and their intellectual and/or artefactual sub-lineages. The various modes of cultural transmission (vertical, oblique and horizontal; see MacDonald 1998) produce complex patterns of expression. Over time/generations, when the horizontal transmission between two segments of the ancestral population diminishes, empirically recognisable new cultures arise (Cultures 1′ and 2). Which particular mode of transmission dominates at a given time is an empirical question (Bellwood 1996). Note that what is not shown here is that in each generation some individuals are likely to leave no cultural descendant, terminating a particular sub-lineage. This figure is redrawn from Hennig (1966)

however, such a formulation is problematic. Palaeobiologists who cannot observe interbreeding directly face similar difficulties and have argued that a phylogenetic species concept may be more suitable to examining long-term changes in the historical relatedness and changes in adaptation in the organic world (e.g. Mishler and Theriot 1999; Nixon and Wheeler 1990; Wheeler and Platnick 1999). These species, however, are not ontologically equivalent to living species, just as an archaeological culture or techno-complex cannot be compared to cultural groups observed ethnographically. In order to avoid confusion, therefore, the culture = species notion should be rejected. Where does this leave us?

It is important to remember that species in palaeontology are merely one kind of taxonomic unit (Lee 2003). Phylogenetic species then are one of the operational taxonomic units (OTUs) of palaeobiology, and it has been suggested (Foley 1987), most recently by Gamble et al. (2005), that similar units should be used in archaeological enquiry. The use of archaeological taxonomic units (ATUs) incorporates the key epistemological insights of the rejection of essentialism and avoids the terminological confusion surrounding the term "culture"; they are the "cultural counterpart to the operational taxonomic unit (OTU) of biology and evolutionary science" (Gamble et al. 2005, 195). In contrast to most definitions of "culture", the ATU concept offers archaeologists a pragmatic avenue for constructing appropriate units for cultural phylogenetics, because as O'Hara (1997, 323) has suggested "tree thinking" complements the population perspective by providing an explicit historic dimension (O'Hara 1997, 324–5, my emphasis):

> Tree thinking is simply the phylogenetic counterpart to population thinking, and like population thinking it has brought a more completely evolutionary perspective to systematics… Tree thinking, in contrast to group thinking, considers species in a phylogenetic context, not as independent replicates but as parts of a single phylogenetic tree. If we seek to understand common causes acting in evolution then the replicates we need to examine are not species, but the evolutionary events that are of interest in a particular study, and this can only be done by plotting those events on a tree… Although tree thinking…is an aspect of systematic biology, the idea of tree thinking isn't necessarily tied to living things – all it requires is descent and inheritance.

A great number of recent studies have advanced the use of tree-building approaches in anthropology and archaeology (Lipo et al. 2006; Mace et al. 2005; O'Brien 2008; O'Brien et al. 2003). Here, I query one of these approaches using a case study from the Southern Scandinavian Late Glacial. In particular, the resulting phylogenetic diagrams will be explored as tools for defining archaeological cultures.

Defining Archaeological Taxonomic Units

Although Gamble and colleagues have reopened the discussion on taxonomic units in archaeological analyses, they fail to provide an adequate methodology for actually constructing such units. Taxonomies remain inert classification exercises unless they are placed into an explicitly evolutionary framework (see Riede 2009b; Fig. 13.2). The subjects of evolutionary archaeological analysis

	Equivalent	Examples from Gamble et al. (2005) > *this study*
ATU1	Period	Palaeolithic, Mesolithic, > *Late Upper Palaeolithic*
	Sub-period	Early Mesolithic, > *Terminal Palaeolithic*
ATU2	Techno-complex / Culture	Aurgignacian, > *Arch-Backed Piece Complex*
	Culture / Industry	Upper Magdalenian, > *Hamburgian, Bromme*
	Industry / assemblage	Magdalenian IV, > *Havelte phase*
ATU3	Artefacts / type fossils	Navettes, Mouilah points, > *Zinken, Bromme points, Wehlen scraper*
	Attribute	Scalar retouch, truncation, > *tang-thickness / orientation, tip retouch*

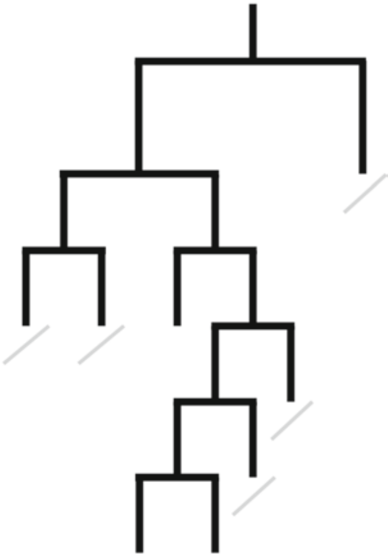

Fig. 13.2 *Left*, the division of archaeological taxonomic units for the North European Late Glacial suggested by Gamble and colleagues. The text in italics are examples from this study. *Right*, a schematic representation of how these units may map onto to a phylogenetic branching diagram. Such diagrams must be generated, and read, from bottom to top beginning with the smallest units distinguishable by a shared history of social information transmission

are the "learning lineages" (Harmon et al. 2006, 209) of craft production, manifest in their consistent and repeated material expressions.

By virtue of their biological endowment, humans are strongly predisposed towards both learning (Fragaszy and Perry 2003; Laland 2004; Reader and Laland 2003; Shennan and Steele 1999; Tomasello et al. 1993) as well as teaching (Csibra and Gergely 2011; Tehrani and Riede 2008; Thornton and Raihani 2008). Contexts of scaffolded learning and indeed of active teaching can, at least on occasion, be identified in the archaeological record (see Bamforth and Finlay 2008). Although other methods for identifying the appropriate units of cultural transmission (Pocklington 2006; Pocklington and Best 1997) have been put forward, Apel (2008), Apel and Darmark (2007), Riede (2006a, 2008b) and Tehrani and Riede (2008) have suggested that detailed technological analyses, following the *chaîne opératoire* approach, can be used to identify those elements of material culture that are consistently passed on from generation to generation, at a level suitable for archaeological enquiry. Similarities in material culture are so generated through the historical relatedness of their makers and their placement within communities of learners/teachers that persist over archaeological time. On the level of the population of learners, such traits – as proxies for the knowledge, skill and know-how – can be tracked in space and time. Specifically for the Late Glacial, it can be demonstrated at several locales that teaching played an important role in the transmission of craft skills (Bodu et al. 1990; Pigeot 1990) taking place at flint-knapping "schools" and workshops (Fischer 1988, 1989b, 1990). Such traits are decidedly not memes (Dawkins 1976) or cultural viruses (Brodie 1996; Cullen 1996); these cannot be identified archaeologically (Lake 1998) if indeed they can be identified or exist at all (Aunger 2006; Bloch 2000; Boyd and Richerson 2000; Sperber 2000). Either way, a replicator akin to genes is not a necessary condition for cultural evolution to follow Darwinian principles (Henrich and Boyd 2002; Henrich et al. 2008) and a focus on knowledge and its material expression in durable craft items offers an empirically more solid, and at least for archaeologists much more useful, starting point. Placing knowledge and know-how firmly at the centre of an evolutionary approach to culture further highlights the conceptual ground that this approach shares with other archaeological paradigms, especially agentive ones (Riede 2005; VanPool 2008; VanPool and VanPool 2003). Although traditionally seen as theoretically conflicting, Apel (2008: 95) has recently noted that, encouragingly, "this division of interest has diminished as researchers use the operational chain approach in studies of evolutionary aspects of artefact continuity and change". As pointed out repeatedly by Shennan (1989, 2004a, b), these approaches are in fact complimentary, with the evolutionary framework, providing a diachronic perspective on individual action.

Actions are executed by individuals (Dobres 2000). In order to construct ATUs grounded in empirical values, we must therefore begin with individual artefacts. The approach adopted here largely parallels those by O'Brien and colleagues (Darwent and O'Brien 2006; O'Brien et al. 2001, 2002) and Buchanan and Collard (2007, 2008a, b), except that it foregrounds the relation of technological action to the attributes used in the analysis. I focus on lithic projectile points from the Southern Scandinavian Late Glacial. Projectile points in general are often sensitive culture-historical markers (Beck 1998) and the Southern Scandinavian data-set is no exception (Fig. 13.3): "There are several grand changes in lithic projectile points that provide horizon markers for all of northwestern Europe" (Price 1991, 198). On the basis of these changes, the culture-historical sequence consists of the Hamburgian culture ("Classic" and Havelte facies), and the Federmesser groups (FMG), followed by the Bromme and Ahrensburgian cultures (see Eriksen 2002; Terberger 2006). Yet, as Fischer (1993, 52) points out "knowledge of the geographical and chronological range of the four groups is as yet very limited. As a result, any attempt to assess their inter-relationship must remain preliminary". While some workers (e.g. Madsen 1996) have suggested that repeated episodes of colonisation or *landnam* may have shaped this picture, others see the process as one of gradual, continuous and autochthonous adaptation of local human groups to slowly ameliorating climatic conditions (e.g. Fischer 1989a, 1991). However, recent advances in environmental science have led to a major refinement of traditional time-averaged climatic models based largely on relatively low-resolution pollen analyses (Björck et al. 1998; Blockley et al. 2006; Burroughs 2005; Eriksen 2002). In particular, recent revisions of the dating for some of these techno-complexes (Grimm and Weber 2008) and the growing recognition that the Laacher See volcanic eruption, dated to c. 13,000 BP (Baales et al. 2002; Blockley et al. 2008), had a major impact on the culture-historical development in the area (Riede 2007a, 2008a)

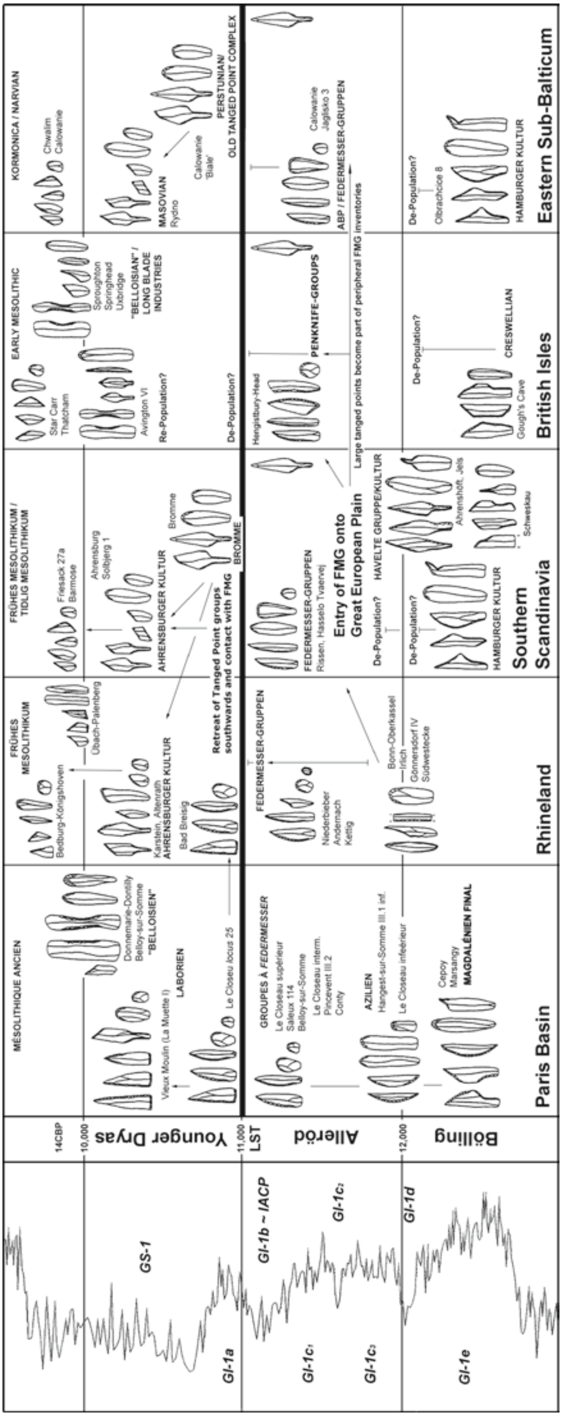

Fig. 13.3 A culture-historical scheme for some regions in Northern Europe, juxtaposed to the high-resolution δO[18] temperature proxy record of the GISP2 ice-core. The scheme is originally based on Baales (2002), but modified in light of recent work (see text and Riede 2007b)

make a reassessment of the Southern Scandinavian Late Glacial both pressing and timely (see Gramsch 2004). Despite over 150 years of archaeological research in Southern Scandinavia (Jensen 1982; Klindt-Jensen 1975), we remain "stymied…by our lack of basic taxonomic knowledge of the parts that make up the things we identify as societies" (Pocklington 2006, 30). It is this lack of taxonomic clarity then, rather than a particularly patchy database, which makes unravelling the processes that have shaped the Late Glacial archaeological record in this region so difficult.

For this study, a database of 607 projectile points was collated. Each specimen was examined for a suite of 23 qualitative/technological and quantitative/metric traits (Fig. 13.4 and Appendix 13.1). Using exploratory data analysis (e.g. Tukey 1977), an appropriate coding schemes for each character was devised. Interestingly, this approach has recently been shown to not only facilitate a reasonably faithful (yet strictly quantitative) discrimination of artefact classes, but that these classe can match onto meaningful emic definitions (Abramov et al. 2006; see also Begossi et al. 2008 for a discussion of how biological folk taxonomies correspond to those derived using biological scientific principles). In an initial analysis, the NETWORK

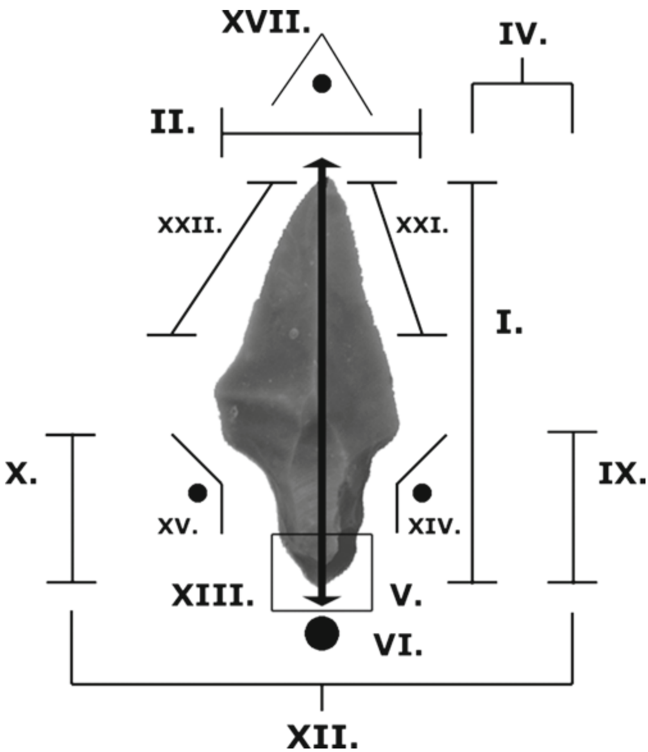

Fig. 13.4 An example of the lithic armatures measured for this study. Photo by the author with permission of the National Museum of Denmark, Copenhagen. All attributes measured and those used in the final phylogeny-building exercise are listed in Appendix 13.1

software (www.fluxus-engineering.com) was used to construct a phylogenetic network, based on the entire database. Phylogenetic networks are powerful new tools in phylogenetic analysis, specifically designed for dealing with large amounts of data, and for investigating the reticulating or horizontal transfer of genetic information (Bandelt et al. 1999; Morrison 2005). Recent studies in cultural and linguistic phylogenetics have applied these (Forster and Toth 2003; Nakhleh et al. 2003, 2005; Riede 2008b) and similar (Bryant et al. 2005; Lipo 2006) methods to counteract the long-standing criticism that reticulation and blending in cultural evolution obscure the historic/phylogenetic signal in cultural data beyond retrieval (Brew 1943; Kroeber 1917; Moore 1994; Terrell 1988). The results presented in Fig. 13.5 show that although the network is no doubt complex, it does show significant tree-like structure. Moreover, the phylogenetic analysis successfully recovers the broad, traditional typological categorisations, but provides an explicit hypothesis of how these are related. The most salient feature of the network graph is the different structures found within the four techno-complexes: the more clustered patterns in the Bromme culture, for instance, imply fewer strictures on flint knapping, its teaching

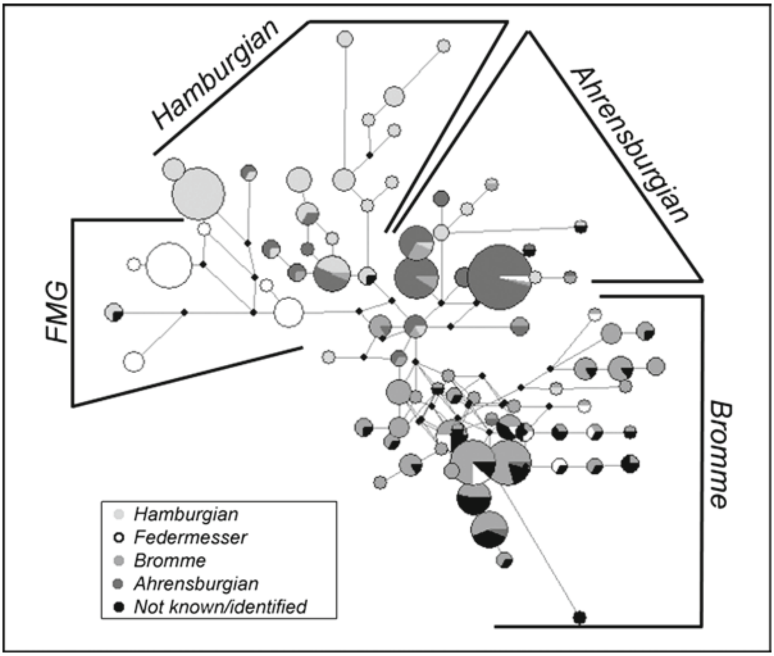

Fig. 13.5 A phylogenetic network of Late Glacial projectile points from Southern Scandinavia. Node size is proportional to the number of actual artefacts that fall into it and each node is shade-coded by its typological composition. The typological assessment of the excavator or curator is followed in this. Note the contrast between, for instance, the Bromme cluster with many smaller but highly connected nodes and the Hamburgian cluster showing a more linear arrangement with, on average, larger nodes. Guidelines for the interpretation of phylogenetic network graphs are given by Bandelt et al. (1995, 1999)

and execution than, for instance, in the Hamburgian. This notion is supported by broader technological analyses that view Bromme technology as relatively "straightforward" (Madsen 1992, 128), "wasteful" (Fischer 1991, 116) and "simplified" (Barton 1992, 192), while the Hamburgian flint technology as highly elaborate, "a more complex technology, perfectly fitted to having scarcer and perhaps more distant and varied lithic resources" (Madsen 1992, p. 128). This complexity is somewhat paradoxical in light of the ready abundance of high-quality flint in Southern Scandinavia (Madsen 1993), but can be explained in term if historical inertia – a reflection of the Magdalenian ancestry of Hamburgian groups (see Burdukiewicz and Schmider 2000; Schmider 1982). Alternatively, the linear arrangement of Hamburgian clusters may indicate successive bottlenecking in small populations under fairly tight regulation of craft production.

Incorporating the entire range of technological variability in the dataset used here is conceptually attractive, but it distracts from the overall goal of defining ATUs or cultures. Although individual idiosyncrasies are clearly critical for cultural evolutionary processes by generating variation on which selective processes can act, it is repeated behavioural patterns and consistent trans-generational teaching and learning that are of interest here (see also O'Brien et al. 2002). The NETWORK software provides useful statistical output which allows a stepwise exclusion of characters from the analysis in order to refine the phylogenetic signal (see Riede 2007b for further details). The exclusion of highly variable characters and the focus of stable "constellations of knowledge" (Keller and Keller 1996) reduce the dataset to a matrix of 16 ATUs, each defined by 12 characters (Table 13.1).

For the analysis of such smaller datasets, a number of techniques are available (Felsenstein 2004; Hall 2004). Many archaeologists have used tree-building methods to investigate both variability in stone artefacts in general (Cziesla 1998; Kind 1992) as well as specifically European Late Glacial cultural differentiation (e.g. Burdukiewicz 1986; Burdukiewicz and Schmider 2000), but invariably these were the so-called phenetic approaches, which are inadequate for distinguishing historical relatedness (Brooks and McLennan 1994). Both parsimony-based as well as maximum likelihood (ML) approaches can be used to generate evolutionary trees, and both methods can be used in the context of defining archaeological cultures phylogenetically. Here, I chose Bayesian phylogenetics because it provides a statistically robust way of constructing phylogenies for use in comparative analyses (Mace and Holden 2005; Mace and Pagel 1994). Bayesian statistics has already been introduced to archaeology in the area of radiocarbon calibration (e.g. Bronk Ramsey 2009; Buck 2001) and functional artefact classification (Dellaportas 1998). They offer a means of incorporating uncertainty and prior information about the data into its analysis. Bayesian phylogenetics is ideally suited for tackling what has become known as "Galton's Problem" (Naroll 1961), first raised by Francis Galton in response to a cross-cultural analysis of marriage patterns by Edward B. Tylor. Galton objected that "some of the occurrences might result from transmission from a common source, so that a single character might be counted several times from its mere duplicates", in other words that historically related units of analysis are not statistically independent because they may be derived from a common ancestor (see Tylor 1889,

Table 13.1 Characters and character states used in the Bayesian tree-building exercise

Character	Character state	Character	Character state
I. Maximum length	0. ≤45 mm 1. 45–68 mm 2. >68 mm	VII. Tang retouch	0. Opposing 1. None 2. Same side
II. Maximum width	0. <19 mm 1. ≥19 mm	VIII. Tang symmetry	0. >2.5 1. 1.5–2.5 2. 1.0–1.4
III. Maximum thickness	0. <5 mm 1. ≥5 mm	IX. Tip retouch	0. None 1. Unilateral 2. Bilateral
IV. Size[a]	0. <39 1. 39–58 2. 59–166 3. >166	X. Combined tang/body ratio[b]	0. <23 1. 23–42 2. >42
V. Tang/body ratio[c]	0. Unilateral retouch 1. No tang 2. <2.0 3. ≥2.0	XI. Retouch extent ratio[d]	0. 4–18 1. 19–40 2. >40
VI. Percussion bulb morphology	0. Faint bulb 1. Pronounced bulb 2. Distinct bulb with scarring	XII. Tang retouch symmetry	0. ≤1.4 1. No tang 2. >1.4

[a]Size = length × width × thickness
[b]For this ratio maximum length is divided by the tang/body ratio of the specimen
[c]Tang/body ratio is the ratio between maximum length and the lowest common tang measurement of a specimen (i.e. however far retouch extends on both sides of the specimen)
[d]This is calculated by adding together the total retouch extent of a given specimen and dividing this by length multiplied with width

272). All comparative analyses, be it of cultural or biological data, are plagued by this methodological challenge and although a number of non-phylogenetic solutions have been suggested (e.g. Denton 2007; Hull 1998), the use of phylogenies as hypotheses of historical relatedness among the units under consideration allows valid statistical procedures to be developed (Harvey and Pagel 1991; Mace and Pagel 1994; Pagel 1992). Contemporary comparative methods offer an arsenal of analytical techniques that take account of Galton's objection. They are "one of biology's most enduring sets of techniques for investigating evolution and adaptation" (Pagel and Meade 2005, 235). They can also be used to examine a variety to cultural processes, and have seen increasing application by phylogenetically minded anthropologists (Mace and Holden 2005; Mace and Pagel 1997, 1994; Mace and Sellen 1997).

The results of this analysis are presented in Fig. 13.6. Not surprisingly, some of the traditional typologically defined groupings are evident. However, it is noteworthy that statistical support for the Hamburgian clades is low and that FMG and Hamburgian (esp. Havelte Group) taxa are often grouped together. In light of recent dating evidence (Grimm and Weber 2008), this can perhaps be interpreted as an

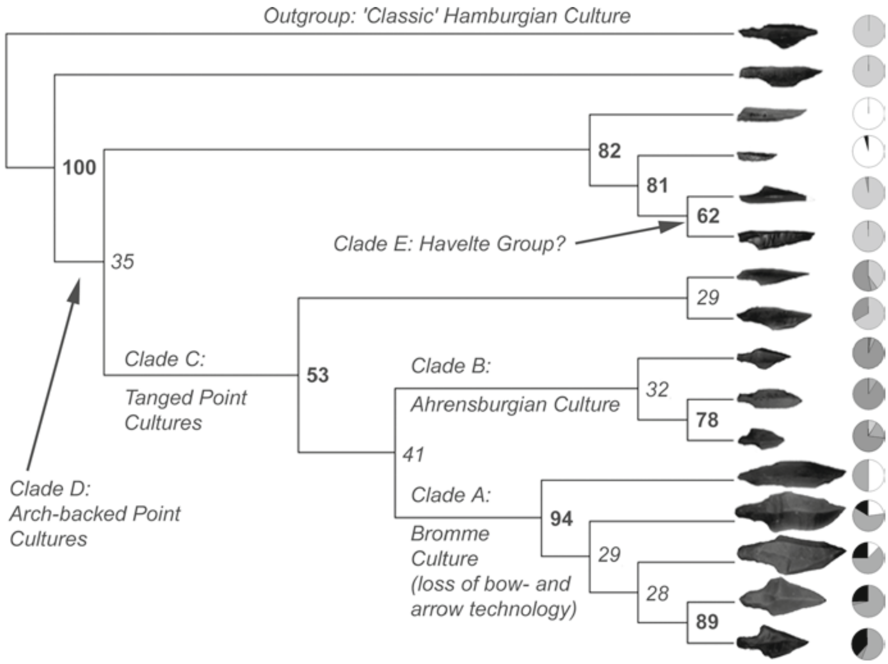

Fig. 13.6 The consensus tree based on a sample of 100 maximum likelihood trees produced using the Markov Chain Monte Carlo (MCMC) methods implemented in *BayesPhylogenies* (Pagel and Meade 2004). The model of evolution used is a simple multistate model (KSTATES), where the rates of gain and loss of the traits are presumed to be equal. A total of 10,000,000 iterations were run and the tree universe sampled at every 40,000th iteration to ensure statistical independence of each sample. This tree is rooted with the taxon that contains dates from the site with the oldest radiocarbon date in the region (Ahrenshöft: see Clausen 1998), belonging to the ("Classic" Hamburgian culture). The numbers along the branches are the posterior branch support. Note that support for some clades is rather low. This may indicate the degree to which horizontal transmission has shaped these taxa and their position. Only coherent clades with high branch support, for instance clade A (Bromme Culture), should be used to define archaeological "cultures". As in Fig. 13.5 the pie charts behind each taxon show the typological composition of each taxon. The mixed composition of some taxa may be due to mis-classification of specimens, in particular those derived from older excavations. The difficulty of distinguishing morphologically and technologically between some Hamburgian and Ahrensburgian artefacts, for instance, has created some confusion about the relatedness of these groups. Before radiocarbon dates became available, the Ahrensburgian was commonly seen as a direct descendant of the Hamburgian (e.g. Bordes 1968), in part because both groups practised specialised reindeer hunting economies, but despite the fact that there are salient differences in their technology, settlement pattern and demography (Riede 2007c, 2009a). Note also the occurrence of large tanged points assigned to FMG on contextual grounds. These make up parts of the ancestral Bromme taxa and indicate that the origin of this clade or culture must be sought in the Federmesser groups of the middle Allerød (Riede 2007a, 2008a)

indication of significant interactions between these northern and southern groups, respectively. The Bromme clade is both robust and highly diverse. This clade was the result of the isolation of northern groups following the eruption of the Laacher See volcano and the subsequent demographically mediated loss of bow-and-arrow technology as well as more complex stone working skills (Riede 2007a, 2008a). Indeed, this culture can be defined as a monophyletic clade (an ancestor and all its descendants). Interestingly, the tanged point groups (the Bromme [clade A] and Ahrensburgian [clade B] taxa in clade C) are subgroups of the Arch-backed Point complex (ABP: the Late Magdalenian and Federmesser tradition; clade D). Phylogenetically, they are not therefore equivalent units and cannot be separated at the level of a "culture". Hierarchical schemes for subdividing Late Glacial cultures are not, of course, new (Kozlowski 1999; Schwabedissen 1954), but framing such a hierarchy explicitly and on the basis of individual artefact morphologies provides a useful starting point for exploring the processes that created these hierarchical patterns in the first place. For instance, elsewhere I use this phylogeny to explore whether the introduction of domestic dogs played a role in enabling and structuring the Late Glacial recolonisation process (Riede 2010b, 2011). Long presumed to have been important in this process (Eriksen 1996, 2000), this key "innovation" can be seen as part of the human constructed niche (Bleed 2006), and in investigating this niche construction process it is critically important that, following Galton, we control for the historical relatedness of the units under study (Odling-Smee et al. 2003).

While there are no straightforward means of deciding which level of branching defines an archaeological "culture", workers can now decide which clades or components of the tree may collectively be referred to as a "cultural" group. In any case, we now have an explicit hypothesis of historical relatedness of craft lineages manifest empirically in the archaeological record. Even simple trees are not straightforward in their interpretation (Sandvik 2008). The picture of Late Glacial technological diversity suggested here is perhaps somewhat more complicated than previously proffered schemes, but it arguably constitutes a significant improvement over previous unilineal, typological schemes in that it facilitates further analysis. The great strength of evolutionary analyses is that they often reveal counter-intuitive insights and that they draw our attention towards new avenues of investigation.

Conclusion

In 1847, William Whewell (1847, 637) noted that "Comparative Archaeology", along with geology and historical linguistics is a historical science that is conducted differently to fields such as physics and chemistry. Historical events, he added, are contingent, necessitating the parallel investigation of patterns *and* processes of change and causality (see also Bintliff 1999 and O'Brien and Lyman 2000 for more recent discussions). A little later, Darwin proposed a mechanism that produces these sequences of contingent changes in the biological world, and shortly after the publication of his *Origin* some 150 years ago references to this mechanism – descent with modification – became more common in some archaeological writings. Montelius, for instance, picked up on the similarities between palaeontology and archaeology suggesting that there is much methodological and epistemological overlap. However, the early part of the twentieth century saw an "eclipse of

Darwinism" (Huxley 1943, 22; also Bowler 1983) and an intellectual fragmentation of the sciences. The revolutionary rejection of essentialism and typological thinking in biology, whose implications for unit-building are profound and still debated today, had passed by the archaeological establishment (see discussions by Sackett 1991 for a discussion of this with regards to French Palaeolithic research, and O'Brien and Lyman 1999, 2000 for a more general treatment from an American perspective).

It is argued here that the bottom-up construction of units for diachronic cultural studies rooted in individual technological action provides an empirically grounded rationale for the application of phylogenetic methods to archaeological data. Although numerous scholars, especially in Eastern Europe (e.g. Kozlowski and Kozlowski 1979) have used explicit taxonomic approaches to archaeological classification and even tree-building and network methods for data analysis (e.g. Burdukiewicz 1986; Schild 1984), these were methodologically flawed. Contemporary phylogenetic principles were not readily adopted in Eastern Europe and Russia (Todes 1989) and this may be reflected in the use of phenetic rather than phylogenetic methods in archaeology. It is argued here that detailed technological analyses allow us to construct units of analysis that index "culture" as a system of social information transmission (Boyd and Richerson 1985). Placing these units in nested hierarchies of increasingly exclusive shared attribute constellations facilitates both the definition of "cultures" – strictly perhaps as monophyletic clades – as well as the comparative analysis of casual processes acting upon these units in the first place. Ultimately, which clades or clusters of clades we designate as a "culture" is an arbitrary decision. Archaeological data provide access primarily to the actions of past people as manifest in durable material culture. If we build our definitions of "cultures" from this database, any such definition will not be equivalent to those used by ethnographers. Artefact types are the common "idiom of description" (Sackett 1999, 115) in archaeology, and talking of archaeological "cultures" is certainly a useful linguistic convention. As such the notion cannot readily be abandoned, but much like the typological approach as a whole, it holds only limited analytical utility (see Bisson 2000 for an archaeological argument, and Levit and Meister 2006 for a biological one). In contrast, a phylogenetic definition of archaeological cultures based on an explicit use of ATUs that reflect past human actions renders such groupings analytically tractable.

We must not forget, however, that phylogenies are always merely *hypotheses* of relatedness based on current knowledge and characters that are specific to a given dataset. Although a phylogenetic definition of "culture" may not be epistemologically unassailable (Lee 2003; Lee and Skinner 2008), Pagel (1994, 30) has stressed that "pragmatism is a virtue in science, and…strict adherence to epistemological criteria, although laudable in principle, can often hinder rather than promote the understanding of empirical phenomena". Here, I endorse such a pragmatic stance and have presented first steps towards an operational definition of culture under the umbrella of evolutionary archaeological theory. A cultural phylogenetic framework demands explicit units, but it makes these units comparable and it opens the door to further, empirical analyses that rely on the construction of precisely such units.

The tracing of individual craft lineages for a single class of tools is the first step in a more comprehensive analysis of material culture (Riede 2008b; VanPool et al. 2008). Methods for collating and comparing phylogenies that are historically associated are available (Page 2003; Page and Charleston 1998) and hold the promise of building more synthetic pictures of cultural evolution that draw on a wide range of craft production domains (e.g. lithics, ceramics, artistic production; Riede 2009b; Tehrani et al. 2010). The conceptual overlap between the data employed, and the analytical challenges faced by evolutionary biologists and archaeologists may warrant the application of such co-phylogenetic approaches. Unfortunately, there is still considerable misunderstanding about the remit, goals and limits of an evolutionary archaeology (see Kristiansen 2004; Shennan 2004a and Henrich et al. 2008 for discussions), but with regards to the definition of archaeological cultures evolutionary archaeologists "understand the problem of units and scale, accepting that a cultural phylogeny represents in only the broadest of terms the path that most of the members of a culture followed…The key word is *broadly*; no phylogeneticist would view a cultural phylogeny using "cultures" as taxonomic units as anything but a broad picture of ancestry" (O'Brien et al. 2008, 54). The mere definition of cultures, however, is not the ultimate goal of anthropology or archaeology. What we are interested in is addressing and explaining processes of culture change in the past. It is quite clear that traditional, implicit, typological definitions of culture are analytically moribund: "culture is everything to anthropology, and it could be argued that in the process it has also become nothing" (Foley and Lahr 2003, 109). The approach outlined here suggests a rather narrower, reductionist, and knowledge-centred definition of culture. It promotes a return to a more decidedly comparative archaeology in the sense of Whewell and Hildebrand and in so doing it offers new ways to examine the processes of culture change that are at the heart of archaeological inquiry.

Appendix 13.1

List of all traits measured and calculated. For similar attempts at describing Late Glacial armature shape see Fischer (1985), Burdukiewicz and Schmider (2000), Ikinger (1998), Szymczak (1987), Madsen (1992, 1996), Hahn (1993), and Beckhoff (1967).

I. Maximum length
II. Maximum width
III. Maximum thickness
IV. Body/tang ratio
V. Percussion bulb presence and morphology
VI. Tang orientation vis-à-vis bulb of percussion
VII. Tang retouch direction, right
VIII. Tang retouch direction, left
IX. Tang retouch length, right

X. Tang retouch length, left
XI. Hafting notch (presence/absence)
XII. Tang symmetry
XIII. Tang alignment vis-à-vis midline
XIV. Shoulder angle, right
XV. Shoulder angle, left
XVI. Tip angle
XVII. Tip retouch intensity, right
XVIII. Tip retouch intensity, left
XIX. Tip retouch direction, right
XX. Tip retouch direction, left
XXI. Tip retouch length, right
XXII. Tip retouch length, left
XXIII. Tip alignment

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Chapter 14
Archaeology and Cartography: In Search of the Prehistoric Cultures in the Neolithic Near East

Olivier Aurenche and Stefan K. Kozlowski

Introduction

The time has gone when a sole "fossile directeur" was enough to characterise a prehistoric culture. The time of assemblages has come, when what is commonly called a "prehistoric culture" corresponds instead to a combination of several elements of material culture (those that survive to be accessible to the prehistorian). This involves not only chipped lithic industries, which are the most commonly analysed aspect of the archaeological record, but also the heavy stone industry, the bone industry, "artistic" expressions (e.g. ornaments, figurines, statuettes, statues, paintings), funerary practices, architecture and ceramics, etc. (the question of subsistence economy, i.e. hunting–foraging vs. farming can also be mapped but has been left aside for the time being). All of these elements exist, to varying degrees, in all of the "cultures" grouped under the label the "Neolithic of the Near East". Successive discoveries and research undertaken during the second half of the twentieth century have shown that these cultures were not identical but instead could be distinguished from one another: while they shared common traits (e.g. arrowheads, chipped axes, roundhouses, bone needles, etc.), what Kathleen Kenyon found in the 1950s at Jericho did not correspond exactly to what Robert Braidwood discovered about the same time at Asiab or Jarmo or to what Jacques Cauvin was excavating at Mureybet during the 1970s. What was observed on the then "unique" sites could now be extended to the scale of a larger geographical territory, the extension and limits of which had to be delineated. The pursuit of research from the 1980s onwards at last allows, as a working hypothesis, the first cartography of these cultures to be proposed (Kozlowski and Aurenche 2005; Aurenche 2007). The goal of this paper is to describe the methodology involved. This is presented in four stages.

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B.W. Roberts and M. Vander Linden (eds.), *Investigating Archaeological Cultures: Material Culture, Variability, and Transmission*, DOI 10.1007/978-1-4419-6970-5_14,

Selecting the Relevant Elements

The first stage is the selection of elements which are considered to be representative of a given culture in order to distinguish those elements which can be regarded as discriminative from those which belong to what could be called the Neolithic common stock or *koine* (Aurenche and Kozlowski 1999). For instance, in the

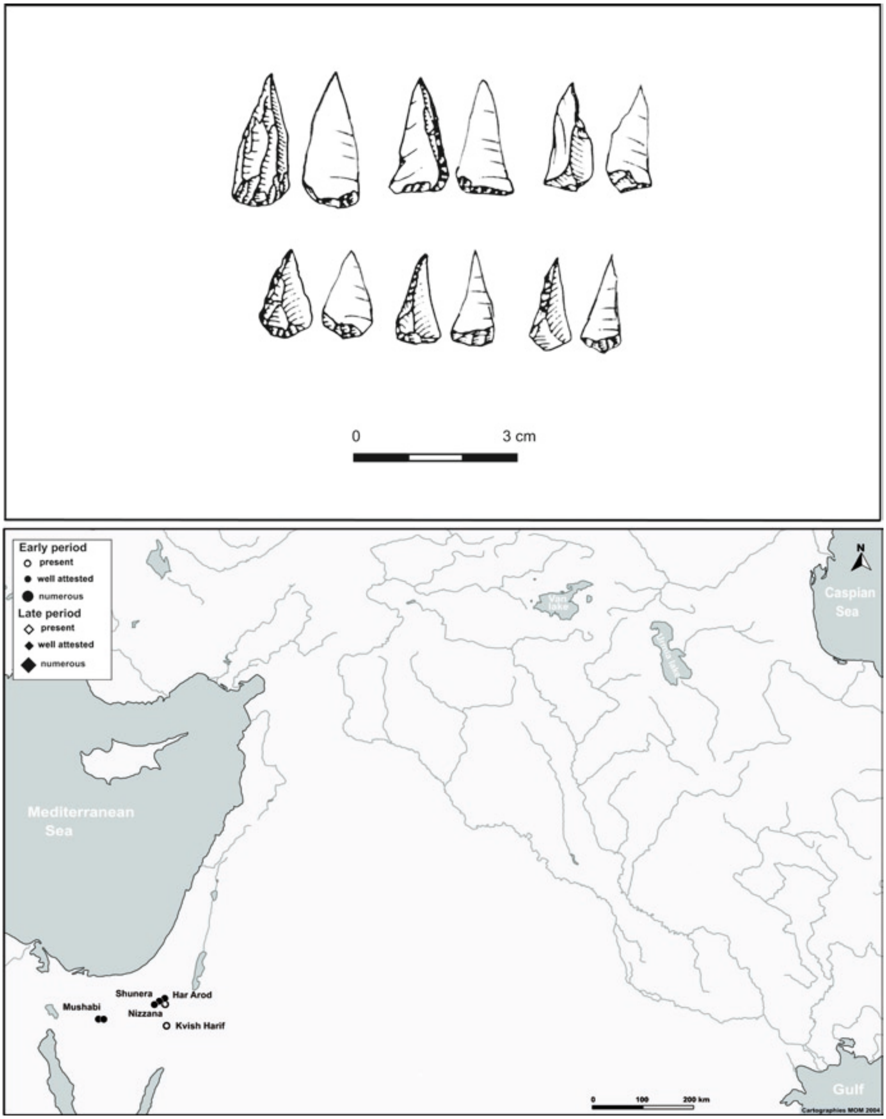

Fig. 14.1 Shunera points. Type described by N. Goring-Morris. Date: first half of the tenth millennium cal. BC; Geographical distribution: Sinai and Negev. Remarks: rare; of local significance, correlation with Harif and Ounan points. *Top*: examples after N. Goring-Morris. *Bottom*: geographical distribution

chipped lithic industry, this common stock comprises burins and scrapers, most of the retouched blades as well as certain points. These are thus not included in the analysis. On the contrary, the majority of arrowheads are considered: 19 types with distinctive morphology can be used (Figs. 14.1–14.3), as well as geometric elements or sickle forms. From a technological point of view, lithic cores as well as

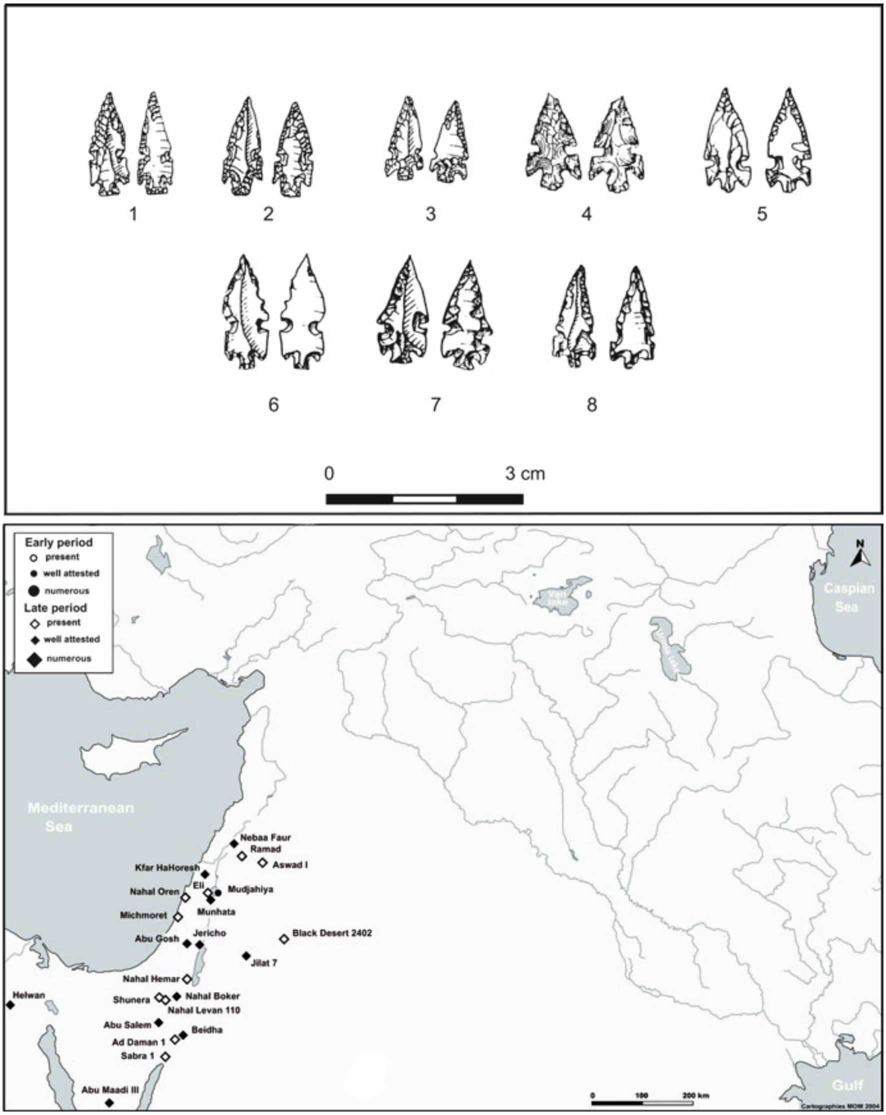

Fig. 14.2 Abu Salem points. Southern variant of the Helwan points. Date: end of the ninth and eighth millennium cal. BC. Geographical distribution: central and southern Levant, also in northern Egypt. Remarks: common; of regional significance, correlation with Jericho points, Yarmukian sickles, Tahounian axes, Beit Taamir knives and bell-shaped pestles. *Top*: 1, 2 and 3 – Nahal Lavan 109; 4 – Abu Gosh; 5 – Nahal Hemar; 6, 7 and 8 – Abu Salem. *Bottom*: geographical distribution

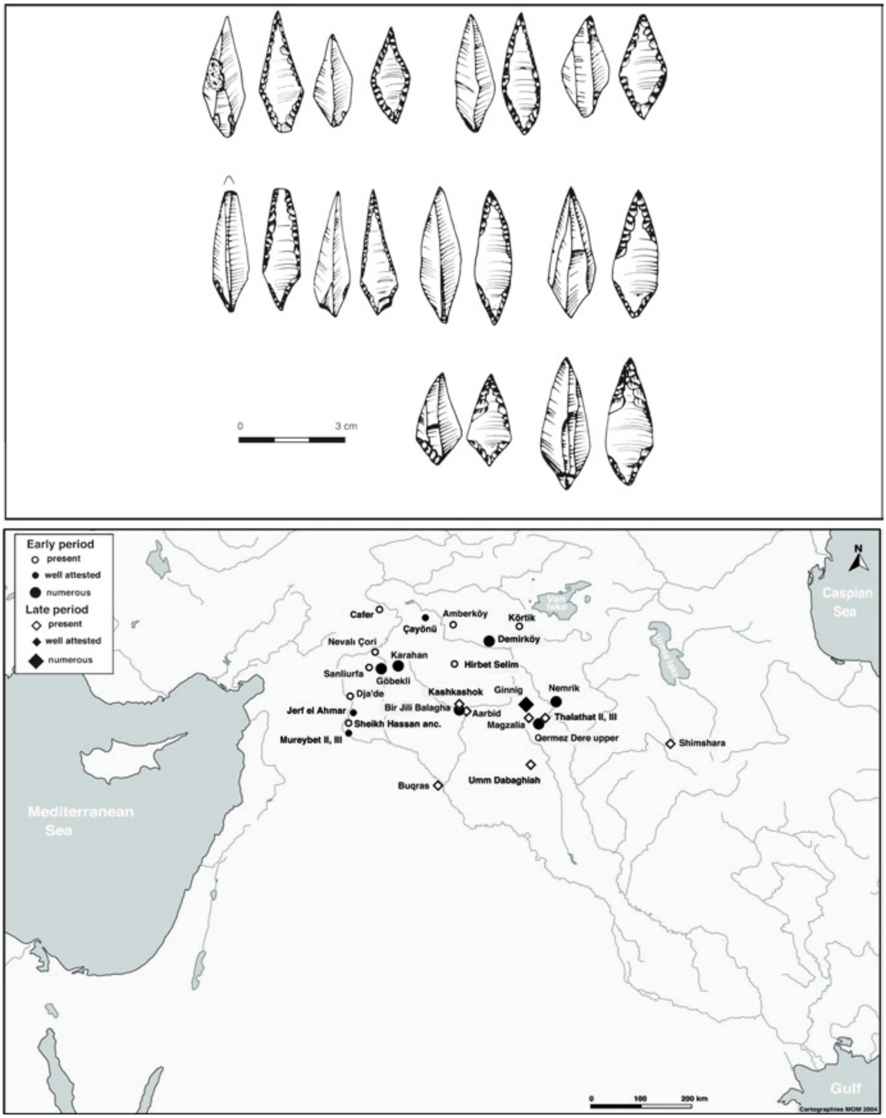

Fig. 14.3 Nemrik and related points. Type described by S. Kozlowski. Date: Tenth and ninth millennium cal. BC and later (Shimshara, Thalalat). Geographical distribution: Jezirah, mostly east of Khabur. Remarks: common; of supra-regional significance with Demirköy points, triangular sickles, Çayönü tools, felines and bird of prey statuettes and mushroom-shaped tokens. *Top*: Nemrik. *Bottom*: geographical distribution

certain debitage products allow relatively precise morphological schemes though these have not been taken into consideration in the present study. For the non-chipped stone industry, several elements must be considered: mortars and pestles, stone vessels and "ornaments". Other elements comprise forms from the bone industry, figurines, statuettes as well as both human and animal statues. Certain architectural characteristics, as well as, when present, ceramics, have also been included. In total, more than 160 elements have thus been considered as discriminative. Their cultural weight changes according to their rarity: the less frequent the object, assuming that it possesses "original" details which can be identified, the more characteristic it is of the human group which produces it. Therefore, the increase in the number of these categories of artefacts on the same territory, though not numerous by definition but highly recognisable, raises the chances of identifying a group and distinguishing it from its neighbours. This is indeed less an internal than an external perspective – what exists among some groups but not others, nearby or far away – that lies, in the absence of other criteria, such as languages, costumes, everyday practices, which are beyond reach of the prehistorian, the potential identities of prehistoric human groups (Barth 1969).

Chronological Evolution

The second stage is the inclusion of the chronological and cultural framework over a given timespan. By convention and for simplicities sake, we have only retained two periods, that before and after 8300 cal. BC. The latter date corresponds mainly to the general economic changes throughout the Near East, such as the definitive acquisition of agriculture and animal husbandry and the important modifications in the lithic industry and settlements (Aurenche and Kozlowski 1999). Using the ASPRO chronology (Hours et al. 1994), what we call the "early period" corresponds to period 2 and the first half of period 3 (10500–8300 cal. BC) which is, in the traditional Near Eastern terminology, the Pre-Pottery Neolithic A (PPNA) and the early Pre-Pottery Neolithic B (PPNB). The "late period" covers the second half of period 3 and periods 4–6 of the ASPRO chronology (8300–6400 cal. BC) which corresponds to the middle and late PPNB and the beginnings of the Pottery Neolithic (PN).

Creating Base Maps

The third stage consists of recording on maps the provenance, site by site, of the elements considered as pertinent. This corresponds, in this case, to the creation of 168 different maps. Aside from the chronological distinction, with elements from the early period represented by circles and elements from the late period by lozenges, the differences in their frequency on each site have also been indicated, whether numerous, well attested or merely present. At this stage, the role of statistical empiricism remains important, since the precise counting of each category of lithic tools or ceramic potsherds does not occur in all publications with the same level of precision. The differential frequencies allow, however, the distinction, should the need arise, of central zones, the possible origins or epicentres of these cultures, and the peripheral and marginal zones (see below).

The creation of this first set of maps, which we call the analytical series, is the longest stage as it requires either the coverage of a considerable amount of dispersed published material, the direct study of unpublished collections, or advice provided by colleagues. This stage is, however, essential as it provides the baseline for any further inquiry and allows, at any time, the required data auditing.

From this stage onwards, a glance at the maps allows the distinguishing of different geographic distributions, from the micro-region (e.g. Fig. 14.1 for the Shunera points) to a larger area, such as the southern Levant (e.g. Fig. 14.2 for the Abu Salem points), or even to the eastern wing of the Fertile Crescent (e.g. Fig. 14.3 for the Nemrik points). This first distinction allows the organisation of the data into a hierarchy based on the discriminative cultural value of each of the considered traits, thus tests the validity of the criteria used in the definition of a prehistoric culture (i.e. its sole material culture). Three scales of distribution have thus been recognised: the supra-regional scale corresponding to the entire western or eastern wing of the Fertile Crescent, the regional scale which is approximately 500 km² and the sub-regional or local scale, which is restricted to about 100 km². The first distinction shows the division into two distinctive "worlds" which, at first sight, could have been considered as homogeneous. When the chronology is added, it appears that several tools "cross-over" both periods, such as the so-called Tahounian axes (Fig. 14.4) or alternatively are limited to a sole period as for adzes, in the early period, (Fig. 14.5).

Playing with the Base Maps

The next stage consists in "playing" with the base maps, by superimposing two or several of them in order to create synthetic maps. This process confirms the first impressions: for instance, by placing seven elements (four types of arrowheads, a type of statuette, a type of stone bead and a type of bone hook) on the same map, it appears that their distribution is restricted to the western wing of the Fertile Crescent (Kozlowski and Aurenche 2005, 39; Fig. 14.6). Conversely, seven other elements (four types of microliths, a type of core, a type of stone bracelet and a type of figurine) either do not go beyond or only do so in a sporadic sense, the eastern wing of the Fertile Crescent (Kozlowski and Aurenche 2005, 40; Fig. 14.7). These examples, among others, illustrate better than lengthy developments the major division which structures the Near Eastern Neolithic.

It is possible to subdivide further: seven elements (two types of arrowheads, a type of architecture, a type of adze, a type of grooved stone interpreted as an arrow straightener, decorated stone vessels and stone sculpted statues) characterise the sole northern area (northern Levant) of the western wing of the Fertile Crescent (Kozlowski and Aurenche 2005, 42; Fig. 14.8). Likewise, ten other elements (two types of arrowheads, two types of architecture, a type of sickle, a type of axe, a type of mortar and pestle, a type of stone bracelet and earth-made statues) are only found in the southern part (southern Levant) of the same western wing (Kozlowski and Aurenche 2005, 43; Fig. 14.9). The rare occurrences outside this area are probably related to convergences or "exchanges".

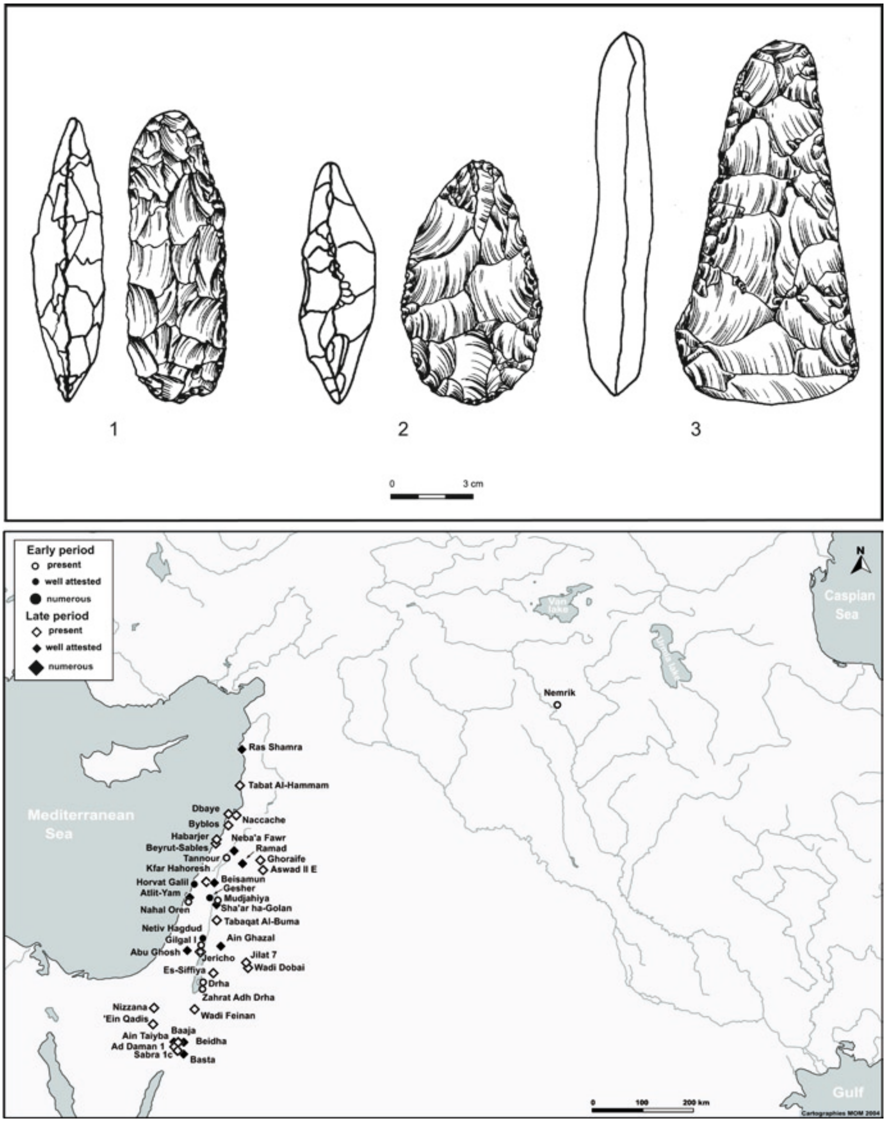

Fig. 14.4 Tahounian axes. Type described by J. Crowfoot-Payne. Date: Tenth and ninth (Jericho A, Gesher), eighth (Basta, Aswad) and seventh millennium cal. BC (Sha'ar ha-Golan, Neba'a Fawr). Geographical distribution: central and southern Levant. Remarks: common; of regional significance, correlation with Abu Salem points, Jericho points, Yarmukian sickles, Beit Taamir knives and bell-shaped pestles. *Top*: 1 – Jericho; 2 – Ramad; 3 – Beisamoun. *Bottom*: geographical distribution

The second result of this combination of maps is to reveal the existence, not only of "territories", but, as a consequence, of "frontiers". These are either empty areas (although it is always possible that these are caused by the absence of excavations and/or surveys) or, more often, buffer zones where there is a relative interpenetration

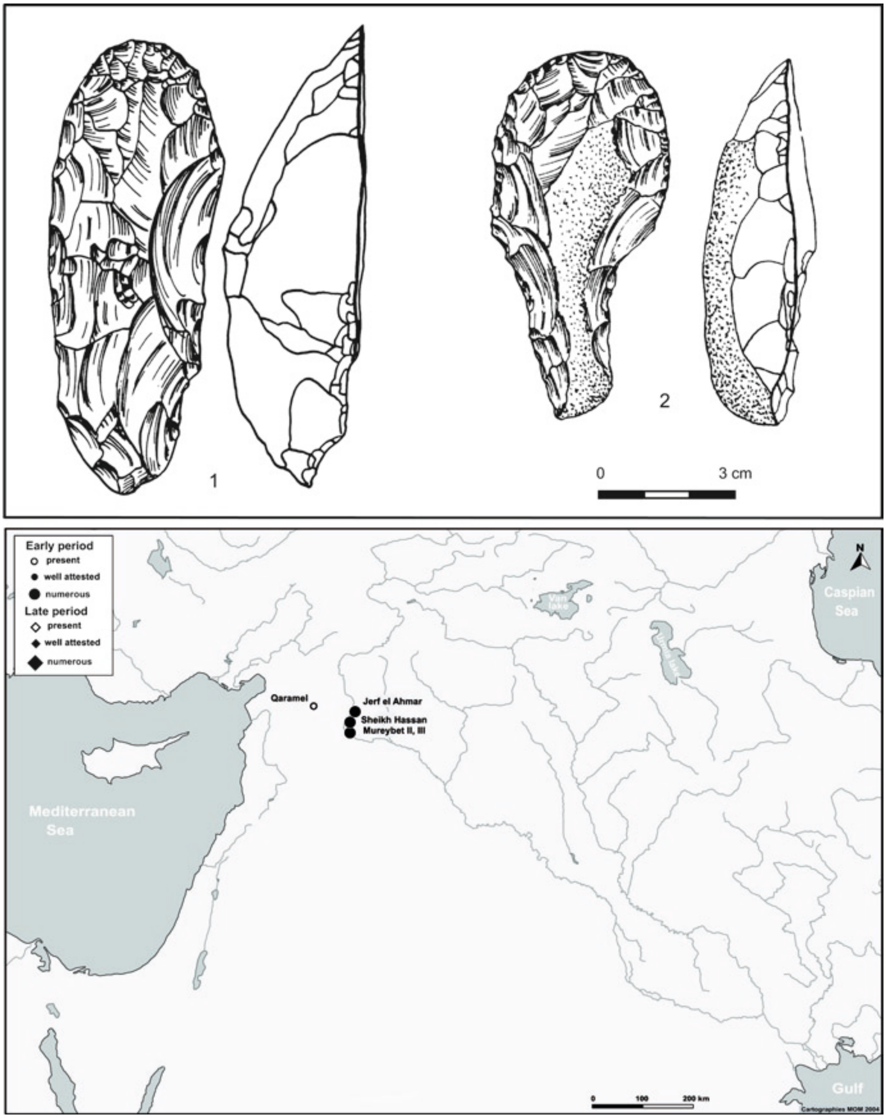

Fig. 14.5 Herminettes (adzes). Type described by J. Cauvin. Date: Tenth and ninth millennium cal. BC. Geographical distribution: northern Levant. Remarks: common; of local significance, correlation with Qaramel points, pedunculated pestles, decorated shaft straighteners, richly decorated stone vessels and stone statues. *Top*: Mureybet. *Bottom*: geographical distribution

of elements from one territory to the next, usually of about 50 km – negligible at the Near Eastern scale. In this case, the attention paid to artefact densities can be extremely useful: abundance on one side of the frontier can be contrasted with insignificant numbers beyond it. From the first period to the second, the main frontier between the western and the eastern wings of the Fertile Crescent has moved between the Euphrates and the Balikh rivers, about 50 km westwards or eastwards

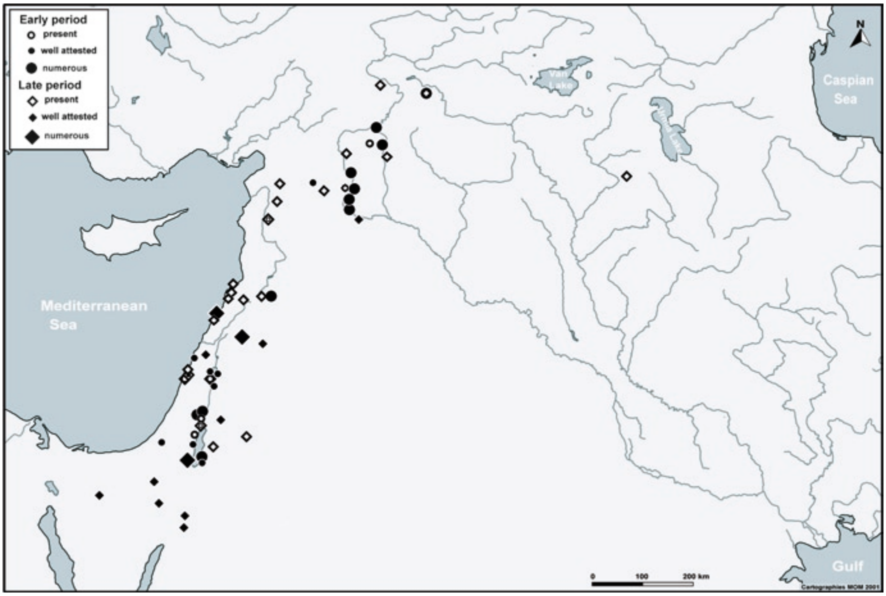

Fig. 14.6 Distribution of diagnostic material traits for the western wing of the Fertile Crescent

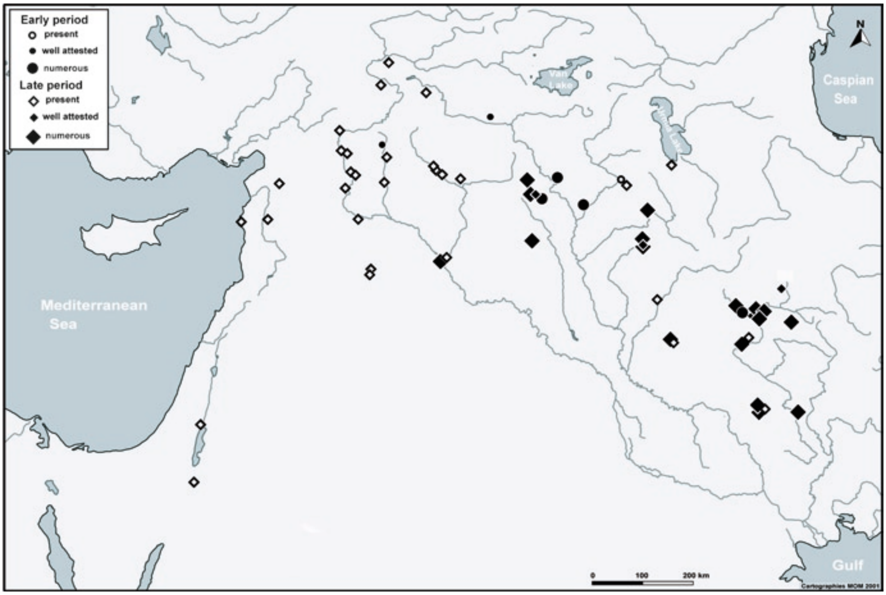

Fig. 14.7 Distribution of diagnostic material traits for the eastern wing of the Fertile Crescent

(Kozlowski and Aurenche 2005, 49–51; Fig. 14.10). Frontiers of the second rank have also been delineated, for instance, between the northern and southern Levant which were separated by the hydrographic basins of the Orontes to the north and the Jordan rivers to the south (Kozlowski and Aurenche 2005, 54–55), or between

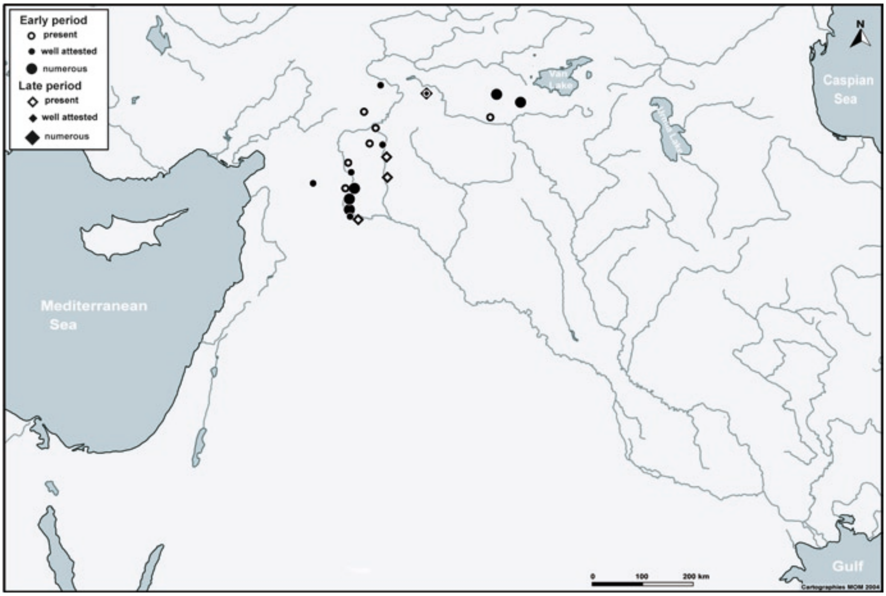

Fig. 14.8 Distribution of diagnostic material traits for the northern Levant

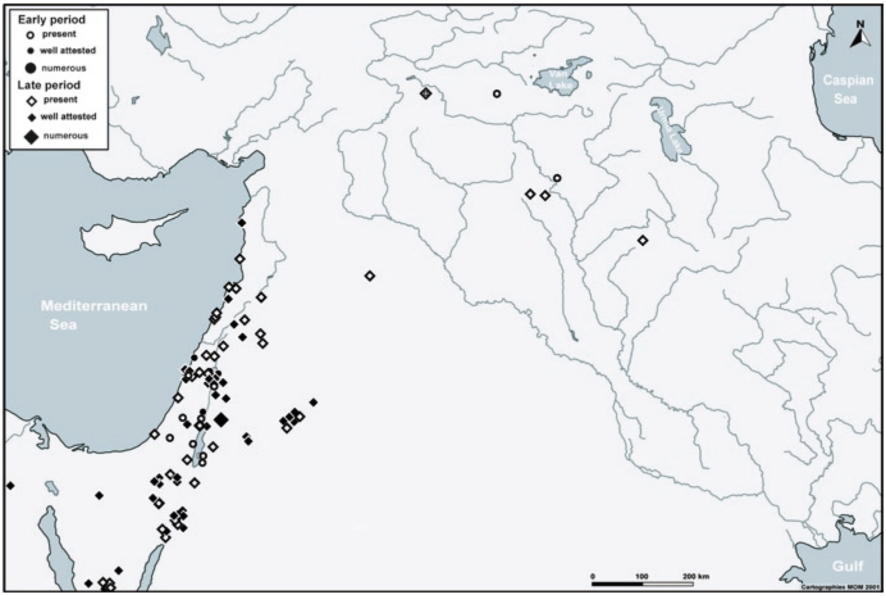

Fig. 14.9 Distribution of diagnostic material traits for the southern Levant

the upper valleys of the Tigris and Euphrates rivers, and the rest of the Fertile Crescent. Frontiers of the third rank can be seen in the southern Levant (Kozlowski and Aurenche 2005, 56–57) or in the Zagros Mountains (Kozlowski and Aurenche 2005, 58–60).

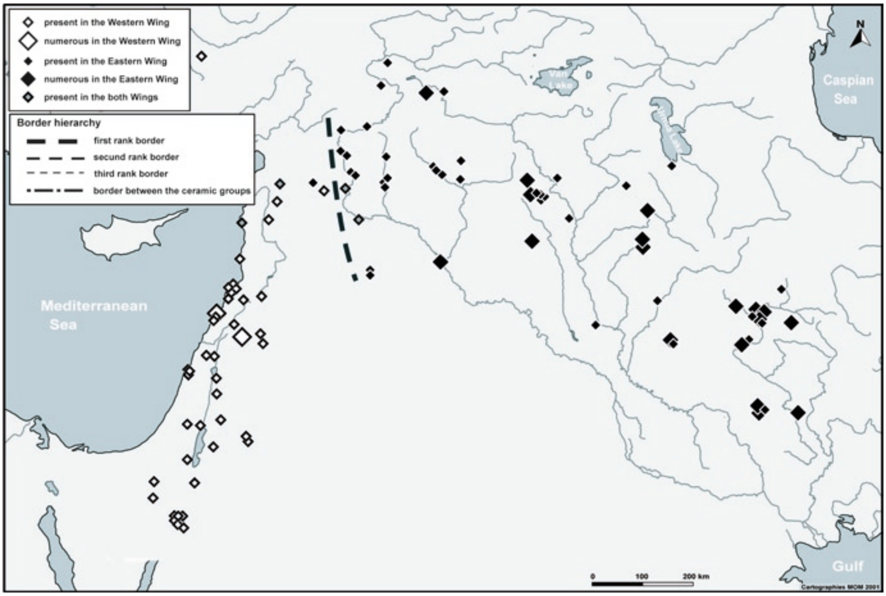

Fig. 14.10 Alternative main border between the western and eastern wings of the Fertile Crescent in the Late Period

At this stage, it is possible to come back to what, in the absence of a better term, is called a "prehistoric culture". Can the prehistorian translate this notion of culture in terms of "tribe" or "ethnic group", or indeed any other social or political system? This question is addressed in the preface written by F. Hole for our book (Kozlowski and Aurenche 2005, 9–10). In the absence of determining elements, for instance, the knowledge of the language or the access to a world-view of magical or religious nature, any attempt seems to be doomed to failure, unless one assigns arbitrary fantasies which are more informative of those who project them – prehistorians – than of those who are supposedly studied – prehistoric people.

Our ambition is more modest: we only can attest that a human group using the same assemblage, one that is statistically representative in terms of diverse material elements but which are identified in excavations, has lived on the same territory for an amount of time which remains to be determined. The chronological question is indeed crucial. Our perception, despite the multiplication of ^{14}C dates, remains approximate as no one can affirm that two sites observed in a same territory are rigorously contemporary. By convention, one thus agrees to consider a culture which lasts, at best, for several centuries, as homogeneous. It is not the place here to solve this problem.

Through the cartographic analyses, we can identify, for the early period (10500–8000 cal. BC), eight "cultures" or "taxonomic entities" for the entire Near East (Fig. 14.11). Each possesses several characteristics with, in decreasing order, elements common to the whole of the area; elements which are only found in one or several neighbouring territories; and lastly, elements peculiar to this culture, in relative quantities, from 1 to 11 (Kozlowski and Aurenche 2005, 67–71). For the south-west

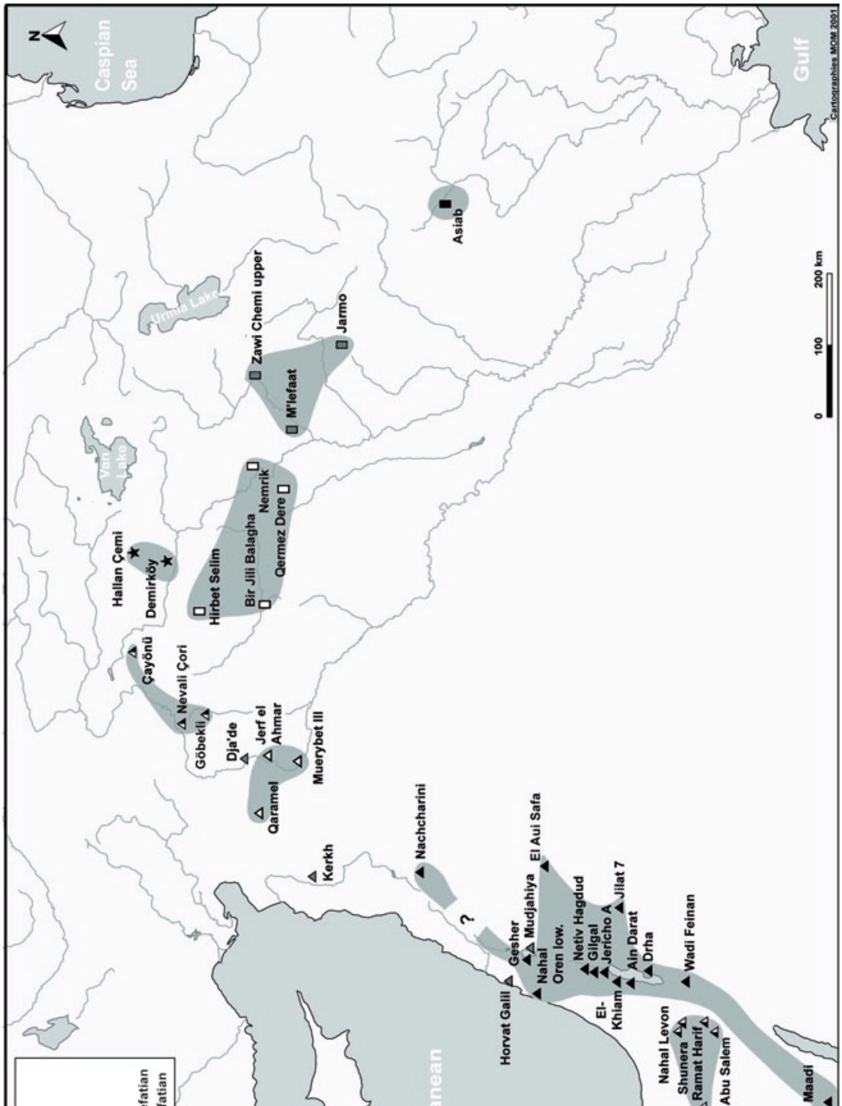

Fig. 14.11 Territories, borders and cultures in the Early Period (10500–8000 cal. BC)

to the south-east, one can observe the Harifian, which occupies the Sinai, the northern Negev and extends westwards up to Egypt. Then, there is the Sultanian, which is well documented, and centred upon the Levantine corridor (Jordan Valley, Wadi Araba and the coastal plain), with a peripheral zone to the east and south. The Aswadian, which is still poorly defined, seems to be later and overlaps with the northern area of the Sultanian and the southernmost extension of the Mureybetian. It corresponds to the older phase of the PPNB. Intermediary points are missing for this area which has been described as a "black hole" (Kozlowski and Aurenche 2005, 68). In the bend of the Euphrates River, from the Aleppo basin to the sources of the Balikh, lies the Mureybetian, which is contemporary with the Sultanian, and is represented by a few well-documented sites. The Nevalı Çorian occupies the upper valleys of the Euphrates and Tigris, and its contacts with the Mureybetian are well attested. The Trialetian appears to be on the upper Tigris and an extension of a culture known from the region of the Caspian and the Caucasus, north of the Fertile Crescent. To the east, between the Balikh and the Tigris, one finds the Nemrikian, and even further east, the Mlefatian. Even if these frontiers are not entirely conditioned by natural features, as they are divided by hilly landscapes or rivers with the possible exception of the deep valleys of the Zagros, each of these territories is based upon waterways and separated by empty arid or semi-arid zones, which were probably less favourable to human occupation. Waterways thus play a triple role: privileged settlement areas, axes of circulation, but also frontiers, as exemplified by the Euphrates or the Upper Zab. The area of the territories varies, and the "empty spaces" are considerable. Is this a reflection of reality? It is still possible that these empty areas correspond to zones without sufficient fieldwork, and it is likely that new discoveries modify the extent of these territories, which we give as working hypotheses.

For the late period (8300–6400 BC), we identified ten different "cultures" with the majority being continuities of the earlier ones, with the exception of Mesopotamia where new entities appear (Fig. 14.12). Even if the extent of their territories varies, the persistence of these frontiers from one period to the next is noteworthy, which is not the less surprising results of the analysis: the frontier between the Balikh and the Khabur rivers or between the northern and southern Levant is especially striking in this analysis.

The Proof of Territories and Frontiers by Ceramics

A fundamental element confirms the validity of the territories and of the frontiers that have been defined. It is ceramics, a marker so precise that, at a general scale, it overwhelms all others, including the lithic industry. Its appearance in the Near East enables the analysis of its distribution within the territory of several already defined cultures Aurenche et al. 2004. This "proof by ceramics" is especially valuable for us given that this work has already been independently done by other scholars (Le Mière and Picon 1998). Four distinct groups have been identified during the appearance of ceramics which occurred more or less simultaneously

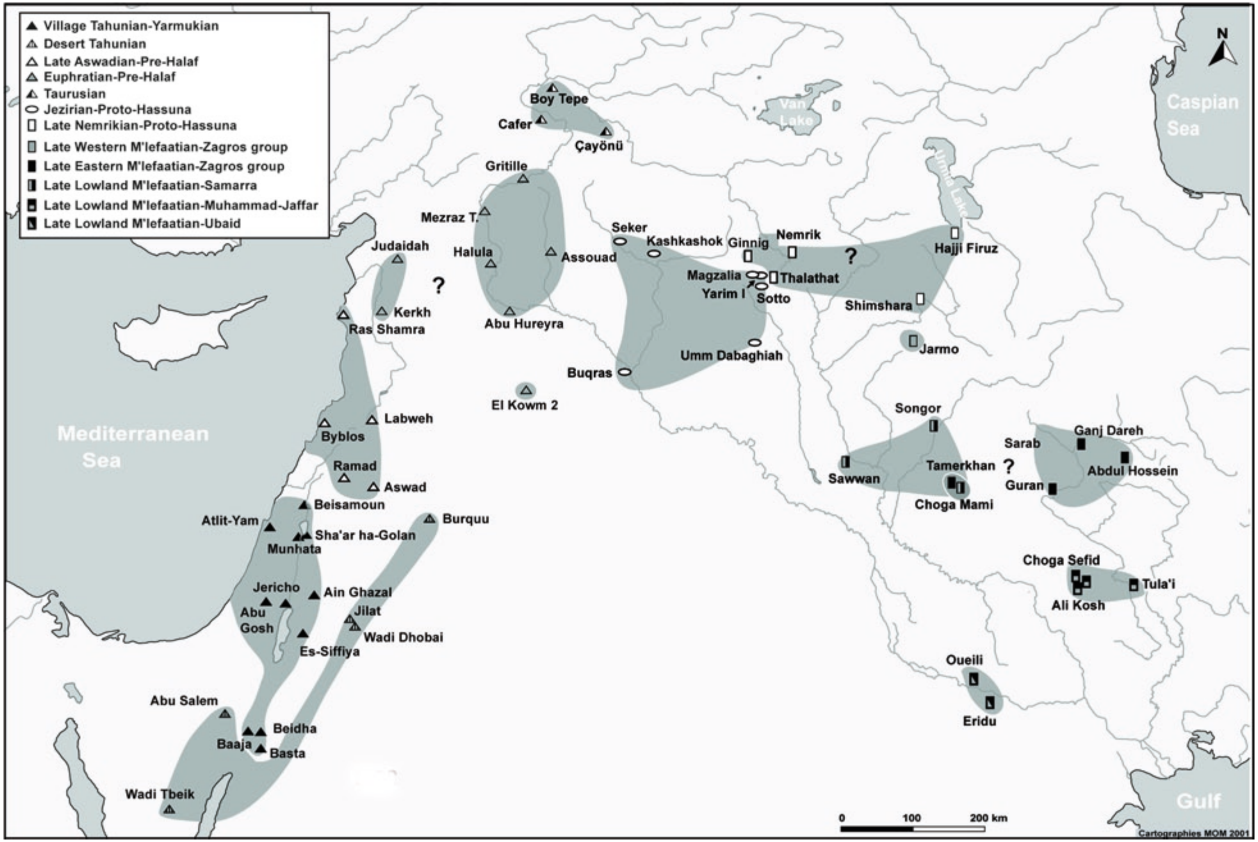

Fig. 14.12 Territories, borders and cultures in the Late Period (8300–6400 cal. BC)

across the Near East around 7000 cal. BC: the Zagros group, which covers the territory of the Mlefatian; the proto-Hassuna group corresponding to the Nemrikian tradition; the pre-Halaf group, which covers the entire northern Levant (Mureybetian then Euphratian); and eventually, with a slight chronological interval, the Yarmoukian group which occupies the southern Levant (Sultanian then Tahounian). How should this coincidence be interpreted? It is too precise to be fortuitous, unless the appearance of a new technology within already constituted cultural groups developed autonomously within each community.

Another Proof by Ethnography

On the basis of ethnographic fieldwork done in Mali, and in a way following up Hole's questioning, A. Gallay has explored and mapped the distribution mode of the ceramic production of a given ethnic group (Gallay, 2007). Three zones are distinguished: "a central zone corresponding to the external limits of the matrimonial spheres and thus at the heart of the ethnic group …; a peripheral zone where potters travel beyond the limits of the ethnic group …; a zone of sporadic finds resulting only of acquisition mechanisms by individuals external to the ethnic group …"

Although undertaken at a different spatial and temporal scale, this "regularity" shown by A. Gallay seems to us, in principle, fairly similar to the interpretative attempt of the material remains suggested here for the entire Near East.

Communication and Circulation

The definition of these "territories" and "frontiers" does not imply, however, that there is no communication or circulation between these territories. A certain number of elements attest to this occurrence, such as, during the late period, several types of stone vessel which are proof of high technological skills being found from one end of the Fertile Crescent to the other (Kozlowski and Aurenche 2005, 83 and 173–175). This evidently corresponds to the transport of manufactured objects since the location of the raw materials is restricted to the Taurus Mountains and there are no signs of local manufacture. The route of these objects can even be reconstructed and does not always follow the waterways but, for instance, followed a chain of oasis through the Syrian Desert in order to reach the central and southern Levant (Aurenche and Kozlowski 2001, Fig. 14.2b).

Another, even more important, proof of the communication between territories corresponds to the circulation of obsidian: thanks to physico-chemical analysis, the exact geographical origin can now be determined (Cauvin et al. 1998; Chataignier 1998). Distribution patterns have thus been reconstructed: obsidian from the Göllü Dag, through the Syrian Middle Euphrates, supplied the western wing of the Fertile Crescent, while obsidian from Bingöl and Nemrut Dag is found, still through the Middle Euphrates, in the eastern wing and, to a lesser extent with the exception of one type only, in the southern Levant, probably also thanks to the relay of Syrian oases.

Conclusion

Is it really possible to translate in ethnographic terms this archaeological reality? At least we have brought to light the existence of human groups differentiated by their material culture, living on distinct and delineated territories, in contact with each other through the exchange of raw materials and manufactured objects. To speak of "cultures" corresponds to a generally accepted consensus, probably in the absence of a more adequate term. But to know if these groups belonged to one or several "political" entities; or if they corresponded to different ethnic groups differentiated by their languages or their world-views; if they belonged to tribal confederations; or if they corresponded to chiefdoms or kingdoms; if they practised endogamy or exogamy; if the power was hereditary or elected; if the exchanges were commercial or related to war all remains, for the time being, largely out of reach for the prehistorian. Still, this attempt of "automatic cartography", which does consider not a few isolated elements but rather the entire range of known archaeological remains, seems to us one of the surest ways to represent, in an empirical but concrete way, the mode of territorial organisation of the Neolithic populations of the Near East.

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Chapter 15
To Tame a Land: Archaeological Cultures and the Spread of the Neolithic in Western Europe

Marc Vander Linden

Seeing the Neolithic Through the Lens of Cultural Transmission

Although the meaning of the term "Neolithic" differs from one archaeological tradition to the next one, until recently, there has been a general agreement in the western European and American literature to associate the Neolithic with the emergence of plant and animal domestication (Barker 2006; e.g. compare with the Russian academic world where the Neolithic is synonymous with the presence of pottery: see Dolukhanov and Shukurov 2004). Over the past two decades, though, several researchers have challenged this economic perception of the phenomenon in the European case. A majority of archaeologists still consider the Neolithic to be synonymous with the introduction of farming and ensuing sociocultural changes (e.g. Rowley-Conwy 2004). A few scholars – mostly of the post-processual *école* – alternatively stress the idea that the Neolithic represents, first and foremost, a change in world views, wherein the domesticates play an incidental role at best (e.g. Hodder 1990; Thomas 2007, 2008; see also Cauvin 1994 for the Near East). These competing positions are further complicated by diverging visions about how the process of neolithisation is articulated: scholars of the first camp stressing the role of incoming populations, a variable which is almost denied by members of the second camp.

The variability of the Neolithic archaeological record can be cited to explain partially this divergence of opinion. During the several millennia separating its formative stages in the Near East from its establishment in the remotest parts of Europe, this phenomenon has, without surprise, undergone various profound transformations (e.g. the adaptation of farming practices to new ecological conditions, changes in material culture and associated practices). As further archaeological excavations have revealed more and more of this diversity and research strategies, both practically and theoretically, have become better equipped to deal with such

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B.W. Roberts and M. Vander Linden (eds.), *Investigating Archaeological Cultures: Material Culture, Variability, and Transmission*, DOI 10.1007/978-1-4419-6970-5_15,

complexity, there is a loose but steadily growing consensus to view the neolithisation of Europe as a “mosaic of kinds of transition: a major demographic incursion here, something more filtered and piecemeal there, and a case or two perhaps of leapfrogging, to be set alongside and integrated with transfers and adoption of practice through existing networks and among existing populations, rapid changes as the outcome of welcomed change in one area, and slow alterations as the result of prolonged resistance or indifference in another” (Whittle 2007, 622).

Becoming more focused on finding and voicing diverse (rather than grand) narratives has, however, also opened up a Pandora’s box of issues that did not previously trouble research agendas. Over and above the usual matter of untangling the palimpsests of natural and cultural information that make up the archaeological record, we are now faced with accounting for principles such as cultural transmission, through recourse to more sophisticated thinking than mere diffusion. Ecologists have come to realise that ecosystems do not advance and retreat en masse in response to climatic change, but that elements within them respond in a more individualistic fashion; so archaeologists have come to realise that individual cultural traits may well not respond to change in a normative manner either. The extent to which changes in one sphere of culture impact and affect those in another, and how cultures are formed and transmitted are new and challenging fields of archaeological inquiry. In the case of the Neolithic, the question obviously revolves around the primacy given (or not) to new agricultural practices and how they were incorporated into existing societies and economies.

The current preoccupation with disentangling and evaluating this potential interdependence of distinct traits, nonetheless, does emerge from its own long history in archaeology. In the old days when archaeological cultures meant something, changes affecting a privileged trait within a given material culture (often in its lithic or ceramic repertoire) were interpreted as evidence of a new incoming population, which was to be then chased and detected in the rest of the corresponding data. A variation in one trait was thus considered as indicative of variation in the entire material collective of a culture. A more sophisticated approach was devised by David Clarke, who considered that an archaeological assemblage should be viewed as a polythetic group, “a group of entities such that each entity possesses a large number of attributes of the group, each attribute is shared by large numbers of entities and no single attribute is both sufficient and necessary to the group membership” (Clarke 1968, 36). The last part of Clarke’s definition is of particular interest for our purposes, as it advocates a non-hierarchical approach to material culture, in which no trait is given any a priori interpretative primacy. The interpretation of the data rests instead on the unravelling of the partial or complete correspondence between the various elements considered: “[Some archaeologists], and Clarke was a prime example, obtain a pure enjoyment, similar if not identical to that of the mathematician, in the discovery of patterning – at a multiplicity of scales – in the archaeological record” (David and Kramer 2001, 31). In my opinion, despite major improvements in our knowledge of material culture, less progress has been made since Clarke’s day regarding the integration of the various strands of evidence in a coherent narrative; we have been left with multi-vocality, as an end rather than as a means to an end within archaeological interpretation.

For example, several decades of ethnoarchaeological fieldwork have illustrated the countless cultural, social or psychological factors that are at play in the variation we see in material culture. Sometimes this seems to prevent any straightforward archaeological inference (David and Kramer 2001). Owing much to the long-standing French *école* founded by Marcel Mauss and André Leroi-Gourhan (e.g. Mauss 1935; Leroi-Gourhan 1943; Lemonnier 1986), numerous studies show the intricate relationship between the variation and the transmission of material culture, especially techniques, and therefore offer detailed descriptions of modes of transmission (e.g. master and apprentice relationships: Wallaert-Pêtre 2001). Other research has insisted on the relationship between a practice being transmitted and its mode of transmission. For instance, following upon her ethnoarchaeological fieldwork in India, Valentine Roux has demonstrated that the motor skills required for the use of the potter's wheel are such that successful apprenticeship requires a lengthy time investment, only manageable in the context of an already specialised pottery production (Roux 1990). Few ethnoarchaeological studies have yet explored how the mechanisms of variation in one material trait potentially interfere with other elements (but see Sillar 1996) and are in this sense, presently, of little help in addressing the all-important question of the "patterning of material culture at a multiplicity of scales" mentioned by David and Kramer (2001, 31).

Since Clarke's work was initially influenced by the New Geography of the 1960s, it is hardly surprising that, for him, patterning was first of all a question of geographical correspondence, as evidenced by his definition of an archaeological culture as "a polythetic set of specific and comprehensive artefact-types categories which consistently recur together in assemblages within a limited geographical area" (Clarke 1968, 490). Archaeological cultures are thus a particular type of assemblage, characterised by spatial regularities. Although the spatial, as well as temporal, coherence of an archaeological culture remains a necessary dimension, these criteria are purely descriptive and, therefore, hardly informative of the processes at play in the constitution of these spatial regularities. A complementary element of interpretation can, I think, be found in another field with a profound interest in cultural variation and transmission: Darwinian archaeology. In sharp contrast to the multiplicity of processes unravelled by ethnoarchaeology, a highly specific stance on cultural transmission lies at the core of Darwinian archaeology, with the concepts of vertical (from parents to offspring), horizontal (between members of similar or different groups) and oblique (from any member of the older generation to the younger generation) transmission (Cavalli-Sforza and Feldman 1981; Shennan 1989; Eerkens and Lipo 2005, 2007). Roughly speaking, these three modes of transmission, coupled with further insights into the properties of the transmission (e.g. replication, descent with modification), provide the foundation for extensive mathematical modelling of cultural variation over time and space, that can then be applied to archaeological situations (e.g. Shennan and Wilkinson 2001; Bentley and Shennan 2003). It must be noted that in accordance to neo-Darwinian evolutionary biology, where the individual is the sole locus of genetic change, cultural transmission in Darwinian archaeology only occurs at the level of individual interactions. A mediating process is thus required in order to pass from the individual scale to a largest one, comparable to archaeological assemblages spread over large sways of time and space. In a provocative paper, Stephen Shennan has assessed the potential role of population dynamics in cultural change (Shennan 2000). After considering on the basis of selected ethnographical examples that most cultural practices are likely to follow vertical lines of cultural transmission, Shennan suggests that "the prevalence and the way [practices] change will be strongly affected by what happens to the biological population through which they are being passed on" (Shennan 2000, 813). In this scenario, cultural transmission only acts at the individual level and its extent, temporal and/or geographical, is a function of the number of individuals involved. Population thus acts as a quantitative operator (compare this, for example, with the macro-evolutionary theory advocated by Zeder 2009).

Darwinian archaeology is not exempt from criticism as, for instance, it rarely considers as pertinent the potential relationship discussed by ethnoarchaeology between the object and mode of transmission. This is because in neo-Darwinian biology, the information coded by a gene is irrelevant to the mechanism of its transmission. Yet, the emphasis on the role of population in cultural transmission and variation, as advocated by Shennan, is worth pursuing. In particular, it allows us to introduce a much needed element of scale to discussions about cultural transmission. It achieves this by suggesting that the extent of cultural variation may fluctuate, in one way or another, in parallel to changes in the demographic composition of the human communities involved (e.g. Hassan 1979). This is especially relevant to the Neolithic, as it has long been suspected that the process of neolithisation could be either the outcome or the cause of a drastic change in demographic regime (e.g. Ammerman and Cavalli-Sforza 1971; Cohen 1975). This hypothesis has recently been revived by the identification of a recurrent signal in the proportion of immature individuals in several cemeteries associated with the onset of agriculture in both Europe and the Americas, which is interpreted as an increase in the birth rate indicative of a demographic transition (Bocquet-Appel 2002, 2005; Bocquet-Appel and Naji 2006; and contributions in Bocquet-Appel and Bar-Yosef 2008).

Following upon Clarke's polythetic definition of an archaeological culture, this paper will explore the nature of the cultural variation and transmission of the "Neolithic" through a non-hierarchical approach of its various components (e.g. animal and plant domesticates, material traits and exchange networks) in western Europe, especially Britain. Particular attention will be paid to the role of population structure in the potential patterning of these various traits. The ultimate objective will be to test the validity of the current "mosaic consensus" for the Neolithic by evaluating if this pattern is randomly constructed, or rather regulated by recurrent internal mechanisms.

A Tale of Two Centres: The Late Sixth Millennium BC in Present-Day France

It has long been recognised that after its initial exit out of the Near East, the spread of the Neolithic proceeds across Europe via two main geographical routes, with distinctive culture histories. To the North, the first route can be traced across the continent.

The early Neolithic, corresponding to the Starčevo-Körös-Criş culture, diffuses during the late seventh and early sixth millennium cal. BC in the Balkans and part of the Hungarian Plain (Whittle et al. 2002, 2005). After a few centuries of stasis, the spread resumes around 5500 BC in the form of the Linearbandkeramik culture (Linear Pottery Culture; hereafter LBK). This episode of diffusion is as brief as it is fast (Dolukhanov et al. 2005) since, in the space of a few centuries, the LBK eventually covers the entire belt of loess soils which stretches from the Low Countries and the Paris Basin westwards, to the fringes of Ukraine and Russia eastwards (e.g. Gronenborn 2007). To the South, the second route is associated with the Impressa and Cardial cultures, and progresses along the coasts of the Adriatic (Forenbaher and Miracle 2005) and the Tyrrhenian Seas (Malone 2003) to reach the shores of southern France and of the Iberian Peninsula during the second part of the sixth millennium BC, if not earlier in certain cases (Zilhão 2001; Manen and Sabatier 2003).

By the end of the sixth millennium cal. BC, there are thus two distinct poles of neolithisation in western Europe, separated by a few hundred kilometres: on one side, the LBK centred upon the Paris Basin and the Belgian loess area and, on the other side, the Impressa/Cardial of the French and Iberian Mediterranean shores (Fig. 15.1).

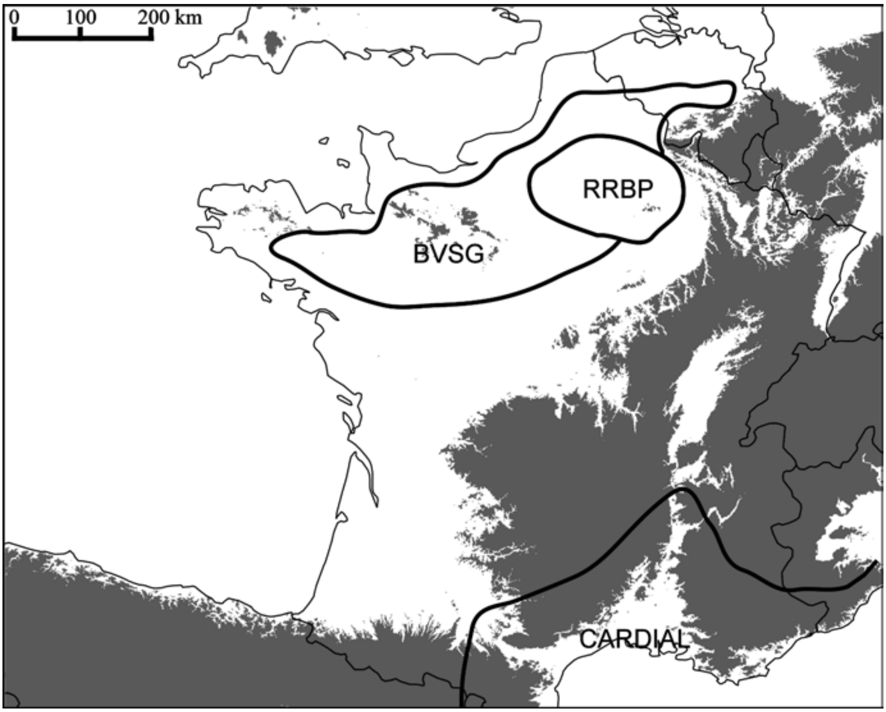

Fig. 15.1 Distribution of major French archaeological cultures for the late sixth–early fifth millennium cal. BC in France. Rubané Récent du Bassin parisien (RRBP), Blicquy-Villeneuve-Saint-Germain (BVSG) and Cardial cultures

The next sections will briefly review the idiosyncrasies of each pole, particular attention being paid to their mutual interference and their respective impact on the neolithisation of the French Atlantic façade (see contributions in Marchand and Tresset 2005).

To the South: Impressa and Cardial in Mediterranean France

The first phase of the neolithisation of Mediterranean France is confined to a handful of coastal sites attributed to the Impressa culture (5800–5600 cal BC: Manen and Sabatier 2003; Guilaine and Manen 2007). It is only in a secondary phase that we witness an expansion further inland, associated with the Cardial culture (and later on the Epicardial: Guilaine and Manen 2007). Albeit lying outside the geographical scope of this paper, it is worth mentioning that the Cardial is also the vector for the neolithisation of the Iberian Peninsula (Arias 2007). The importance of this particular culture has been stressed by Guilaine and Manen, who described it as "the result of a more structured process of development and demographic expansion, provoking a rapid transformation of the scope of identity references" (Guilaine and Manen 2007, 37).

From a ceramic typological point of view, the southern French Impressa and Cardial belong to larger cultures which encompass the entire Western Mediterranean basin. These wider Mediterranean affinities are also discernible in the subsistence practices. For instance, the faunal assemblage of the Impressa site of Portiragnes (Pont de Roque Haute) is dominated by sheep, with attestation of fishing and fowling, but near absent are traces of hunting. This restricted faunal spectrum has been interpreted as a highly specialised package brought by the first Neolithic pioneers coming from Italy (Tresset and Vigne 2007, 197). In contrast, the Cardial sites possess faunal assemblages with a more balanced husbandry with cattle and sheep (and secondarily goat and pig), as well as a relative proportion of hunting. Vigne and Tresset, however, stress the intrinsic variability in the different Cardial assemblages, function of cultural choices and local environmental adaptations: "It seems that each local Cardial population rebuilt its own Neolithic subsistence system, according to its traditions and natural environment" (Tresset and Vigne 2007, 199). From the point of view of plant domesticates, Cardial assemblages present the same general range of cereals and pulses found throughout the Mediterranean sites, which contrasts rather significantly with the LBK situation (Colledge et al. 2004; see below). The Western Mediterranean basin is also the area of domestication of the poppy seed (*Papaver somniferum*), a process which seems to have occurred during the early stages of the local Neolithic (Bakels 1992; Zohary and Hopf 2000, 135–38).

The cultural dynamism of the Cardial is not only defining for the western Mediterranean sequence, but also extends far beyond its boundaries. Firstly, Cardial interferences on the northern pole of neolithisation are well attested in several facets of the material culture (see below). Secondly, Cardial typological influences are noticeable on Late Mesolithic assemblages, the discussion revolving mostly around the typology of Mesolithic and Neolithic transverse arrowheads, especially a type known as the Montclus point (Marchand 2007). The Montclus point has a complex life history as it first appears in the Mesolithic of Languedoc and was then added to Cardial toolkit (Barbaza et al. 1984). From there on, its diffusion back into other Mesolithic contexts can be observed along the French Atlantic coast as well as further inland (e.g. site of L'Essart, Poitiers: Marchand et al. 2007). The Retzian group, of which over 40 sites are known (but only four excavated), lies between the Gironde and the Loire rivers during the second half of the sixth millennium cal. BC. The occurrence of Montclus points in this group has been convincingly argued to be the result of a process of function transfer and technical translation by local Mesolithic communities in contact with the Neolithic incomers (Marchand 2005a, b, 2007). The Retzian group is of further interest since it also acts as a buffer between these southern Neolithic influences and the late Mesolithic of Brittany, where such Cardial-derived traits are absent (Marchand 2005a, b, 2007).

Beyond these typological influences, further signs of Neolithic settlement along the French Atlantic coast at such an early date are currently absent. A few sites were tentatively assigned to the last centuries of the sixth millennium cal. BC, but the evidence now appears unconvincing (Laporte 2005). Likewise, the absence of any clear technological links with the Iberian Cardial rules out the possibility of a neolithisation by sea travel originating from Spanish coasts (Marchand 2007). The neolithisation of the southern half of the French Atlantic façade is, therefore, dated to the first half of the fifth millennium cal. BC. This process occurred, once more, under strong meridional influences, this time related to the Epicardial. Although data concerning early cereal cultivation remain rare for the French Atlantic coast, there seems to be a preference for *Triticum aestivum/durum*, which is perhaps an expression of such meridional influences, although the role of ecological factors cannot be ruled out (Zapota and Peña-Chocarro 2005). The Epicardial affinities are mostly manifested in the ceramic repertoire, from both decorative and morphological points of view (Laporte 2005; Marchand and Manen 2006). It must be stressed, however, that despite the obvious meridional influences, the early Neolithic in this part of the French Atlantic coast cannot be considered as a mere Epicardial subgroup, since a strong local component is very perceptible in the lithic industry. Furthermore, although the Epicardial is indeed characterised by a wider use of the landscape, there is a major geographical gap between its distribution and the Atlantic French coast (Marchand and Manen 2006).

To the North: LBK and After

The northern pole of neolithisation is associated with the LBK, which is present in the Belgian loess area (Hesbaye and Hainaut) by 5300 cal. BC (van Berg and Hauzeur 2001), and towards the very end of the sixth millennium BC in the Paris Basin (Rubané Récent du Bassin Parisien, or RRBP: Dubouloz 2003; Chap. 9).

Villages composed of several of the iconic longhouses are confined to river valleys, while a few enclosures, whose function is still hotly debated, are also documented (Keeley and Cahen 1989; Krause 1998). The economic identity of this northern Neolithic pole is noticeable. Contrary to the Cardial, the LBK plant assemblage presents a restricted range of crops and pulses, a feature which is recognised across the entire distribution area of this culture (Colledge et al. 2004; Conolly et al. 2008). The presence of poppy seeds in several French, Belgian and German sites points to imports from the Cardial sphere (Bakels 1992; Heim and Jadin 1998). From the point of view of animal domesticates, LBK faunal assemblages are dominated by cattle, a preference which remains in the Paris basin during the succeeding cultures of Villeneuve-Saint-Germain, Cerny and Chassean until the beginning of the fourth millennium cal. BC (Tresset 2005).

Despite its strong cultural identity, the LBK is not exempt from external influences. For instance, a local Mesolithic component is perceptible in the typology of arrowheads of the RRBP (Allard 2007). Similar claims have been made for Belgian LBK sites, but a recent reexamination of the evidence suggests that these putative similarities were over-exaggerated and that there are no straightforward evidence for interactions between the Mesolithic and LBK lithic typology and technology in this area (Robinson 2008). Cardial influences, already noted through the presence of poppy seeds, are also perceptible in the bone industry of the RRBP (Sidéra 2008).

Comparable southerly influences, in ceramic morphology and decoration, ornaments and, to a lesser extent, lithics (Hauzeur 2001), are partly present in the constitution of the Blicquy/Villeneuve-Saint-Germain (hereafter BVSG), which succeeds the LBK in both the Paris basin and the Belgian loess area during the first third of the fifth millennium cal. BC (Dubouloz 2003; Jadin 2003). The BVSG settlement pattern is characterised by a more systematic use of the plateaus. This wider use of the landscape is also noticeable in a marked westward expansion out of the Paris basin (Allard 2007). While the RRBP is known in the Caen plain, the BVSG is well documented in the sedimentary area of Normandy (Verron 2000, 91), as well as in the Armorican Massif and central Brittany (Marchand 2007, 234–5). This last area must have been of crucial importance for the BVSG communities, as it was one of only two sources of slate, the preferred raw material for the extensive production of stone rings, one of the strongest cultural markers of this culture (the other source being the Belgian Ardennes: Fromont 2005). BVSG sites were thus less than 100 km away from contemporary coastal Mesolithic, although there are no straightforward signs of interaction between these two human groups.

By the first half of the fifth millennium cal. BC, a complex cultural geography was thus in place, with two geographically distinct – but not culturally impermeable – poles of neolithisation, with the LBK and then the BVSG to the North, and the Impressa-Cardial-Epicardial sequence to the South. Along the Atlantic coast, we observe an intricate mix of local traits, as well as slight but definitively southern ones, which meet the westward expansion of the northern pole, albeit in a form where other southerly influences have already percolated. This complex synthesis dominated by this North–South dialogue shapes – and is somehow played again – during the succeeding centuries.

A Time for Syntheses: The Cultural Sequence of the Second Half of the Fifth Millennium cal. BC

While separated by a few centuries from the end of the BVSG, I will outline here only the cultural situation of the second half of the fifth millennium cal. BC as it largely determines the neolithisation of Britain and Ireland. This period is characterised by the formation of two main large-scale archaeological cultures which together cover most of present-day France. On one side, the Chassean culture is originally centred on the French Mediterranean coast but also presents northern groups; on the other side, the Michelsberg culture (German Michelsberger Kultur, hereafter MK) covers most of the northern half of France and neighbouring areas (Belgium, southern Netherlands and western Germany).

The southern Chassean culture is dated between 4400 and 3500 cal. BC, and covers the entire French Midi (Vaquer 1998), with some further groups scattered along the Atlantic coast between the Pyrenees and the Loire (Roussot-Laroque 1998). While the well-studied ceramic repertoire constitutes the most discriminate trait in the definition of this culture, the rest of the material evidence shows marked variability, as acknowledged by all specialists (see contributions in Beeching et al. 1991). Funerary practices provide a perfect example of this situation. Graves occur in pits, reused silos and wells, as well as cists and even in a few proper monuments. Cemeteries coexist with burials within settlements, and there is no recurrent feature in the range of grave goods (Beyneix 2007). Likewise, the period is marked by the development of wide-ranging exchange networks, especially concerning lithic raw materials. Although Alpine axeheads were already in circulation across Europe from the turn of the sixth and fifth millennium cal. BC onwards (Pétrequin et al. 1997; see below), the distribution of, for instance, *silex bédoulien* in the French Midi corresponds to a quantitative change (e.g. Léa 2004). The partial economic homogeneity suggested by the exchange networks is, however, not mirrored by the lithic technology. On the contrary, investigations of the *chaînes opératoires* and use-wear studies reveal the existence of profound regional differences throughout the Chassean. This situation suggests that several filters are active in the adoption of techniques from one region to the next, which seem to relate to further differences in the settlement pattern (Léa et al. 2004; Léa 2004; Gassin et al. 2006a, b). Another defining feature of the Chassean is the widespread occurrence of large-scale, sometimes enclosed, settlements (10–30 ha in size), alongside smaller occupations, including caves (Vaquer 1998, 435). This situation is sometimes interpreted as the existence of a hierarchised settlement pattern and society (Demoule et al. 2005). Further north, the variability of the Chassean culture is manifested by local groups known as *Chasséen du centre* and *Chasséen septentrional* (central and northern Chassean), distributed in central France, the Paris Basin and Normandy. As in the case of the southern Chassean, the definition of these entities rests mostly upon ceramics which, as the name suggests, present elements of similarities with southern assemblages. This loose coherence, coupled with a restricted series of traits pointing to marked southern connections, is also indicated in other elements of the archaeological record (e.g. bone assemblages and technology: Tresset 1989; Sénépart and Sidéra 1991; Arbogast 1994, 107).

The second archaeological culture that dominates the cultural landscape of the period in north-western Europe is the MK. The MK is roughly contemporaneous with the Chassean and also demonstrates a large-scale distribution, which extends far beyond the geographical limits of this study (Biel et al. 1998). Recent typological reinvestigations of this group suggest that its origins must be sought further east in the Bischeim group (Jeunesse et al. 2004). Typological interaction of the Chassean culture can be observed on ceramic assemblages in the Paris basin and in Belgium (e.g. Vanmontfort et al. 1997; Constantin and Blanchet 1998; Vanmontfort 2001; Crombé et al. 2005). Several structural elements reinforce the comparison between the MK and the Chassean, and suggest that they both follow similar, intertwined, historical trajectories. The impact of the MK on the landscape is rather dramatic with the opening of several flint mines and the concomitant building of several new enclosures. Although networks supplying lithic raw materials are well established in the LBK and the BVSG (with some possible early flint extraction pits at Longrais: Ghesquière et al. 2008), the last centuries of the fifth millennium cal. BC see a major development in this process. Extraction structures, including deep shaft mines, are indeed opened and exploited for several centuries across the entire distribution area of the MK (e.g. 5000 shaft mines in Jablines: Bostyn and Lanchon 1992; see also Augereau 1998). It is difficult, if not impossible, to make any accurate estimation of the amount of axes or other flint tools produced at any of these sites. Yet, the sheer density of these exploitations, at both inter- and intra-site levels, testifies to the huge demand. Another major imprint of the MK – and of the *Chasséen septentrional* – on the landscape takes the form of the dozens of enclosures built around this time (Constantin and Blanchet 1998, 606, 609; a phenomenon also observed in the MK of central Europe: Raetzel-Fabian 2002). These enclosures do not show any defensive function, but rather occur in a wide range of settings and exhibit various associated features: long and complicated biographies of the surrounding ditches (e.g. Maisons-Alfort: Cottiaux et al. 2008), acts of structured deposition (including human remains: e.g. Poulain and Lange 1984) and faunal remains indicative of potential feasting activities (e.g. Méniel 1984). Beyond this multiplicity, their sheer number remains their most impressive characteristic. This quantitative explosion has been interpreted – as part of a wider pan-European pattern – as an archaeological proxy for a relatively late response to the demographic pressure triggered by the shift to farming (Bocquet-Appel and Dubouloz 2003, 2004).

The expansion of the MK across the landscape sometimes takes the form of a clear geographical diffusion. Between 4900 and 4450 cal. BC (i.e. end of the BVSG to early MK), the loess area of Belgium seems to lack any major human occupation, as indicated by the absence of any artefacts attributed to this period either in excavations or surface scatters (Crombé et al. 2005). The nature of this process is difficult to pinpoint: drastic depopulation (as suggested for Germany: Shennan and Edinborough 2007) or reorganisation of the settlement pattern in favour of the Paris Basin? Whatever the reasons for this absence of human occupation, this situation contrasts with the contemporary presence of hunter-gatherer sites in the northern sandy area of Belgium (e.g. site of Doel: Crombé 2005). By 4450 cal. BC, the loessic area is reoccupied with MK sites, whose typology recalls more the *Chasséen septentrional* than its German eastern counterpart (Crombé and Vanmontfort 2007). This occupation takes the form of large sites, including several enclosures (Cauwe et al. 2001; Vanmontfort et al. 2004), and extensive flint exploitation (e.g. the site of Spiennes covering at least 100 ha: Collet et al. 1997, 2008). Smaller sized MK sites are also documented in the sandy part of Flanders, where they are associated with the introduction of early farming (Crombé and Sergant 2008). The nature of the differences between loess-based and sand-based sites is difficult to assess: Vanmontfort has suggested, with reference to the Chassean and the rest of the MK, that it could indicate the existence of a hierarchised settlement system with enclosures, settlements and flint mines (Vanmontfort 2007).

To finish this brief summary of the cultural sequence, it is worth mentioning that the second half of the fifth millennium cal. BC also corresponds to the final neolithisation of the French Atlantic façade, and to the early development of the megalithic tradition in this area. The relationship of these two processes is far from being straightforward, as there is no direct chronological correspondence between the two phenomena (Scarre 2007). From the very stages, megalithic structures show an impressive regional variability, which is most probably indicative of their multiple origins and local developments (Scarre 2002; Laporte and Le Roux 2004, 9–30).

North and South: Constant Recombinations

From the late sixth millennium cal. BC through the entire fifth millennium cal. BC, the culture history of the area investigated here is tributary of two contrasting cultural poles, firstly associated with their respective flavours of neolithisation, and secondly, with contrasted but broadly similar lines of historical developments. In a way, this situation is hardly surprising and is merely the outcome of the physical geography, although the recurrent cultural predominance of southern traits is an intriguing phenomenon that requires further explanation. This bi-polarisation is obviously somewhat of a simplification, but its greatest merit is in enabling the identification of recurrent patterns in an otherwise opaque mosaic of mutual influences and, therefore, reorienting the discussion towards the elucidation of the mechanisms involved.

Processes of cultural transmission associated with the earliest Neolithic stages have been detected in multiple traits of the material culture and all, with their own specifics, tell the same story: be it technological transfers identified through the typo-morphology of arrowheads, the introduction of ceramics, bone technology or farming practices, the dominant cultural role of the Cardial and Epicardial upon the Atlantic façade and, to a lesser extent, on the Paris Basin is evident. This web of influences, coupled with local elements and the geographical extensions of the BVSG in Normandy and Brittany, leads to a process of extensive combination, eventual source of original cultural constructs.

The constitution and interaction of the Chassean groups and the MK are largely comparable. At the regional level, each of these archaeological cultures is itself the result of combinations of disparate traits, loosely bound together by similarities in the ceramic repertoire. As Vanmontfort puts it, with regard to the Belgian MK, 'The Northwest European archaeological cultures of the late fifth and early fourth millennium in their polythetic meaning thus seem polycentrically formed and developed. The "Belgian Michelsberg culture," as it is still frequently labelled, is in this view a local version of similar development in neighbouring regions' (Vanmontfort 2007, 112; see also Demoule et al. 2005, 64). Hierarchised and expanding settlement patterns dominated by large enclosed sites, as well as wide-ranging exchange networks and extensive production centres, suggest changes in the demography of the communities towards the end of the fifth millennium cal. BC. Nothing suggests that this increase in the mere number of people led to unmanageable population pressure and an imbalance in the carrying capacity of the land. Rather, the territorial expansion, creation and subsequent maintenance of these large-scale archaeological cultures are three expressions of a notable increasing density of the human networks, which must have been accompanied by the development of required, adapted social structures (see below). Although a more precise delineation of these social processes is tempting, I will, for the time being, only note this coincidence of factors and explore its implications for the introduction of farming practices in the British Isles.

Islands in the Stream: The Neolithisation of Britain and Ireland

The introduction of the Neolithic in the British Isles has always been the locus of intense exchange of ideas and was, for instance, heralded as a key sequence by the anti-migrationists in their attack of culture history (Clark 1966). Yet today, here more than anywhere, the debate still revolves around the binary opposition between those who invoke the necessary role of external factors and those who stress the predominance of local traditions (see, for instance, the debate following Peter Rowley-Conwy's recent position paper: Rowley-Conwy 2004). As a result, large amounts of data are now available, which have been further increased by development-led archaeology over the past two decades and the constant re-examination of older excavations (e.g. Bradley 2007, 2008; Whittle et al. 2008; Pailler and Sheridan 2009 and contributions in Whittle and Cummings 2007). The following discussion will focus on Britain, with occasional references to Ireland.

A growing and consistent body of evidence now indicates that the successful introduction of farming to the British Isles occurred in the first couple of centuries of the fourth millennium cal. BC, with a few potentially older precursor episodes (Pailler and Sheridan 2009; see below). Although low until recently, the inventory of early timber buildings with proper base plans is now relatively extensive with occurrences in Ireland, Scotland and, to some extent, England (Ireland: Milner and Woodman 2005; Cooney 2007; Scotland: Brophy 2007 and Thames valley: Hey and Barclay 2007;

see also Bradley 2008). These sites have yielded clear-cut, relatively large assemblages of domesticates, which recall the LBK and the subsequent cultures of the northern pole of neolithisation (e.g. a reduced range of cultigens dominated by *Triticum diccocum*: Zapota and Peña-Chocarro 2005; see also Brown 2007). British Neolithic domestic sites notoriously lack extensive assemblages of domesticated plants. This feature has sometimes been interpreted as an indication of their low economic value. Alternatively, scholars, such as Julian Thomas, have argued that cultivated plants were first and foremost a kind of prestigious food preferably consumed in ritual contexts (Thomas 2007, 2008). In a recent paper, Bogaard and Jones have conducted a systematic comparison of central European (LBK) and British archaeobotanical assemblages in order to test this hypothesis (Bogaard and Jones 2007). Their results are enlightening. Firstly, values of cereals per sample for Britain are comparable to values gained for the site of Vaihingen, which has yielded one of the highest amounts of cereals known for the LBK. Secondly, the generally highest level of charred chaff found in LBK sites is probably related to specific practices (e.g. use for feeding animals or as fuel), but cannot be explained in terms of a more extensive agricultural production. Thirdly, claims for the existence of a low-intensive, mobile agriculture are not supported by weed data either from central Europe or Britain. Bogaard and Jones thus conclude that agriculture was well established and routinely practiced by communities in Neolithic Britain (Bogaard and Jones 2007). Continental connections and real economic roles are also apparent for animal domesticates (Tresset 2002, 2005; Tresset and Vigne 2007). Similarities between the Paris Basin and Britain are noticeable in the size range of the first domesticated cattle, as well as the frequency of domestic species in the faunal spectrum (Tresset 2002, 2005). The argument of a predominantly ritual use of cattle during the British Early Neolithic is mostly based on the presence of animal remains in enclosures, but fails to explain their discovery in domestic sites such as Runnymede (Tresset, 2002, 2005). Here also, the comparison with the Paris Basin is instructive, as animal remains in enclosures are also commonplace there and routinely interpreted as food detritus (Tresset, 2002, 2005). Lastly, recent biomolecular studies of pottery have demonstrated the consumption of dairy products from the early Neolithic and throughout later prehistory in Britain (Copley et al. 2005a, b; Evershed 2007). These various elements demonstrate beyond doubt that the British early Neolithic corresponds to the introduction of farming practices, which contributed in a significant way to the diet of these communities. Further support is provided by stable isotope studies which have shown a shift towards a more terrestrial diet during the early Neolithic when compared to the preceding Mesolithic period (e.g. Schulting and Richards 2002). The magnitude of this change is, however, disputed, as other types of evidence indicate that Neolithic communities did occasionally resort to maritime resources. Equally, it must be noted that methodological problems remain surrounding the precision of stable isotope measurements (Milner et al. 2004; Barberena and Borrero 2005). Whatever the changing role of the sea in terms of nutrition, there are no grounds to sustain the claim of a Neolithic taboo on fish consumption made by Julian Thomas (2003), who always seems keen to put Neolithic people on a restricted diet.

Although difficult to disentangle from wider climatic modifications, a variety of evidence throughout Britain and Ireland also suggests the potential impact of early agriculture upon the environment, whether through the opening of fields and/or the cutting of selected trees for the construction of wooden buildings. These include the so-called elm decline (Cooney 2007), the use of soils most suitable for agriculture, the change in erosion and sedimentation of the lochs in eastern Scotland (Warren 2005; see also Bonsall et al. 2002) and episodes of forest clearance in the Thames valley (Hey and Barclay 2007).

Changes are equally noticeable in the material culture. In southern England, late Mesolithic and early Neolithic assemblages are often difficult to distinguish, save for the introduction of new morphological types such as leaf-shaped arrowheads (e.g. Edmonds 1995). The situation in Scotland changes from one region to the other: for instance, in the eastern part, the Neolithic lithic industry is markedly different from the Mesolithic one, with new techniques and types similar to the rest of Britain (e.g. leaf-shaped arrowheads), while further south, elements of continuity are more obvious (Warren 2005). While the Irish Mesolithic lithic technology exhibits a markedly insular character (Costa et al. 2005), early Neolithic assemblages are characterised by the introduction of new tools, such as scrapers and, once more, leaf-shaped arrowheads (Milner and Woodman 2005).

Contrary to the rest of North-Western Europe (Blankohm 2007; Verhart 2007), both the British and Irish Mesolithic are aceramic. Although new evidence can always come to light, it can be safely proposed that pottery production was introduced in Britain at the same time and by the same people who introduced farming. Alison Sheridan has extensively discussed this phenomenon in several papers and has identified, mostly on the basis of the ceramic typology but with occasional reference to other traits, four main strands of neolithisation (e.g. Sheridan 2005, 2007, 2010; Pailler and Sheridan 2009). The first one corresponds to the discovery of a few domesticated bones in the otherwise Mesolithic Irish site of Ferriter's Cove dating to the late fifth millennium cal. BC (Milner and Woodman 2005) and is interpreted as a putative first unsuccessful colonisation attempt. The second episode, defined on the basis of both ceramic and monument typologies, sees contacts between Britain and Brittany and is dated between 4300 and 4000 cal. BC (Sheridan 2005). Based on similar arguments as the previous one, the third episode points towards interactions across the Channel between Normandy and Brittany on one side, and southern England on the other. The last – and definitive – strand of neolithisation is characterised by a form of distinctive pottery production, the Carinated Bowl tradition (Sheridan 2007). As the associated evidence has recently been discussed at length by Alison Sheridan (2007), I will point out only two main elements here. Firstly, contrary to the three other previous episodes, the Carinated Bowl tradition is recognised across all of Britain and Ireland, with several regional concentrations. For instance, there are no less than 40 sites falling within 4000 and 3700 cal. BC for Scotland only. I do not wish here to revive scenarios of armadas of farmers sailing to conquer new lands, but the extensive – as far as the British Isles are concerned – scale of this phenomenon is worth mentioning.

Secondly, despite evident continental typological affinities to be sought with the *Chasséen septentrional* and the MK of Picardy and/or Pas-de-Calais, it is impossible to pinpoint a single geographical origin for the Carinated Bowl tradition. Rather than hoping for some putative perfect typological match waiting to be excavated in northern France or Belgium, there is more to be gained by considering the Carinated Bowl tradition as corresponding to an original recombination of typological traits which were, as we have already seen, already extensively mixed on the continent (Sheridan 2007).

This apparent tendency towards cultural recombination in the early British Neolithic finds support in other evidence at the turn of the fifth and fourth millennium cal. BC. For more than a decade, the French archaeologist Pierre Pétrequin and his collaborators have documented the existence of an extensive exchange network of green jadeite axeheads. The extent of this network is only rivalled by the circulation of copper and tin during the Bronze Age (Pétrequin et al. 1997, 2008). Systematic field prospecting has enabled Pétrequin and his colleagues to locate and excavate the source quarries of the jadeite in the western Italian Alps (sites of Viso and Beigua: Pétrequin et al. 2006). From there, the axeheads were distributed all across western Europe, as far as Britain, Denmark and Germany, with a few exceptional outliers as far east as Bulgaria (Pétrequin et al. 1997, 2008). Since few of these axeheads were found – and probably deposited in many cases – from secure archaeological contexts, their absolute chronology has been difficult to establish. However, through a combination of typological and geographical distribution (e.g. opposition between northern and southern types), and information gathered on the quarries themselves and in a few privileged points of discovery, Pétrequin and his collaborators have created a reliable chronological framework (Pétrequin et al. 2008). The production, distribution and consumption of these green axeheads extend from the late sixth to the early fourth millennium cal. BC. The British examples are some of the latest known ones: two axes of the Bernon type cannot be from earlier than 4300/4200 cal. BC. Another axehead was found at Glastonbury Sweet Track, which was built around 3807–3806 BC and abandoned around 3791 cal. BC (based on dendrochronology), thus contemporaneous with the Carinated Bowl Tradition (Pétrequin et al. 2008). The European distribution of the types of axeheads also allows some inferences on the ways of dispersal and the potential origin of the British examples. Two concentrations of northern style axeheads (Altenstadt/Greenbauw type) in Scotland and East Anglia probably originate from Normandy or the Somme area, while another concentration of Durrington-type axes on the southern coast of England also suggests origins in Normandy, although Brittany cannot be ruled out. Britain thus witnesses the convergence of at least two main streams of circulation, which contrasts with the continentale situation where the distributions, and probably circulation, of Durrington and Altenstadt/Greenbauw are mutually exclusive. Likewise, the hoards from Oxnam/Cunzierton and Glenluce/Glenjorrie Farm combine axeheads of both northern and southern types, while similar associations are otherwise extremely rare in the much larger continental dataset (e.g. there are just two cases in France: Pétrequin et al. 2008).

Recently, there has been a growing awareness of the regional variation of the early Neolithic in Britain and Ireland, which is best exemplified in work carried out on the chronology of monument-building (Whittle 2007). It has already been noted that a few megalithic monuments are potential indicators of very early stages of the neolithisation process in this country (Pailler and Sheridan 2009). For instance, there are a few monumental structures in Scotland which fall within the same timespan as the Carinated Bowl Tradition (five non-megalithic long barrows: Kintore, Fordhouse Barrow, Eweford, Pencraig Hill and Biggar Common; and five cursus monuments at Holywood North and South, Holm and Dungarit, Upper Largie), as well as, possibly, the Irish causewayed enclosure of Mogheaboy (Sheridan 2007). The picture for southern Britain is rapidly changing and markedly different. Parallel to the discovery of the previously discussed early Neolithic horizon, new extensive dating programs and associated Bayesian modelling directed by Alex Bayliss and Alasdair Whittle have demonstrated that the building of causewayed enclosures and of long barrows in southern Britain only starts around the 38–37th centuries cal. BC (Whittle et al. 2007; Bayliss et al. 2008; see also Cummings 2007). The "monumentalisation" – and growing insularity – of the southern British Neolithic thus postdates the introduction of farming in the area by three centuries (Bradley 2008).

Further West, Further Recombination

It is hardly original to link the neolithisation of Britain with the events occurring in the last centuries of the fifth millennium cal. BC on the near Continent, especially in the Belgian sequence with the reintroduction of human settlement in the loess area and the concomitant neolithisation of the sandy area (e.g. recently Whittle 2007; Pailler and Sheridan 2009). The need for new richer soils has been invoked as an explanation (Pailler and Sheridan 2009). This could have been eased by the parallel extension of soils favourable for agriculture in some parts of Britain under a new climatic regime (Bonsall et al. 2002). If continental North-Western Europe seems undeniably more populated than ever around this time, it is unclear how this demographic change was translated in terms of carrying capacity of the land. I have stressed that the corresponding social networks were getting progressively denser, and perhaps reached a saturation point where expansion was needed, if not encouraged. Both points were already made nearly 20 years ago by Broodbank and Strasser in the conclusion of their paper on the neolithisation of Crete: "An aspect of the hunter-gatherer farmer transition that deserves more attention is the degree to which pre-farming perceptions of socially acceptable population densities might have impelled farmers outwards to the new areas well before the 'objective' carrying capacity of an occupied zone was reached" (Broodbank and Strasser 1991, 242; see below). Whatever the underlying reasons (and neither economic nor social perspectives are mutually exclusive), it is safe to say that, by 4000 cal. BC, the Continent was getting too small and that more – insular – room was needed. If the causes remain elusive, there is perhaps more to be said about the mechanisms of expansion.

In a classic and in many ways visionary paper, Humphrey Case outlined some of the logistics involved in the neolithisation of Britain, which he saw not as an accident, but as the planned transfer of an entire economical system (Case 1969). For instance, Case distinguished between "two main types of movement: local and slow *communal movements* (of entire systems) and far-reaching and rapid *seasonal movements* (of a few individuals)" (Case 1969, 178, emphasis in the original). While the second type of movement can be labelled reconnaissance, the first type corresponds to the proper neolithisation. This idea of communal movements is crucial. While the current debate always considers either no migrants at all or a massive colonisation, the reality must by definition lie between these two extremes. Rather than focusing on continental connections, for which evidence is plentiful as we have seen, it is necessary to consider the nature of the neolithisation process and its logistics. Firstly, this process happens not only over a somewhat limited timespan but also at a global scale across the British Isles. This indicates that several communities, not necessarily from a single origin, were involved. Secondly, each of these communities must have been relatively large, be it in terms of work force or biological minimal size. Moving sufficiently large herds of animals must have required careful planning, as did the opening and maintaining of new fields or the construction of the large-scale wooden buildings, which are now the trademark of the British and Irish early Neolithic. Likewise, in order to be sustainable, a constant flow of individuals must have been required: this would have concerned human beings (e.g. establishment of new marriage networks and/or continuity of links with the founder continental groups) and animals (see, for instance, the estimates provided by Broodbank and Strasser 1991). In this sense, one of the key dimensions of the neolithisation of Britain is indeed demography, not simply because of the demographic explosion suspected on the continent, but also in terms of the structure of the colonizing groups.

Each of the participating communities must, therefore, have been either a self-viable subset of a larger group, or an *ad hoc* aggregate of individuals of different backgrounds. From the point of view of the archaeology, the processes can hardly be distinguished and have the same material signature. Drawing from a pool of mixed vague cultural similarities (i.e. the large, loose archaeological cultures, such as the MK and Chassean), the British and Irish early Neolithic extends and reproduces the contemporary cultural situation of the Atlantic façade. Beyond the general impression of homogeneity across the Irish Sea, particularly marked when contrasted to the preceding late Mesolithic (Cooney 2007; see also Cummings 2007 on the homogeneity in the landscape setting of portal dolmens), we observe a diversity of local situations. These all combine in new original syntheses of traits already existing on the continent. The plant and animal domesticates point to northerly connections; the Carinated Bowl tradition is without doubt of continental origin but lacks any precise single origin; while the green axeheads witness combinations of a genre unknown in continental Europe.

The process of neolithisation on both sides of the Atlantic façade can be summarised in a single logistic cycle, strongly influenced by the cultural and demographic history of the communities involved:

1. The neolithisation of present-day France corresponds to the introduction and intermingling of new traits deriving from two distinct centres, which eventually lead to the constitution of original syntheses which, to an extent, also incorporate local elements;
2. The corresponding human networks reach a phase of saturation, resulting in the demographic explosion associated with the passage to a new productive subsistence economy. This phase of saturation marks a new level in the combination of otherwise disparate traits, resulting in the constitution of only broadly coherent, large archaeological cultures (Chassean and MK);
3. This saturation leads to a phase of geographical expansion, marked by the dissemination of smaller communities, which extends the endless process of material recombination.

It can be suggested that this cycle does not die out with the neolithisation of the British Isles. Indeed, three centuries after the early Neolithic stage, the insularity of the British sequence becomes more accentuated (Bradley 2008), with the concomitant development, especially in southern Britain, of causewayed enclosures, long barrows, extensive flint mining (Holgate 1995) and exchange networks of axeheads made of local raw material (Bradley and Edmonds 1993). These changes, which replicate the situation observed in France and Belgium a few centuries earlier (see also Bocquet-Appel and Dubouloz 2003, 29–30 on the specifics of the British sequence), are made at the expense of continental links which gradually fade away until their apparent complete disappearance at end of the fourth millennium cal. BC (Vander Linden 2006). The dynamics of this cycle had already been outlined by Humphrey Case: "Demanding refinements are unlikely to have belonged to the period of early settlement, but rather to *stable adjustments* of mature and fully extended economies in favourable environments. The dead did not require *initial* burial in massive ritual structures […] Causewayed camps too are best regarded as features of stable adjustments" (Case 1969, 181, emphasis in the original).

For all its simplicity, this three-stage cycle encapsulates in a coherent single model the variety of processes and material traits described at length in the literature. It must also be stressed that this cycle does not constitute the explanation of the neolithisation, but rather an explanation of some of the mechanisms at play in the constitution of the consensual mosaic vision mentioned in the introduction.

Identifying Logistic Cycles: Back to the Eastern Mediterranean Basin

The neolithisation of Britain is often associated with similar changes occurring more or less at the same time as the rest of north-western Europe (cf. Belgian sandy area: Crombé and Vanmontfort 2007; Dutch lowlands: Louwe Kooijmans 2005;

northern Germany: Hartz et al. 2007 and southern Scandinavia: Larsson 2007; Andersen 2008). In this last section, I suggest that the cycles described above, especially the phase of expansion leading to new original syntheses (i.e. process of recombination), are not a mere regional idiosyncrasy, but can actually be detected in other areas in the neolithisation of the Old World, including where it all began, the eastern Mediterranean basin.

Archaeozoological, archoaeobotanical and, more recently, genetic research concur that there was no "Neolithic Revolution" in the Near East, in the sense that the domestication of plants and animals was a long, complex process which matured over several millennia and thousands of square kilometres in the Fertile Crescent (e.g. Jones and Brown 2000; Willcox 2005; Zeder et al. 2006; Allaby et al. 2008; Olsen and Gross 2008; Zeder 2009). The domestication of each plant (cereals and non-cereals) and animal (pig, sheep/goat and cattle) demonstrates its own history and seems to have occurred independently at multiple times and places. Precise chronological estimates for plant domestication are further complicated by methodological difficulties in distinguishing in the archaeobotanical record the tiling and tending of wild strands, so that there could be a delay of up to 1,000 years before the appearance of morphological traits that are securely associated with domestication (Weiss et al. 2006). The same varied pattern in both chronological and geographical terms applies for the domestication of animals, which, moreover, postdates the domestication of plants by several centuries (see below).

In this sense, the neolithisation process in the Near East can safely be described as polycentric, not unlike the two poles of neolithisation discussed above for present-day France. This polycentric model (Gebel 2004) is also perceptible in the material culture, especially during the period of generalisation of farming in the Near East, that is the PPNB (Pre-Pottery Neolithic B) culture or *koinè*. This archaeological culture covers the entire Near East between 8500 and 6250 cal. BC (Aurenche et al. 2001, Kuijt and Going-Morris 2002). The description and interpretation of the PPNB culture is heavily debated, with some scholars adopting a position strongly rooted in culture history (Cauvin 1994), while others opt for a more postmodern deconstructionist perspective (Asouti 2006). In a recent publication, Olivier Aurenche and Stefan Kozlowski have demonstrated that the distribution of several material traits delineates recurring regional groups which, globally, ensure the existence of the PPNB as an archaeological culture according to Clarke's polythetic definition (Kozlowski and Aurenche 2005; Chap. 14). The human reality, if any, reflected by this geographical patterning is not discussed here, but it must be noted that the structure of the PPNB culture presents some intriguing similarities with some of the processes discussed here (such as constitution of the Chassean and MK and corresponding saturation of the human networks; see Byrd 1994; Kuijt 2000), which would be worth exploring in an explicitly comparative framework. Likewise, the expansion of the PPNB culture is sometimes associated with the constitution of original material assemblages, which cannot only be explained by the assimilations of local populations and material traits (Cauvin 1994). A perfect example of this process of recombination is provided by the neolithisation of the island of Cyprus.

Until recently, the island of Cyprus was thought to have been colonised around 6500 cal. BC. Several excavation projects undertaken over the last 15 years have, however, dramatically altered this vision by demonstrating the presence of a handful of early Neolithic sites which date back as early as the ninth millennium cal. BC (e.g. Peltenburg et al. 2000). The material culture, as well as the architecture, found on these various sites demonstrates the Near Eastern origins of this Cypriot PPNB, as well as the persistence of the contacts with the continent (e.g. obsidian from the Anatolian source of the Göllü Dağ found in the Shillourokambos site, especially in the oldest levels: Briois et al. 1997). Yet, the Cypriot PPNB is by no means a faithful copy of its continental counterpart but demonstrates an originality of its own, best exemplified by the domestic animal assemblages. Pigs, cattle and sheep/goats are independently domesticated somewhere between 9000 and 7400 cal. BC in the central part of the Fertile Crescent (pigs: 8500–8000 cal. BC in south-eastern Anatolia; sheep/goats: 8400–7400 cal. BC between the northern Zagros and south-eastern Anatolia; cattle: 9000–8000 cal. BC in the upper Euphrates valley: Zeder 2008; Fig. 15.2). These temporal and geographical differences are more marked when the diffusion of each of these domesticates is taken into consideration: there is indeed a delay of up to two millennia in some areas of the Near East between the earliest domestication of these animals and the constitution of the classical farming trinity of cattle – pigs – goats/sheep (Horwitz et al. 1999; Zeder 1999, 2008).

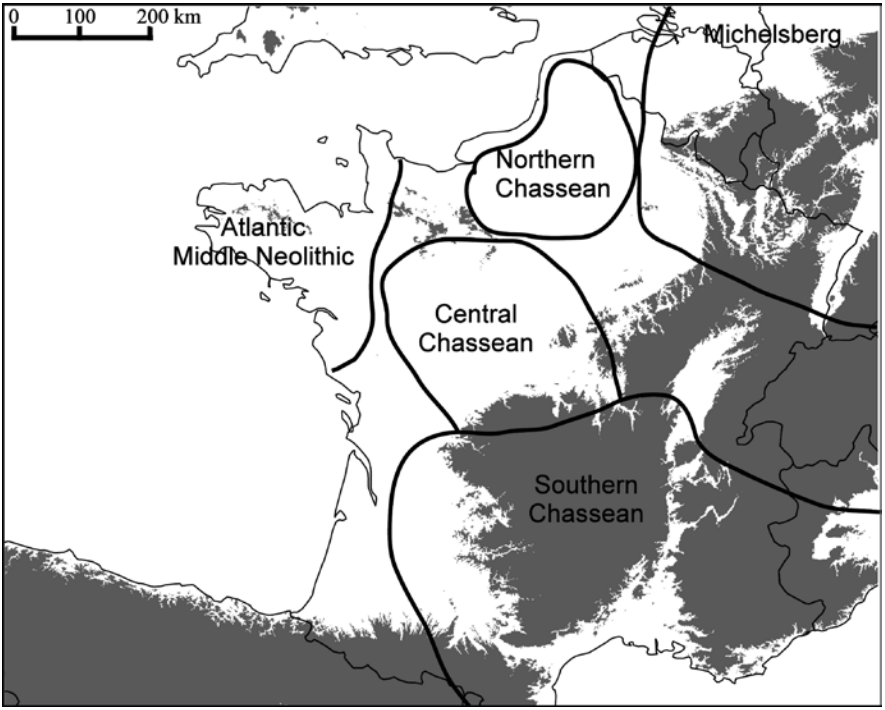

Fig. 15.2 Distribution of main archaeological cultures during the second half of the fifth millennium BC in France and neighbouring areas

In Cyprus, however, cattle, pigs and goats/sheep were deliberately introduced together by seafaring farming migrants by 8500 cal. BC, alongside a few wild species, including fallow deer (Vigne and Buitenhuis 1999), and at least one clandestine passenger, the domestic mouse (Cucchi et al. 2002). In the same vein as the neolithisation of Britain, this Neolithic colonisation must have been a relatively large, carefully planned process involving several communities putting together their respective skills and expertise, eventually leading to an original combination which does not present any counterpart in its source area.

Lastly, as suggested by Melinda Zeder, "far from being an isolated event, the colonisation of Cyprus provides a clear and valuable template for the subsequent diffusion of the Neolithic across the rest of the Mediterranean basin" (Zeder 2008, 11600). Broodbank and Strather have put forward a strong argument on the planned colonisation of Crete (Broodbank and Strasser 1991). Geographically close but not falling under the category of island archaeology, Catherine Perlès has pointed out the heterogeneity of traits of near Eastern origin in the early Neolithic of mainland Greece (Perlès 2001, 2003, 106–110). She sees this pattern as the result of the far-reaching expansion ("leapfrogging": van Andel and Runnels 1995) of pioneer communities, which "followed different pathways from their original ancestral 'home' to their ultimate settlement in Greece. Each would have retained some, but some only, of their most valuable symbols or techniques" (Perlès 2003, 110).

Waves and Backwash: Beyond the Consensual Neolithisation Mosaic

The image of the Neolithic wave has a long history in the field of Neolithic studies as it goes way back, at least, to the ever influential wave-of-advance model formulated by Ammerman and Cavalli-Sforza (Ammerman and Cavalli-Sforza 1971, 1984). Without denying the importance of this model (Bocquet-Appel et al. 2009; Vander Linden 2011), the concomitant image of the backwash probably is a better fit with the complexity of situations described here: multiple neolithisation streams linked together through a multitude of processes of cultural transmission operating at diverse scales, resulting in constant recombinations of these varied influences, episodes of geographical expansion associated with further recombination of material and cultural traits. Thus, at first sight, much of the discussed evidence provides a strong supporting case for the consensual mosaic mentioned in the introduction. Yet, a few recurring patterns can be observed underneath this appealing and multivocal variety of local situations, in particular the existence of cycles which can be shown to regulate, in many cases, the shape of the Neolithic expansion.

The small comparative exercise proposed here shows the necessity to depart from our general expectations regarding the spread of the Neolithic and allegedly associated phenomena, and rather to go back to the structure of the archaeological data involved. Indeed, neither this backwash of influences nor the cycles that structure it can be explained in satisfactory terms by a mere opposition between incoming farmers and native hunter-gatherers. The French sequence illustrates the manifold interactions at play between different incoming Neolithic communities, as well as with the local foraging groups. Yet, this vast array of processes of cultural transmission leads to some eventual coherence with the formation of large-scale archaeological cultures, which are the result of the major demographic changes of the period, and expresses associated social readjustments. Demography, or more precisely the logistics of the expansion, also explains the shape of the neolithisation of Britain and Ireland, as well as of Cyprus, Crete and mainland Crete. In all these cases, the movements of restricted, but necessarily sufficiently large, groups condition not only the structure of the Neolithic expansion, but also the resulting cultural variation in the new-found lands.

It must be stressed again and again that these cycles are not the explanation of the spread of the Neolithic in Europe, but just one of its crucial, albeit overlooked, components. For instance, the internal mechanisms of the neolithisation are crucial, but so are their relationships with external factors, such as ecology or climate (Weninger et al. 2006; Berger and Guilaine 2009; Vander Linden 2011). Perhaps the most important dimensions of these logistic cycles are that they allow us an escape from the risk inherent in the consensual mosaic vision of stamp-collecting mere case studies. The interplay of (not necessarily many) parameters is indeed perfectly compatible with a variety of outcomes, a point apparently poorly understood by some archaeologists, but otherwise well known and described in other fields such as agent-based modelling (e.g. Axelrod 1997).

As the structure of this paper exemplifies, going over a mosaic of situations is only possible by resorting to archaeological cultures as the main unit of analysis. While small case studies will always be comfortable to some, especially in order to assume their relevance through fashionable theoretical hypertrophy, grander narratives are an equally valid scale of analysis, to which archaeology, with its general poor resolution, is perfectly suited (Sherratt 1995). Such an exercise neither implies that the archaeological cultures mentioned here are the expressions of past tribes, nor does it lead to a dehumanised archaeology, where cultures are given preeminence over people as the actors of the past: there is no need for Chasseans to have a Chassean archaeological culture. But the existence of such a thing as a Chassean or any archaeological culture is, in itself, an intriguing phenomenon that is worth investigating. By focusing the analytical work on the various processes of cultural transmission that such an archaeological culture encompasses, and by taking into consideration the demographic structure of the communities transmitting it, it is possible to reorientate the archaeological discourse in an original direction. David Clarke once wrote that “the reconstruction of a historical or social picture of prehistoric cultures, written in historical narrative, is a valid but incidental and dangerous aspect of archaeology” (Clarke 1968, 13). Archaeological reasoning should indeed be grounded also in archaeological problems, not in any poor impersonation of fellow social sciences and humanities.

Acknowledgements This research was funded by the Leverhulme Fund and the “Formation of Europe – prehistoric population dynamics and the roots of socio-cultural diversity” program of the European Commission (NEST). The constant support by members of the latter project, especially

A. Shukurov and J.-P. Bocquet-Appel, made the realisation of this paper possible. Several other colleagues have also been instrumental in the (very long) shaping of these ideas, in particular Richard Bradley, James Conolly, Cameron Petrie, Ryan Rabett and my co-editor, Ben Roberts. Special thanks to Frank Herbert and Steve Harris for the title. Of course, I remain solely responsible for all mistakes, omissions and/or misinterpretations to be found here.

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Chapter 16
Are 'Cultures' Inherited? Multidisciplinary Perspectives on the Origins and Migrations of Austronesian-Speaking Peoples Prior to 1000 BC

Peter Bellwood, Geoffrey Chambers, Malcolm Ross, and Hsiao-chun Hung

Introduction

> The unwisdom of naïve correlations between social, archaeological, linguistic and racial entities has been pointed out…Nevertheless, it seems equally foolish to argue for no correlation at all… (Clarke 1968: 398)

The concept of the archaeological culture in the Childean sense implies that ethnicity was expressed in prehistory through the medium of material culture. It also implies that cultures so defined were capable of vertical transmission between generations and within populations. While reticulative (horizontal) processes of borrowing and 'creolization' (Rowlands 1994) have undoubtedly occurred in many situations throughout human prehistory, we need to understand them in balanced perspective.

This paper is not particularly concerned with the concept of ethnicity per se, but rather with showing that languages, material culture and genes can be transmitted through time and space with relatively high degrees of correlation, *especially during periods of population migration* (Bellwood 1996). The example used to demonstrate this is the foundation dispersal of speakers of Austronesian languages, whose descendants today number more than 380 million people distributed throughout Island Southeast Asia (including Taiwan) and Oceania (excluding Australia and interior New Guinea), with extensions westwards to southern Vietnam, Peninsular Malaysia and Madagascar (Fig. 16.1). Austronesian-speaking populations are by definition a *linguistic* entity, but they reveal considerable biological and cultural variation across their vast area of distribution, and therefore questions of history at the population level demand more than simply a linguistic analysis. Languages are attached to living speakers, and groups of speakers carry ancestries of cultural and biological ontogeny, interaction and migration.

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B.W. Roberts and M. Vander Linden (eds.), *Investigating Archaeological Cultures: Material Culture, Variability, and Transmission*, DOI 10.1007/978-1-4419-6970-5_16,

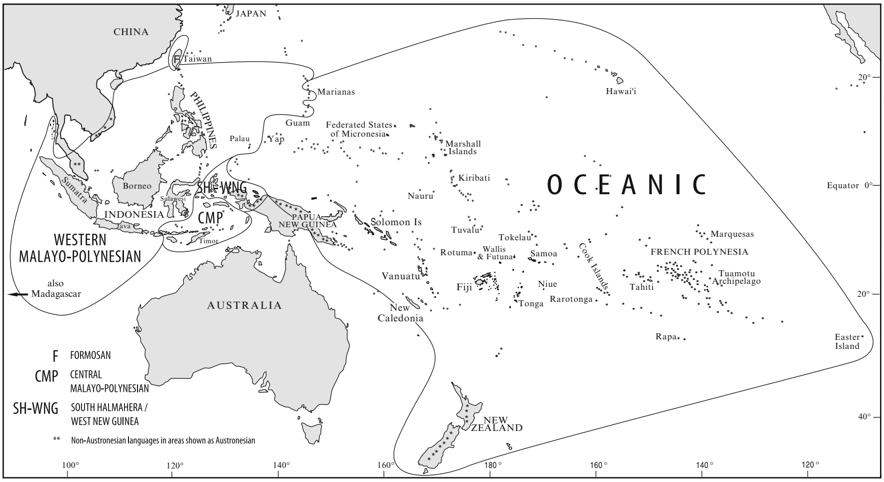

Fig. 16.1 The Austronesian language family and major Austronesian language groups.

The question 'What has been the history of Austronesian-speaking populations?' can only be answered holistically by careful comparison of interpretations drawn from independent historically focused disciplines. Linguists must ask where and when the Austronesian language family originated, and why and how the ancestral languages within the family spread through such a vast area. Did it spread with existing speakers, or by language shift or, more likely perhaps, by differing combinations of both processes according to differing cultural and demographic circumstances? Are there visible episodes of change in the archaeological record that can be associated with a migration of ancestral speakers of Austronesian languages, or is regional cultural continuity a more likely alternative? Do skeletal and genetic data support in situ population differentiation or migratory explanations? These disciplines are independent in terms of data generation, but all come together in the service of a reasoned and well-argued version of history.

Chronologically, we are concerned mainly with a period between 2500 and 1000 BC, focused archaeologically in Taiwan and the Philippines. Archaeological developments beyond these regions, for instance in Indonesia and Remote Oceania beyond the Solomons, are not examined in detail. Neither do we consider Pre-Austronesian linguistic and cultural dispersal from southern China into Taiwan at about 3500 BC. However, the linguistic and genetic sections necessarily must investigate data comparatively, from broadly spread modern populations, owing to the absence of any significant ancient DNA research on humans in the specific region under examination, and of any coherent written records in indigenous languages before the late first millennium AD. It is necessary for comparative purposes to draw sometimes on regions much broader than just Taiwan and the Philippines alone.

Current Linguistic Perspectives on the Austronesian Homeland

Of the disciplines represented in this paper, linguistics is probably the closest to unanimity about Austronesian origins. All Austronesian languages spoken outside Taiwan belong to a single subgroup, dubbed Malayo-Polynesian by Blust (1977), while the 13 Austronesian languages still spoken in Taiwan belong to several primary subgroups (Blust 1999 proposes nine, on phonological grounds). The logical inference is that Proto-Austronesian (PAn) was spoken in Taiwan, that it split initially into dialects, and that these dialects eventually diversified into separate languages. Speakers of just one of the dialects, Proto-Malayo-Polynesian (PMP), left Taiwan and settled initially either on Lanyu (Orchid) Island, or somewhere in the Batanes Islands, or on the north coast of Luzon. It is speakers of languages descended from PMP who have settled the huge expanse of the Austronesian-speaking region beyond Taiwan.

A Taiwan homeland for PAn was first noted as one of several possibilities by Dyen (1965, 1971) on the grounds of phonological mergers common to all extra-Formosan languages. Blust (1977) added an argument based on pronoun forms in Malayo-Polynesian languages, which resulted in rapid acceptance of a Taiwan origin among Austronesian historical linguists (e.g. Harvey 1982; Reid 1982; Starosta et al. 1982). There is no space here to enter into details about linguistic methodology, and the relevant methodological issues and criticisms are handled by Blust (1999), who concludes that by far the most reliable subgrouping method available to historical linguists is the comparative method, which subgroups languages on the basis of innovations that they share relative to a protolanguage from which they are descended. Thus, Malayo-Polynesian languages share certain innovations relative to reconstructed PAn, and these innovations can be attributed to PMP. The phonological innovations are (Blust 1990) as follows:

PAn *C and *t merged as PMP *t.
PAn *L and *n merged (with some unexplained exceptions) as PMP *n.
PAn *S and *h merged as PMP *h.

These innovations are supported by about a thousand PAn and many more PMP lexical reconstructions in Robert Blust's *Austronesian Comparative Dictionary* (in preparation). The merger of PAn *C and *t in Malayo-Polynesian languages is illustrated in Table 16.1. PAn *C and *t are reflected as separate phonemes in each of the Formosan languages Atayal, Tsou, Rukai and Paiwan, but merge as *t* in Malayo-Polynesian languages, of which a scattered sample is given in the table: Tagalog (Luzon), Toba Batak (Sumatra), Uma (Sulawesi), Manggarai (Flores), Kairiru (north coast of New Guinea), Wayan (western Fiji) and Samoan, reflecting their merger as PMP *t.

The morphological innovations of PMP are more complicated and involve pronouns and verbal affixes (Ross 2005). A major set of innovations in pronouns involved a 'politeness shift', reconstructed by Blust (1977). This shift was functionally similar to the English politeness shift, whereby *you*, the second person

Table 16.1 Reflexes of Proto-Austronesian *t and *C in a sample of languages

PAn	*C	*t	ear *Caliŋa	eye *maCa	head louse *kuCu	three *tolu	freshwater eel *tuLa	seven *pitu
Atayal	*t*	*t*	*t aŋia*	—	*kut u*	*tu- a*	*tu a-qiy*	*ma-pitu*
Tsou	*ts*	*t*	—	*mtsō*	*ktsū*	*turu*	*tuŋ-roza*	*pitu*
Rukai	*ts*	*t*	*tsa iŋa*	*matsa*	*kotso*	*to o*	*tola*	*pito*
Paiwan	*ts*	*ty*	*tsa iŋa*	*matsa*	*kətsi u*	*tyə u*	*tyulya*	*pityu*
PMP	*t	*t	*taliŋa	*mata	*kutu	*tolu	*tuna	*pitu
Tagalog	*t*	*t*	*tēŋa*	*mata*	*kūto*	*ta-tlo*	—	*pito*
Toba Batak	*t*	*t*	—	*mata*	*hutu*	*tolu*	—	*pitu*
Uma	*t*	*t*	*tiliŋa*	*mata*	*kutu*	*tolo*	—	*pitu*
Manggarai	*t*	*t*	—	*mata*	*hutu*	*təlu*	*tuna*	*pitu*
Kairiru	*t*	*t*	*tiliŋ*	*mata*	*qut*	*tuol*	*tun*	—
Wayan	*t*	*t*	*taliŋa*	*mata*	*kutu*	*tolu*	*tuna*	*vitu*
Samoan	*t*	*t*	*taliŋa*	*mata*	*utu*	*tolu*	*tuna*	*fitu*

Table 16.2 Proto-Austronesian and Proto-Malayo-Polynesian second-person enclitic genitive pronouns

PAn	singular *-Su	plural *-mu
Atayal	*-su*	*-ma-mu*
Tsou	*-su*	*-mu*
Rukai	*-su*	*-mu*
Paiwan	*su-*	*nu-*
PMP	*-mu	*-muihu, *-nihu
Tagalog	*-mu*	*-ni-ño*
Toba Batak	*-mu*	*-mu-na*
Uma	*-mu*	*-mi*
Manggarai	*-m*	*-s*
Kairiru	*-mu*	*-miu*
Wayan	*-m*	*-kem̩*

(formerly only plural) pronoun, has replaced *thou*, its former singular counterpart. The Malayo-Polynesian politeness shift was considerably more complex, but one of its elements was the replacement in PMP of the PAn second person enclitic possessor singular *-su 'thy' by the corresponding PAn plural *-mu 'your'. At about the same time, the plural *-mu was disambiguated from the singular (just as speakers of some English dialects use *you-s* or *y'all* for the plural) in one of the two ways. It was either reinforced by the addition of the PAn second person free singular pronoun *iSu 'thou', giving PAn *-mu-iSu, which by regular sound change became PMP *-muihu, or it was replaced by PAn *ni-iSu 'of-thou', becoming PMP *-nihu. Table 16.2 shows second person possessor pronouns in the languages (except Samoan) of Table 16.1. The effect of the politeness shift is obvious in the singular pronouns; in Malayo-Polynesian languages, the plural pronoun has often undergone further changes in order to distinguish it from the singular. The possessor pronouns are, however, only a part of the politeness shift (Blust 1977) and of the changes that occurred in PMP pronoun paradigms (Ross 2006).

The methodologically significant point about the body of phonological and morphological innovations reflected in Malayo-Polynesian languages is that it is highly probable that they occurred just once – in PMP. Any alternative hypothesis must provide an alternative explanation of the data patterns from which these innovations are inferred.

The Austronesian 'Family Tree'

Blust's (1977) 'family tree' diagram of Austronesian languages is shown in Fig. 16.2, with the matching map in Fig. 16.1. The details of the diagram are based on the accounts of Austronesian subgrouping given by Ross (1995) and Adelaar (2004) and of Formosan subgrouping by Blust (1999). The diagram shows that the nine groups of Formosan languages are descended directly from PAn: there was no 'Proto-Formosan'. Similarly, perhaps 20–25 groups of western Malayo-Polynesian

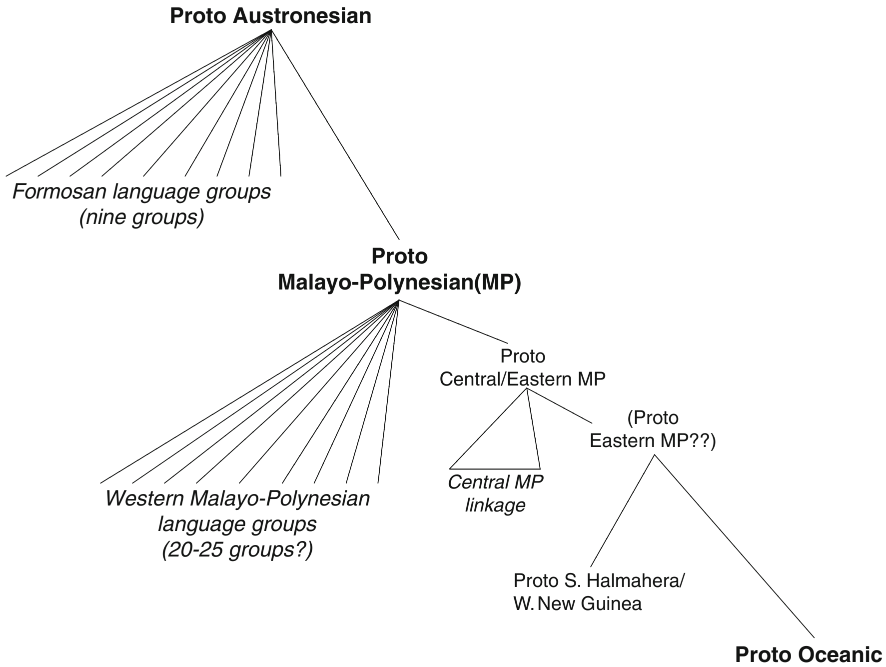

Fig. 16.2 A schematic tree diagram of the Austronesian language family (after Blust)

languages are descended directly from PMP. Despite frequent use in the literature of the term '(Proto-)Western Malayo-Polynesian', this language never existed. Progress on sorting out the history of western Malayo-Polynesian groups has been slow because of its complex linguistic prehistory: the initial settlement of the region by Austronesian speakers was apparently rapid, and there have been numerous population movements within it at different times (Blust 2005a).

The family tree model usually entails the assumption that subgroups are, like PMP, neatly defined by innovations, but divergence of sister languages is often 'a gradual and untidy affair' (Pawley 2002) which results in innovation-linked subgroups (=linkages) rather than innovation-defined subgroups. An innovation-linked subgroup is a group of languages with a network of overlapping innovations (Pawley and Ross 1995). That is, no innovation is shared by all the languages in the subgroup. Instead, for example, languages A, B and C share innovation X, languages B, C, D and E share innovation Y, and languages A, C and E share innovation Z. Innovation-linked subgroups may arise either via a dialect network from gradual diversification within a speech community or from division within an existing linkage.

The Central/Eastern Malayo-Polynesian (CEMP) subgroup is defined by an innovation set which is less significant than those which define Malayo-Polynesian and Oceanic (Blust 1993; Ross 2008), implying that Proto-CEMP speakers spent only a short period as a unified speech community.[1] Figure 16.2 shows no 'Proto-Central Malayo-Polynesian', as the Central Malayo-Polynesian languages do not share a set of innovations in common (Blust 1993). Instead they form an innovation-linked subgroup – a collection of language groups which reflect the diversification of a dialect chain descended from Proto-CEMP.

If a Proto-Eastern Malayo-Polynesian speaking speech community ever existed, this existence was even more fleeting than that of Proto-CEMP. Shared innovations are all possible innovations in vocabulary (Blust 1978 gives 56), some of which may ultimately prove to be shared retentions. If there was no Proto-Eastern Malayo-Polynesian, then its two putative daughters, Proto-South Halmahera/West New Guinea (SHWNG) and Proto-Oceanic, will instead have been daughters of Proto-CEMP. Each is a subgroup defined by a clear set of sound changes (for SHWNG, see Ross 1995) and in the case of Oceanic by other kinds of innovation too. Oceanic is the most clearly defined of all Austronesian subgroups, first identified by Dempwolff (1937). The innovation set has since undergone modifications and additions which have strengthened it (Ross 1998; Lynch et al. 2002: Chap. 4).

Historical linguistics cannot provide absolute dating, but Fig. 16.2 places events in sequence and tells us something about the speed of the Austronesian dispersal. Where a subgroup is well defined by innovations, one may infer that the speakers

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[1]Recently, Donohue and Grimes (2008) have questioned the validity of the innovations which support the PCEMP node, and Blust (2009) has defended them. If the PCEMP node is found to be without support, then the branches beneath it in Fig. 16.2 would instead extend directly from the PMP node.

of its protolanguage spent some time as a unified speech community, during which the innovations in their speech occurred. This is true of PMP, Proto-SHWNG and Proto-Oceanic (and later of Proto-Polynesian and Proto-Nuclear Polynesian). The CEMP and Eastern Malayo-Polynesian nodes in the tree are much less well defined (and Blust's Central Malayo-Polynesian node is undefined), suggesting that the putative Proto-CEMP and Proto-Eastern Malayo-Polynesian speech communities each remained unified only for a short time.

The linguistic findings match the archaeological record with regard to dispersal speed. The archaeology (below) indicates that Neolithic cultural complexes dispersed at some speed, between c. 2000 and 1350 BC, from the northern Philippines southwards into the Indo-Malaysian archipelago, and eastwards beyond New Guinea into the Bismarck Archipelago. If this dispersal can be equated with that of early Malayo-Polynesian speakers, as we suggest, then their haste is attested by the fact that Proto-CEMP and Proto-Eastern Malayo-Polynesian are just tiny blips in the rapid progress of Austronesian speakers. No significant lengths of time were spent in one place until they reached New Guinea. The marked definition of the Oceanic subgroup indicates that the Proto-Oceanic speech community remained integrated for some time, a fact which correlates with the efflorescence of the Lapita cultural complex in northwest Melanesia from about 1350 BC (Kirch 1997; Green 2003).

Alternative Mechanisms of Language Family Dispersal?

The account in the foregoing section presupposes that languages are dispersed by their speakers, i.e. movements of languages entail movements of speakers, with continuity of language from one generation to the next. This was undoubtedly the main driver of Austronesian linguistic dispersal, but another mechanism was also involved. This is language shift, i.e. the replacement by a speech community of its erstwhile language by a language formerly not their own. It is difficult to distinguish between these two mechanisms on the basis of linguistic evidence, although shift sometimes leaves its own special signs.

When a language is carried from one place to another by its speakers, there is linguistic continuity from generation to generation, and gradual, regular sound changes result in regular sound correspondences like those noted in Table 16.1.

Regular sound correspondences also remain, however, when language shift occurs. Speakers usually become fully competent in their new language, leaving little or no linguistic evidence of the shift. Occasionally, evidence remains in a set of pronunciations that are characteristic of the speakers' old language. This has happened in Madak (Austronesian, New Ireland, Bismarck Archipelago), where speakers of a Papuan language have shifted to an Austronesian language but retained a phonological system similar to the one which still occurs in a neighbouring Papuan language (Ross 1994). Sometimes, speakers retain a few items of vocabulary from their old language, as in the Austronesian languages of formerly foraging Negrito groups in the Philippines (Reid 1991, 1994). The speech of one Sissano dialect (Austronesian, north coast New Guinea) retains a few words from the Papuan language Olo. Oral history recounts that its speakers are descended from Olo speakers who shifted language a few generations ago (Laycock 1973). But often language shift leaves few linguistic clues.

For this reason, estimating what role shift has played at any period is difficult, but it is always likely to have been a secondary role, as the correlation of linguistic and archaeological evidence indicates that the Austronesian expansion was rapid, and shift could not have occurred fast enough to account on its own for the expansion. We can infer that shift was more common earlier in the Austronesian dispersal as foragers adopted the speech of agriculturalist Austronesian speakers, but less common in New Guinea, where both Austronesian and Papuan speakers continued to practise agriculture and the absorption of one group by another was less probable. In Remote Oceania (from the southeast Solomons eastwards and southwards), shift played no role at all, as these territories were previously uninhabited.

Another explanation for the spread of Austronesian says that the Austronesian languages of Melanesia are pidginized versions of more westerly Austronesian languages adopted by Papuan speakers (Capell 1943; Ray 1926). The proposed mechanism flies in the face of the evidence. The genesis of a pidgin typically entails social upheaval and the bringing together of speakers of three or more languages in circumstances in which the speakers of one language dominate the others. The pidginization hypothesis thus asks us to believe not only in numerous shifts to pidgins, but also in the repeated occurrence of circumstances appropriate to pidgin genesis. More importantly it provides no account of the shared innovations and regular sound correspondences among the Austronesian languages of Melanesia demonstrated by Dempwolff (1937). Significantly, the one Austronesian-based pidgin recorded in New Guinea at European contact, Hiri Motu, existed alongside the full-fledged Motu of the villages around Port Moresby; thus, it was no one's native language. The features which made Ray and Capell feel that these languages were 'different' from those further west are the result of contact with Papuan languages (Lynch 1981; Ross 1996), but contact does not mean the loss of generational continuity that is entailed in shift to a pidgin.

One other explanation that has occasionally been offered for the Austronesian language family is that languages which were genealogically unrelated have gradually converged until they acquired a family resemblance. There is no case known to us anywhere in the world, however, where convergence of this degree has occurred.

We are thus left with just two mechanisms to account for the *spread* of Austronesian: generational continuity as its speakers spread across the huge domain they now inhabit, augmented to a small degree by language shift.

Out of Taiwan: The Earliest Spread of Austronesian

It is all too easy to attribute too much significance to the statement 'Proto-Austronesian was spoken in Taiwan' (and perhaps not only in Taiwan). The statement simply says that the language from which all Austronesian languages are descended was spoken in Taiwan. It is, in a sense, also an admission of ignorance because PAn must itself have had an ancestry, but we have almost nothing to say about it – 'almost nothing' because there have been serious proposals that Austronesian is related to Sino-Tibetan (Sagart 2008) and to Austroasiatic (Reid 1999, 2005), but at a time depth which makes reconstruction difficult and perhaps impossible.

Two interrelated questions associated with the Out of Taiwan hypothesis have provoked various answers from Austronesianists. The first question is: What is the correct subgrouping of the Formosan languages? The second depends on one's answer to the first: Do the Malayo-Polynesian languages form a primary Austronesian subgroup, or do they subgroup with a subset of Formosan languages? Various answers to these questions have been put forward, but until Blust's (1999) division of the Formosan languages into nine groups and his assignment of the Malayo-Polynesian languages to a tenth, little of this work was based on an undiluted application of the comparative method, i.e. on the reconstruction of shared innovations. Recently, Sagart (2004) has proposed a different subgrouping, based mostly on innovations in the words for early Austronesian numerals.[2] His subgrouping is complex and places PMP in a group with the Formosan languages Kavalan and (extinct) Ketagalan, as well as the Tai-Kadai (or Kra-Dai) languages, which Sagart claims form a subgroup of Austronesian. The Tai-Kadai claim seems at first glimpse to be well founded, but it has yet to receive careful examination by someone who is expert in both Austronesian and Tai-Kadai. The Kavalan/Ketagalan claim is based on a single statement by Li (1995) which has subsequently been withdrawn (Li 1999), since more extensive data do not support it. We are thus no closer than before to knowing what the closest relative of PMP among the Formosan languages might be (and that closest relative may in any case be long extinct).

Nonetheless, the linguistic evidence is unambiguous as to the Taiwanese homeland of Austronesian and about a primary move from there to the Batanes Islands or Luzon, whence speakers of Austronesian languages rapidly settled the rest of the Philippines, much of Indonesia and the Bismarck Archipelago. There they paused for a century or two and their language evolved into Proto-Oceanic, before their descendants moved on to settle virtually every Pacific island.

Current Archaeological Perspectives: New Research in Taiwan and the Philippines

The origins of the Taiwan Neolithic lay undoubtedly in southern China prior to 3500 BC (Tsang 2005; Jiao 2007), but our focus is on Taiwan and the Philippines, with some attention paid to Indonesia and western Oceania, in order to illustrate the

[2] While this paper was in press, Ross (2009) presented an alternative account of Formosan. This affects the subgrouping of Formosan languages in Fig. 16.2 but does not otherwise alter the history presented here.

spread of Neolithic[3] assemblages southwards from Taiwan during the second millennium BC. We accept that the New Guinea highlands witnessed an independent early Holocene development of fruit and tuber agriculture roughly contemporary with that for rice and millet in East Asia (Denham et al. 2004), but this observation does not impinge directly on the immediate issues of Austronesian origin under examination here.

Understanding of prehistory in Taiwan and the northern Philippines (Fig. 16.3) has recently developed very rapidly. The main breakthroughs have come with the established presence, by at least 2800 BC, of rice and foxtail millet cultivation during the Dabenkeng (Early Neolithic) culture of Taiwan (Tsang 2005; Tsang et al. 2006); with the documentation of a sixfold or greater increase in site numbers in eastern Taiwan during the course of the third millennium BC (Hung 2005: 126); and with the recovery of fine-grained ceramic evidence for the spread at about 2200 BC of Neolithic material culture from Taiwan, through the previously uninhabited Batanes Islands, to northern Luzon (Bellwood and Dizon 2008; Hung 2005, 2008). This Neolithic spread carried, not necessarily all at one time since multiple movements occurred, red-slipped but initially undecorated pottery with specific rim forms and body shapes, pottery spindle whorls, stone bark cloth beaters and adzes, adzes and bracelets of Fengtian (eastern Taiwan) nephrite, Taiwan slate knives and projectile points, notched pebble net sinkers, pigs, possibly dogs, and rice (a prehistoric presence of millet still remains uncertain beyond Taiwan). A precise archaeological homeland within the island of Taiwan is not yet identifiable, and it is possible that groups from different regions of southern Taiwan were involved in the movements, with the closest ceramic parallels at present being focused on the southeastern coastline. Figure 16.4 summarizes the archaeological correlations between Taiwan and the northern Philippines, and Table 16.3 presents an ordering of Taiwan, Batanes and northern Luzon archaeological sites through time in terms of their ceramic and other contents.

In the case of the Batanes Islands, to the immediate south of Taiwan, excavations in five caves and rock shelters with plentiful ceramic-period occupation indicate that humans did not reach these wind-swept islands, protected by relatively rough seas and sometimes strong ocean currents, until the Neolithic. There is absolutely no trace of prior hunter–gatherer occupation or flaked lithic tool manufacture. Luzon, to the contrary, had Palaeolithic hunters and gatherers in occupation since at least 24,000 years ago, so the first Neolithic arrivals must have interacted with these groups, as Mijares has shown for the Peñablanca Caves near Tuguegarao (Mijares 2005).

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[3]The term 'Neolithic' in this context is taken to include material culture assemblages that contain pottery and ground stone tools, lack metal artefacts and do not consist entirely of flaked stone. This may seem an old fashioned definition, but in Island Southeast Asia such sites form a related cultural array from the perspective of this paper, with a chronology from before 3000 BC (Taiwan) to 500 BC/AD 1 (depending upon the date of arrival of iron and cupreous metals, usually together). The economic evidence from Island Southeast Asian prehistory is not sufficient at present to support a terminology that highlights agriculture or animal husbandry. However, large numbers of cognates with stable meanings in the areas of agriculture and animal husbandry can be reconstructed for all major Austronesian historical stages, from Proto-Austronesian onwards (Blust 1976; Bellwood 1997: 106–111; Zorc 1994).

Fig. 16.3 Taiwan, the Batanes Islands and northern Luzon.

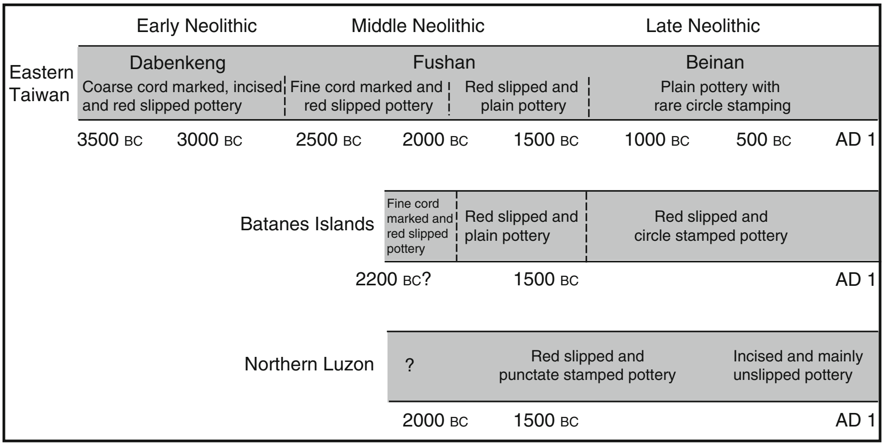

Fig. 16.4 The linkages between Neolithic assemblages in Taiwan and the northern Philippines.

The Spread of Neolithic Pottery from Taiwan into the Philippines

The newly excavated ceramic data establish the development of a tradition of red-slipped pottery manufacture, with diminishing cord marking, in southeastern Taiwan by at least 2200 BC. Figure 16.5 illustrates this gradual transition from cord marking to red slip in the stratified layers of Xiaoma Cave 10. A key single-phase open site nearby is Chaolaiqiao (Hung 2008), with almost 100% red-slipped pottery, accurately dated by AMS C14 to c. 2200 BC (Table 16.3). By 2000 BC, this red-slipped and basically non-cord marked tradition had spread to previously uninhabited Batanes, as documented in Reranum and Torongan Caves on Itbayat. Pottery vessels in this phase were mainly globular in shape, with ring feet, tall everted rims and without body decoration apart from the red slip (Fig. 16.6). Reranum and Chaolaiqiao still have some extremely rare cord marking, and the close similarities in rim forms between these two sites raise the possibility that a direct migration from southeastern Taiwan to Itbayat could have occurred between 2200 and 2000 BC. Apart from Reranum Cave, not a single sherd of cord marked pottery has ever been found in any other archaeological sites in Batanes or northern Luzon.

For the initial 500 years or so of Neolithic settlement in the Batanes Islands, it appears that this red-slipped plainware tradition remained dominant. The situation is not so clear for northern Luzon (Hung 2008), where Ogawa (2005) has suggested that both stamped and red-slipped pottery occurred at the base of the pottery sequence in the Cagayan Valley. New observations from Nagsabaran confirm this (Hung et al. in press), and indicate that a very significant tradition of stamping the exterior surfaces of pottery had certainly appeared in Cagayan by at least 2000 BC, given the dates run so far from Nagsabaran and Irigayen (Table 16.3). Characteristic motifs were created by circle stamping in Batanes and both circle and punctate stamping in the Cagayan Valley, the latter using a multiple-toothed tool (Fig. 16.7). A closely related tradition of punctate stamped pottery decoration is reported from the Mariana Islands in

Table 16.3 Cultural inventories for archaeological sites with red-slipped pottery in Taiwan and the northern Philippines, from c. 2500 BC onwards

Site	Region	Cal. date range BC	Paddle impressed sherds	Stamped sherds (circle and punctate stamped)	Fengtian nephrite and Taiwan slate	References
Xiaoma Cave	SE Taiwan	2700–2400 (2 marine shell)	Fine cord marked, diminishing in quantity through time	None	Uncertain	Huang and Chen (1990)
Youxianfang	SW Taiwan	2360–1600 (7 charcoal)	Rare fine cord marked	None	Both	Tsang et al. (2006)
Chaolaiqiao	SE Taiwan	2250–2000 (2 charcoal AMS)	Rare fine cord marked	None, but rims are red painted	Both	Hung (2005)
Reranum	Itbayat Island, Batanes	Disturbed deposit	Rare fine cord marked	One circle stamped, perhaps intrusive	None	Bellwood and Dizon (2008)
Torongan	Itbayat	2200–1510 (2 charcoal AMS, 4 marine shell)	None	Rare circle stamped above the main occupation	None	Bellwood and Dizon (2005)
Sunget and Savidug	Batan and Sabtang Islands	1250–700 [5 charcoal (3 AMS)]	None	Circle stamped	Both	Bellwood and Dizon (2005)
Anaro	Itbayat	1040 BC to AD 240 (2 charcoal AMS)	One undated cord marked sherd	Circle stamped	Both	Bellwood and Dizon (2008)
Nagsabaran	Cagayan, N. Luzon	2200–1050 (3 charcoal, 1 AMS bone)	Basket and carved paddle impression	Punctate stamped and zone incised	Nephrite only	Hung (2005, 2008), Piper et al. (2009), research in progress
Irigayen	Cagayan	1550–1000 (4 charcoal)	None reported	Punctate stamped and zone incised	None reported	Ogawa (2005)
Magapit	Cagayan	1300–400 (2 charcoal, 2 riverine shell)	None reported	Punctate stamped and zone incised	None reported	Aoyagi et al. (1993)
Pamittan, Andarayan	Cagayan	>2000–1000 (4 charcoal)	None reported	None reported	None reported	Tanaka and Orogo (2000)
Callao Cave	Cagayan	1690–1520 (1 charcoal)	None reported	None	None	Mijares (2005)

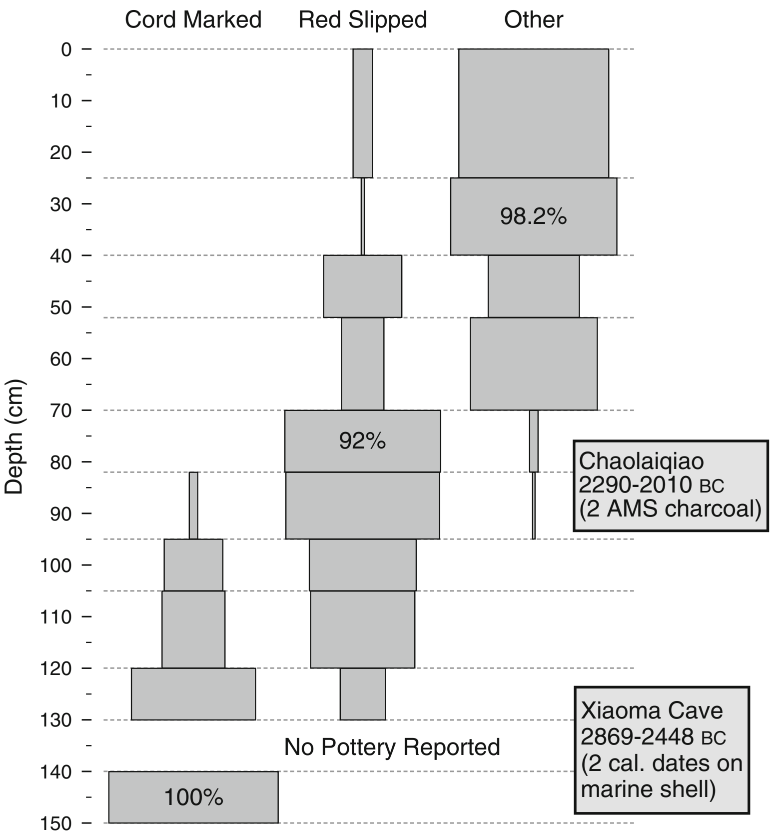

Fig. 16.5 The transition from fine cord marked to red slipped pottery in Xiaoma Cave No. 10, south-eastern Taiwan (data from Huang 1991). The two dates from Chaolaiqiao are interpolated by seriation.

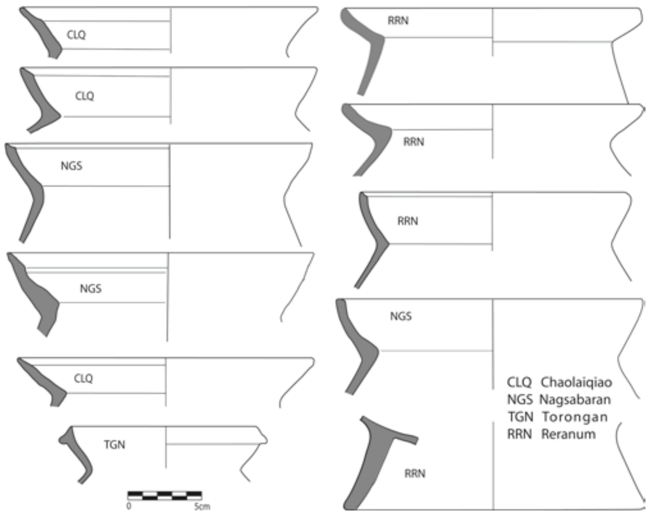

Fig. 16.6 Selected rim forms from Chaolaiqiao (Taiwan), Reranum and Torongan (Itbayat Island), 2200 to 1500 BC.

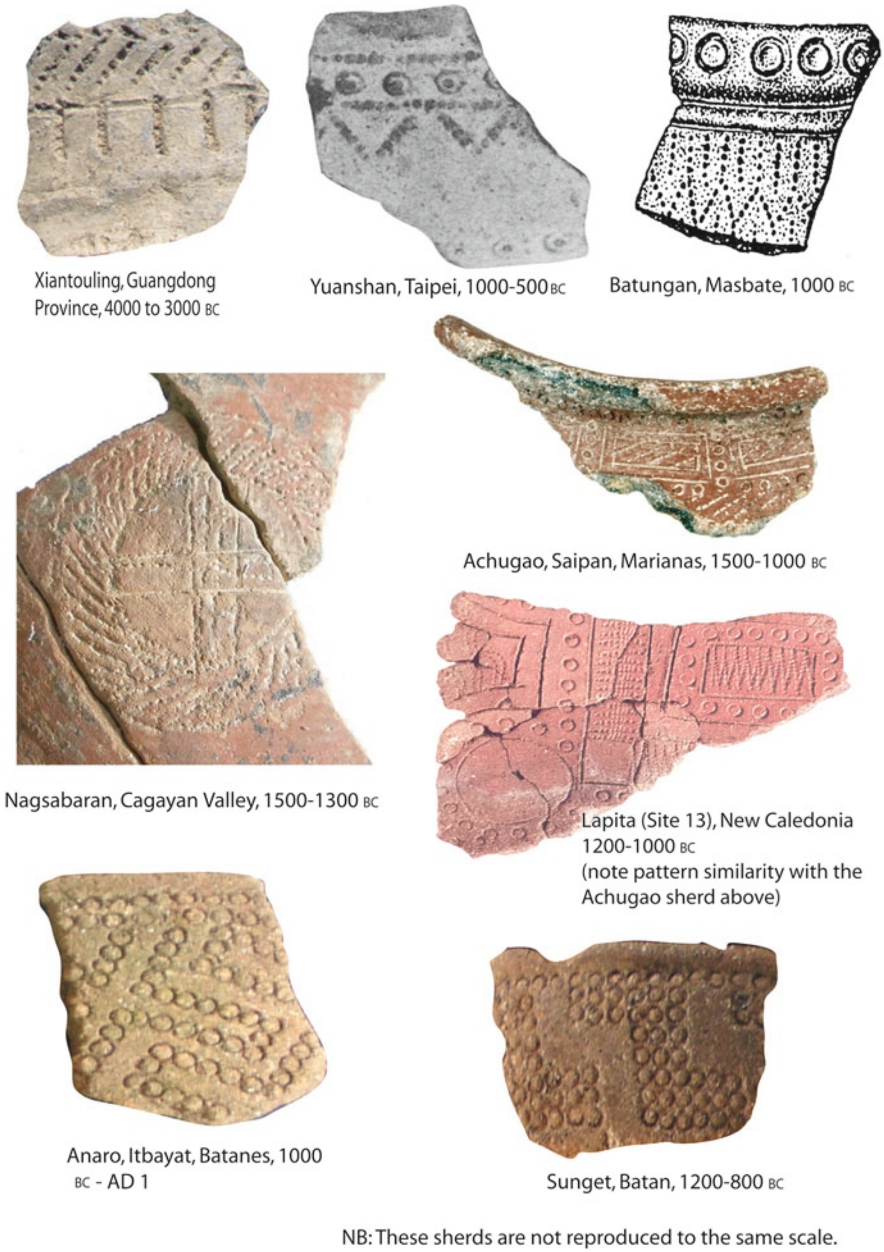

Fig. 16.7 Stamped pottery in southern China, Island Southeast Asia and western Oceania, 1500 to 1000 BC. Credits: Xiantouling - Yang Yaolin, Shenzhen Museum; Yuanshan – Department of Anthropology, National Taiwan University; Batungan – Social Science Research Institute, University of Hawai'i; Achugao – Brian Butler, Center for Archaeological Investigations, Southern Illinois University at Carbondale; Lapita – Christophe Sand, Service des Musées et du Patrimoine, Nouméa.

western Micronesia, in sites such as Achugao on Saipan, where it also dates from about 1500 BC onwards (Carson 2008; Butler 1995: see Fig. 16.7). Comparisons of Marianas stamped pottery with that in the Cagayan Valley agree with the linguistic evidence that the Marianas Islands were indeed settled from Luzon, or at least somewhere in the Philippines (Reid 2002). An allied form of dentate and circle stamping with similar motifs is also a very typical feature of Lapita pottery in western Melanesia (1350–750 BC – Green 2003), and occurs here with white lime or clay infilling of the designs, precisely as in Batanes, Luzon and the Marianas (Hung et al. in press).

These stylistic resemblances render it highly *unlikely* that this stamping tradition was invented independently in each location, and virtually identical designs occur on sherds from Achugao and the site of Lapita in New Caledonia (Fig. 16.7). In the Lapita and Marianas sites, the stamping apparently commenced with the first appearance of pottery (although Carson 2008 raises some uncertainty about this for the Marianas), and the deeper ceramic chronology in the Philippines suggests that the immediate foundation of the stamping tradition that spread into Oceania was perhaps here. Slightly different and mainly incised forms of decoration occurred at about 1000 BC in the Kalumpang sites in western Sulawesi (Simanjuntak et al. 2007: 51–52) and at Bukit Tengkorak in Sabah, but in both cases also preceded by plain red-slipped pottery as in Batanes. At Bukit Tengkorak, the lower plain red-slipped ware occurs with Talasea obsidian imported from the Lapita heartland region in the Bismarck Archipelago (Bellwood 1997: 224; Chia 2003). From a broader perspective, the stamping tradition appears to have its greatest antiquity in the Yangzi region of central China (Rispoli 2008), and was widespread also in the Neolithic of Thailand and both northern and southern Vietnam from about 2000 BC onwards (Higham 2002; Wiriyaromp 2007). Its possible appearance at Nagsabaran by 2200 BC makes the Cagayan Valley the oldest location so far for this type of decoration in Island Southeast Asia, and an ultimate East Asian mainland origin seems certain. The stylistic origins of Lapita pottery were not, therefore, indigenous to the western Pacific, but it is interesting that the stylistic apogee of the Island Southeast Asian stamping tradition was attained in Lapita sites in the Bismarck Archipelago and adjacent areas, emphasizing that migration can be very fertile ground for stimulating creativity.

One archaeological and linguistic possibility that might be drawn from the above is that one of the initial Malayo-Polynesian movements into Oceania went from Luzon via the Marianas Islands into Melanesia, at a time of relative linguistic unity before the innovations that define the present nodes in the Malayo-Polynesian family tree began to accumulate (especially for Proto-Oceanic in the Bismarck Archipelago). However, this possibility requires further research in both archaeology and linguistics.

How Significant Were Rice and Pigs in Early Austronesian Dispersal?

It has recently been suggested that the apparent failure of rice cultivation to spread widely in eastern Indonesia, or into prehistoric Oceania, argues against Neolithic movements out of Taiwan (Oppenheimer 2004). However, this could be due to lack of observation – remains of both rice and millet were universally absent from the Dabenkeng phase in Taiwan (3500–2500 BC) until both were found in unprecedented carbonized quantities in hitherto unique waterlogged conditions dating to c. 2800 BC in the Tainan Science-Based Industrial Park (Tsang 2005; Tsang et al. 2006). In fact, the list of sites in Island Southeast Asia in which evidence for rice has been found, particularly as a result of careful analysis of pottery or phytoliths, is rapidly increasing. It was present in pottery at Andarayan in the Cagayan Valley prior to 1400 BC (Snow et al. 1986). In Malaysian Borneo, rice remains have been reported from contexts dating variously between 2200 BC and AD 1 from Gua Sireh in Sarawak (Ipoi 1993), in 35 other sites in Sarawak including Niah (Doherty et al. 2000), and from Bukit Tengkorak (Doherty et al. 2000) and Madai Cave (unpublished phytolith observations) in Sabah. Put simply, earlier archaeologists probably failed to recognize cereal remains because of poor preservation conditions and lack of observational technology. Furthermore, the above listed Borneo sites, apart from Gua Sireh, are far from fertile rice-growing terrain and, in the case of the Niah Caves (Barker 2005), supported a continuing hunter–gatherer population (Punan) until the Iban incursions of the nineteenth century. Such sites are of questionable relevance for any discussion of agricultural origins in Island Southeast Asia (but see Krigbaum 2005 for stable carbon isotope results on skeletons from Niah that are suggestive of some agricultural subsistence in the general vicinity during the Neolithic).

Nevertheless, current information supports a decrease in the significance of rice with movement towards the equator and towards the Pacific (Spencer 1966), where it was universally absent except in the Marianas Islands. Dewar (2003) discusses reasons of climatic variability for the non-significance of rice in many parts of eastern Island Southeast Asia, suggesting that ENSO-related rainfall unreliability was a major reason for its failure to spread into Oceania. So far, our attempts to identify rice in phytolith samples from Batanes have not been successful, but since rice is only grown there today as a minor monsoon crop we would not necessarily expect to find it.

The domestication of the pig in Island Southeast Asia is also currently a topic of considerable debate (Larson et al. 2005, 2007; Bellwood and White 2005), which is rendered complex by the wide distribution of endemic wild suid in mainland Asia, western Indonesia (Sundaland) and Sulawesi. Domesticated pig bones (*Sus scrofa*) are widespread in Neolithic sites in Taiwan, and occur together with a smaller toothed native Luzon wild pig species by 2200 BC in the Neolithic lower layer at Nagsabaran in the Cagayan Valley (Piper et al. 2009). Unfortunately, the oldest Batanes sites, Reranum and Torongan, contain no animal bone or even free charcoal, apart from food residues burnt on to pottery, but domesticated pig was present by 1200 BC in Sunget on Batan (the Batanes Islands have no wild pigs).

Currently, it is not clear to what degree pigs travelled with Neolithic migrants within Island Southeast Asia. Larson et al. (2007) claim two major domestication foci. One suid mtDNA lineage was evidently taken from southern China through Taiwan into the Philippines by 2200 BC, but was not transferred successfully to the Marianas Islands, where pigs were absent in prehistory. Thus, the likely transfer of material culture discussed above from the Philippines via the Marianas into the

Lapita sphere of Near Oceania did not carry suid. Relevant data are so far lacking for Borneo and Sulawesi pigs, but a separate suid mtDNA lineage appears to have been transferred from Mainland Southeast Asia (see below) along the Sunda Chain into Oceania (Larson et al. 2007). Hopefully, these possible movements can be examined in the future from ancient DNA, should meaningful samples be extracted successfully from Neolithic pig bones in the Southeast Asian tropics.

From Taiwan to Borneo

Within Sarawak (East Malaysia), the caves of Gua Sireh and Niah West Mouth have both yielded fine cord marked pottery that is different from the Philippine and eastern Indonesian red-slipped tradition (Solheim et al. 1959, Fig. 2; Ipoi 1993, Plates 28, 29). That from Niah remains poorly dated and stratigraphically ambiguous, but at Gua Sireh the cord marked and associated paddle impressed pottery dates from 2200 BC onwards and, as noted above, is associated with rice husks and grains in sherd fabrics. Bellwood (1997: 237–238) formerly suggested a link between the Gua Sireh assemblage and the Malay Peninsula Neolithic, but now feels that a Taiwan origin perhaps via western Luzon is more likely, given the existence of both cord marked and paddle impressed pottery in Taiwan before 2500 BC, and thus before any firmly dated occurrences of such pottery on the mainland of Southeast Asia. However, the incised and stamped pottery that characterized both northern and southern Vietnam during the second millennium BC has a few interesting motif design parallels in Island Southeast Asia and Oceania that currently date from about 1500–500 BC, especially using punctate stamping and circles.[4] This perhaps reflects secondary diffusion between Island Southeast Asia and Vietnam, subsequent to the initial phase of Austronesian dispersal from Taiwan, and this possibility is of course of interest for the possible pig movements from Vietnam discussed above.

Neolithic movements from Taiwan and the Philippines, across or around the South China Sea to Mainland Southeast Asia, are much better attested at a later date. The Early Metal Age Sa Huynh culture of southern Vietnam, with its strong Philippine and Niah ceramic and jade parallels, was probably associated with the Malayo-Polynesian Chamic languages that still survive in central Vietnam. The Chamic languages subgroup with Malay, and Blust (2005b) suggests establish-

[4] Reports on recent excavations in the Vietnam Neolithic sites of Man Bac (Ninh Binh Province) and An Son (Long An Province) are in preparation, under the editorship of Marc Oxenham, Peter Bellwood and others. Both these sites date to the second millennium BC and both have punctate and circle stamping, albeit on very different vessel forms than those in Taiwan and the northern Philippines. These sites do not provide convincing origin assemblages for the northern Island Southeast Asian Neolithic, and it is quite possible that the stylistic transfers went the other way, from Taiwan via the Philippines to Vietnam.

ment in Vietnam at c. 300 BC, as a partial result of the development of iron smelting in western Borneo. But earlier Malayo-Polynesian movements to Mainland Southeast Asia and western Indonesia are also quite possible, and it is clear that both the archaeological and linguistic records of the western regions of Island Southeast Asia need considerable research. Western Java, in particular, has long been known to hold sites with cord marked pottery, but none have been accurately dated.

Out of Taiwan into the Philippines: The Archaeological Evidence

Four factors render movement of Neolithic material culture from Taiwan into the northern Philippines a virtual certainty:

1. The strong parallels in material culture between 2200 and 1500 BC from southern Taiwan into the northern Philippines, reinforced by the southwards movement of artefacts of Taiwan slate and positively sourced Taiwan nephrite (two bracelets of which have been found in Nagsabaran).
2. The chronological priority in Taiwan of the artefact types concerned, involving an unbroken continuity since at least 3000 BC in cord marked and red-slipped pottery (Hung 2005), spindle whorls, stone bark cloth beaters, perforated slate points, notched net sinkers, and adzes and ornaments of Taiwan nephrite (Hung 2004; Hung et al. 2007). To these can be added the oldest radiocarbon dates for rice and millet in Southeast Asia (Tsang et al. 2006).
3. The absence of closely related material culture before this time span in nearby regions such as Indonesia or Vietnam.
4. The absence of a prior population in Batanes. This implies a movement of people to establish colonization, not an adoption of Neolithic material culture by an indigenous hunter–gatherer population (as probably happened to a degree in the Peñablanca Caves in Luzon – Mijares 2005).

From an archaeological perspective, the progression of Neolithic material culture assemblages of ultimate East Asian/Taiwan origin through the regions settled by ancestral Austronesian speakers appears can be plotted chronologically as in Fig. 16.8. We should not forget that this migration took 4,000 years to unfold, from Taiwan to New Zealand, and perhaps 6,000 years from southern China. In addition, we do not deny that populations already resident in Island Southeast Asia and Melanesia contributed cultural capital in the form of some shell artefact technologies, tuber and fruit crops of western Pacific (especially New Guinea) origin, and flaked lithic traditions (found commonly mixed with Neolithic assemblages in caves). A baseline migration of Neolithic populations from Taiwan, followed by cultural reticulation with resident forager groups, prior arboriculturalists in some coastal regions of western Oceania, and previously established rice farmers in central and northern coastal Mainland Southeast Asia, can be applied to both the linguistic and the archaeological data.

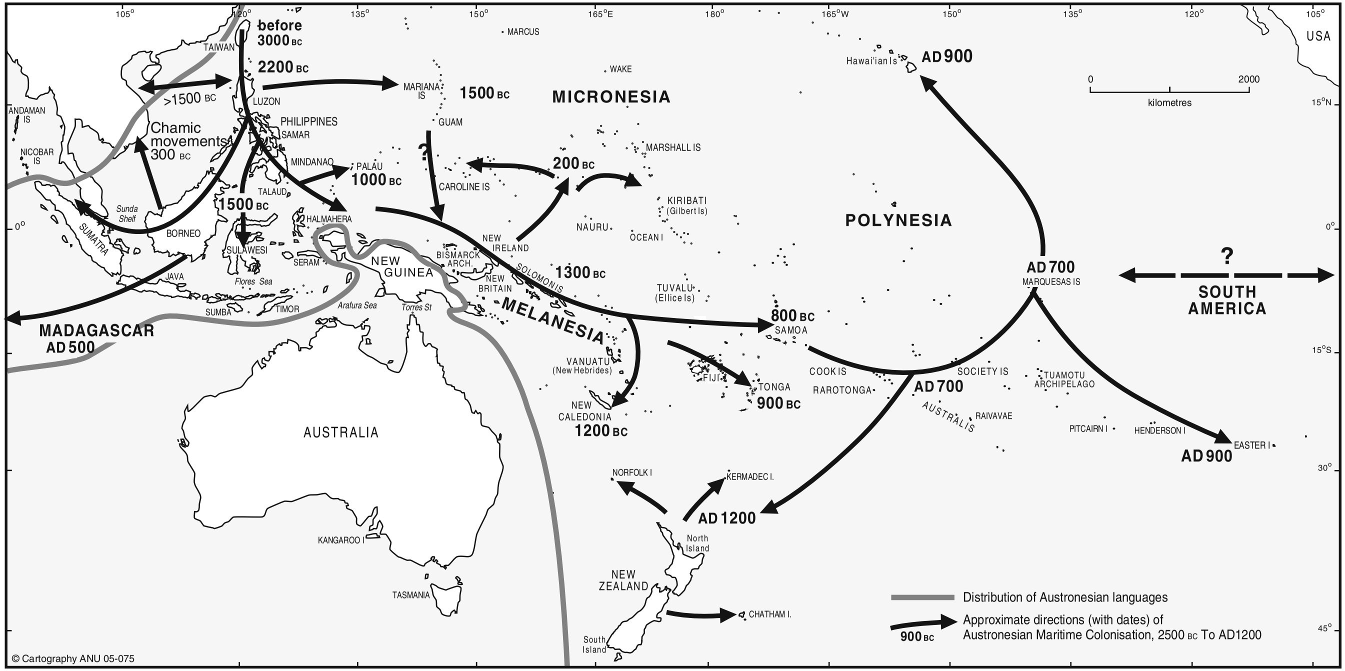

Fig. 16.8 The dispersals and interactions of Austronesian-speaking populations according to linguistic directionality and archaeological chronology.

Out of Taiwan: The Genetic Evidence

Modern genetics is a very powerful element of biological anthropology, but has no pre-eminent claim to capture an accurate reconstruction of recent human history. Rather, it is probably better employed to provide critical tests of hypotheses arising from other disciplines. However, this is not to suggest that human genetics is incapable of generating novel hypotheses in its own right concerning pattern and process in human evolution. A multiplicity of ideas has been advanced by geneticists to explain the presence of closely related peoples on widely dispersed Pacific Islands. Beyond them all, one key question remains hotly contested – did the early movement of Austronesian-speaking people proceed out of Taiwan or from elsewhere in Island Southeast Asia?

It is always difficult to find telling pieces of genetic evidence capable of untangling these two hypotheses. The central question is whether Island Southeast Asia, excluding Taiwan, is sufficient by itself to present a coherent account of Austronesian origins. As is shown below, some role for Island Southeast Asia as a *proximal* source is absolutely required by the genetic data. But excluding Taiwan from the Island Southeast Asia homeland in favour of some other *distal* source, e.g. the Malay Peninsula, is not suggested by the balance of the genetic evidence, not to mention linguistic or archaeological perspectives. However, we must note one methodological caveat; evidence against one idea is not necessarily support for another, unless they really are strictly exclusive alternatives.

We begin in the early years of research into population genetics, when human leucocyte antigen (HLA) markers (Serjeantson 1989), globin gene variants (Hill and Serjeantson 1989), and hypervariable nuclear markers (Flint et al. 1989; Martinson et al. 1993) all established beyond reasonable doubt that modern Polynesian and Micronesian gene pools contain contributions from both Southeast Asia *and* Melanesia. They also demonstrated that genetic variation across Remote Oceania decreases as one moves eastwards, away from Near Oceania.

Mitochondrial DNA

Contemporary with the above developments, an mtDNA feature of special significance (Wrischnik et al. 1987) was identified at high frequency in Austronesian-speaking populations (Hertzberg et al. 1989). This has become known as ‘the 9-bp deletion’, and is the result of mutational loss of one of the two copies of a short tandem repeat (Redd et al. 1995). In Austronesian-speaking populations, it is always associated with the presence of the nucleotide C at position 16,189 to define mtDNA haplogroup B (Trejaut et al. 2005).

One of the first findings from mtDNA analysis was that there are marked differences between coastal and highland populations in Papua New Guinea, corresponding with Austronesian and Papuan-speaking peoples (Stoneking et al. 1990) and probably reflecting past settlement history. Later, investigators (Lum et al. 1994; Redd et al. 1995; Melton et al. 1995, 1998; Sykes et al. 1995; Hagelberg et al. 1999) elaborated the basic account and described other important diagnostic markers. In short, a series of nucleotide substitutions are argued to have accumulated in hypervariable segment I (HVS-I) of the mtDNA control region (*aka* the D-loop) of the B haplogroup in the following order: 16,217C → 16,261T → 16,247G. The first two correspond to haplogroups B4 and B4a (Trejaut et al. 2005). Collectively, these three nucleotide substitutions have together become known as the 'Polynesian CGT Motif' (Melton et al. 1995).

The frequency of the full Polynesian CGT Motif increases from west to east along the settlement route, as documented by Hagelberg et al. (1999). The genetic trail that ultimately resulted in its appearance tracks back to Taiwan, which was nominated 'The Proto-Austronesian Homeland in Asia' by Melton et al. (1998). However, Richards et al. (1998) make the telling, and to some commentators critical, observation (Oppenheimer and Richards 2001) that the substitution at position 16,247 made its first appearance in Island Southeast Asia, rather than in Taiwan. They also present molecular clock calculations that place the date of this substitution earlier than the Neolithic settlement of Taiwan. We return to these ideas below.

The mtDNA haplotypes found across Oceania also belong to several groups distinguished from one another by their possession of one or more unique nucleotide substitutions in HVS-I (see Whyte et al. 2005 for a recent list). These can be classified into three haplogroups, each with several more or less common member haplotypes. These clusters were first described by Lum et al. (1994) as 'major lineage groups I-III'. These findings were borne out by another large study (Sykes et al. 1995); Group I (the only one with the Polynesian CGT motif) was attributed to Taiwan, Group II to Indonesia, and Group III to Melanesia, specifically Papua New Guinea.

A later phase of mtDNA research saw investigators introduce analysis of nuclear markers to complement their studies, in recognition that mtDNA phylogenies are vulnerable to gender-biased processes and incomplete lineage sorting. This body of work includes examination of simple tandem repeat (STR) loci to look at relationships between populations and to measure diversity of biparental markers within mtDNA haplotypes (Lum et al. 1998), testing *Alu* markers whose unidirectional expression (irreversible insertion events) allows directionality to be tracked (Melton et al. 1998) and extended HLA analyses (Hagelberg et al. 1999; Mack et al. 2000). There is general agreement among investigators that these newer data are more or less congruent (give or take a small number of notable exceptions discussed later) with the mixed ancestry story for Polynesians that has emerged from the mtDNA work. However, they do differ slightly with respect to the relative genetic contributions from Southeast Asia and Melanesia. The mtDNA data suggest an 85:15 ratio, whereas the nuclear loci give estimates closer to 50:50.

Paternal Lineages Tell a Similar, But Not Identical Story

The molecular methods for measuring genetic variation resident on Y chromosomes developed more slowly than those for mtDNA. However, key contributions have come from Hagelberg et al. 1999, Su et al. 2000, Kayser et al. 2000, 2001, 2003, Underhill et al. 2001 and Hurles et al. 2002. The general approach involves hierarchical ordering of single nucleotide polymorphisms (SNPs) with more or less exotic names (e.g. YAP and RPS4YC711T) to create haplogroups, and then using STR loci (DSY19, DSY390, etc.) to measure levels of genetic variation within the haplogroups. The apparent state of nomenclatural anarchy has now been resolved with the advent of a set of recommendations from the Y Chromosome Consortium (YCC 2002).

A good recent account of this phase of the international research effort can be found in Cox and Lahr (2006). In brief, Y chromosomes in Remote Oceanian populations seem to be derived from both Southeast Asia and Melanesia. The Asian haplotypes are typified by O-M175, which is widespread in Asia including Formosans, and Melanesian haplotypes by C-M130 and M-M4/M106. Because of repeated founder effects, the frequencies of these haplogroups vary considerably from one island group to another in Polynesia. But it is clear that both the range of Y chromosome haplotypes revealed and the STR diversity within each decreases from west to east, again reflecting historical bottlenecks (e.g. Kayser et al. 2000, 2006; Underhill et al. 2001).

Contrasting Conceptual Challenges and Preliminary Conclusions

Molecular genetics may well be one of our most powerful and objective tools but interpretation of such data must be made with caution. Unlike in archaeology, absolute dates cannot be put on molecular artefacts (i.e. mutations in DNA) by biologists, without the use of assumptions and mathematical models. Unlike in comparative linguistics, mutational directionality is only rarely fixed in changes to strings of DNA bases (the *Alu* insertion events mentioned earlier are a notable and valuable exception). Underlying phylogenetic reconstruction and molecular dating of human history from genetic markers is an assumption of a parsimonious tree-like process. Thus, phylogenetic inferences can be seriously compromised by back mutation (the so-called *hot spots* in mtDNA), cultural practice, population movements leading to reticulation and the effects of natural selection (e.g. on globin genes). Restricted sampling with respect to loci or populations can lead to further complications. For instance, Underhill et al. (2001) observed that the majority of Y chromosomes in New Zealand Maori traced back to Melanesia with only 6% tracking to Asia. This percentage is far less than that implied by the recent analysis of Cox and Lahr (2006).

So what can be decided, given the above disclaimers? It is already clear that some ideas can be confidently discounted as untenable. The 'entangled bank' concept borrowed from Darwin by Terrell (1988), while persuasive as a general observation about human behaviour in certain situations, fails when it is applied to an exclusively Near Oceanic origin for the peoples of Remote Oceania because of the widespread distribution of recent Asian genotypes among these populations. Asian genetic lineages are also widespread and abundant in Island Melanesia (see Cox 2005; Merriwether et al. 1999, 2005). However, Terrell's (1988) model does make a contribution in raising awareness that human movements are not necessarily simple linear processes and may involve diffusion of genes between many different sources, as stated in more complex models (e.g. Green 2003).

In contrast, a relentless movement of Austronesian-speaking peoples south and east across the Pacific with virtually no interaction with Papuan-speaking residents at all is also untenable, although we are not aware that anyone has supported this view in recent years. The fact of the sizable Melanesian content of the Polynesian gene pool, as explained earlier, is quite sufficient to lay this to rest. This viewpoint is often mistakenly blamed on the so-called Express Train model that originated with Jared Diamond (1988; see also Bellwood 1991), although these authors (Diamond and Bellwood 2003) never made a claim for total population replacement. Indeed, Bellwood and Diamond (2005) have made it quite clear that they reject explicit replacement per se. In this regard, Friedlaender et al. (2008) recently carried out an extensive survey of genetic admixture in Melanesia. Their study employed 687 microsatellites and 203 indel markers across 41 populations including Formosan and Maori external reference groups. They concluded that admixture between Papuans and Austronesians was real but more limited than previously supposed. Their analyses include phylogenetic trees and they report that their study 'supports the position that an expansion of peoples from the general vicinity of Taiwan is primarily responsible for the ancestry of Remote Oceania'.

Thus, a model that favours both migration via Taiwan and admixture is the most likely for Austronesian population history, and we suggest this simply be termed the 'Out of Taiwan' hypothesis (Bellwood and Dizon 2005, 2008), preferring to eschew temporal references to express trains and slow boats. This model originated in genetics with Melton et al. (1998, as explained earlier), and has come into apparent conflict with a 'slow boat' model that emphasises 'Out of Island Southeast Asia' (Richards et al. 1998, *aka* 'Eden in the East', after Oppenheimer 1998). To resolve this conflict in favour of the slow boat view, one must demonstrate that Island Southeast Asia is sufficient as a genetic source, with the exclusion of Taiwan. Should any part of the Taiwanese gene pool be shown to be necessary to the process, and not just a derivative product of back migration, then the slow boat also becomes a sub-element of 'Out of Taiwan'.

Some Exciting New Developments

Three recent lines of evidence bear directly on the position of Taiwan in Austronesian origins. First, the central claim of Richards et al. (1998), Oppenheimer (1998, 2004), and Oppenheimer and Richards (2001), that Austronesian dispersal originated in Wallacea rather than Taiwan, hinges exclusively on molecular clock calculations. These are always risky undertakings (see Chambers 2006 for comments on this particular case), and it is prudent to regard them as provisional. Indeed, Cox (2005) has shown that calculation of the ρ statistic used by Richards et al. (1998) is highly vulnerable to the underlying structure of the data, and suggests the following:

> The extant mtDNA evidence can no longer be viewed as favouring a Polynesian origin in eastern Indonesia, but instead remains consistent with an origin of proto-Polynesian peoples in southern China and Taiwan.

Second, Trejaut et al. (2005) have characterized several full mtDNA sequences from Formosans. They found that nucleotide substitution at position 10,238 defines B4a1 within B4a, and the combination of changes at 146, 6719, 12,329 and 15,746 defines B4a1a within B4a1. It is upon this background that the final 16,247G substitution occurred to give rise to the Polynesian CGT Motif. These authors found that the archaic B4a1a haplotype is particularly common in the Amis population of eastern Taiwan, and conclude that:

> …a new subclade, B4a1a, endorses the origin of Polynesian migration from Taiwan.

In turn, this conclusion has itself been challenged by Hill et al. (2007), who point out that the B4a2 branch of the mtDNA network is more common in Taiwan. However, their wider claim that '… major rethinking is needed with regard to the prehistory of the region' still hinges on assumptions about mutation rates and molecular clock calculations, and also on unorthodox interpretations of the archaeological and linguistic evidence. The further study by Soares et al. (2008) on mtDNA haplogroup E shares many of the same characteristics, but some may argue that they make a somewhat better case for this part of the gene pool.

Thirdly, Chambers et al. (2002) remark on the close similarities in allele frequencies at alcohol metabolizing loci between New Zealand Maori and several Formosan groups, in contrast with those found in mainland China. This must be regarded as circumstantial evidence at best, and the authors recognize (Marshall et al. 2005) that in some respects it is an 'absence of evidence argument' since there are no comparable data from the Philippines and Indonesia upon which to base conclusions. However, when one considers these further facts in the light of the above mtDNA findings, the case of Taiwan starts to look compelling.

Pierson et al. (2006) have added further weight to the above assessment through their analysis of 137 full length mtDNAs from across the Pacific (including 19 all new sequences). They use the MinMax Squeeze procedure to create provably minimal haplotype networks and date the divergence of significant nodes via a variety of molecular clock calculations. A detailed and well-dated tree for the B4a1a haplogroup emerges that supports the newly 'orthodox' settlement model of Remote Oceania via Near Oceania, including Out of Taiwan. They note that the distribution of the B4a haplogroup in Southeast Asia is still required to fill the gaps in this newest, but now much more detailed, story.

Finally, two recent studies were published while this chapter was in press and some of the gaps are filled along the genetic trail. First, Tabbada et al. (2010) show

how the Philippine gene pool links Taiwan with Island Southeast Asia and, second, Soares et al. (2011) show that the Polynesian motif arose in the Bismarck Archipelago, and not Island Southeast Asia. These new findings are entirely consistent with the account we have presented above, and are described at length and in much the same form by Wollstein et al. (2010) and by Kayser (2010). The study by Soares et al. (2011) presents molecular clock dates that are at odds with our chronology, which is based on extensive evidence from other disciplines. Their new calculations are based on whole mitochondrial genome data and allow for the effects of natural selection, in order to accommodate the shortcomings recognized earlier (Soares et al. 2009). This helps to bring the dates forward and reduces the confidence intervals, but not sufficiently to bring them into line with received wisdom. Once again, this leads the present authors to urge caution when interpreting molecular clock estimates which depend on inbuilt assumptions both implicit and explicit. Finally, we note the estimate of Kimura et al. (2008), based on extensive nuclear SNP data, that the derivation of the gene pool of Austronesian speakers in Oceania is 70% Asian and just 30% Near Oceanian. This shows that a significant historical exchange of genes did indeed take place, but never to the extent of complete admixture. The sex-biased nature of this process, as explained here, and by Wollstein et al. (2010), is not apparent in their study due to the nature of their data.

Closing Remarks on Genetics

Although molecular genetics serves well as the hand maiden of other disciplines, it has generated an independent perspective on the Out of Taiwan movements of early Austronesians. It has also revealed evidence for a gender-biased gene flow in putatively matrilocal societies, resulting in disjunct distribution patterns of maternally and paternally inherited genetic systems (see Chambers 2006). Out of Taiwan remains contested to some degree, but recent developments in genetics seem to have tipped the balance in favour. It is always difficult to distinguish, finally and conclusively, the two competing explanations, but we are well advanced towards this goal.

Conclusions

Readers will note that this chapter has not reviewed every source of information relevant for tracking Austronesian ancestry. For instance, using phylogenetic methods derived from biology, Gray and Jordan (2000) analyzed 77 Austronesian languages across a data base of lexical cognate sets to produce a 'single most-parsimonious tree' that strongly supports the Out of Taiwan model espoused here. This data base has more recently been expanded to 400 languages to reinforce the above conclusions by Gray et al. (2009), In addition, recent publications on comparative cranial morphology also support the model in a fairly generalized way, at least at the level of an expansion of mainland Asian populations into Island Southeast Asia and Oceania during the Holocene (Pietrusewsky and Chang 2003; Matsumura and Hudson 2005). This perspective can be combined with a different and more localized ancestry for the indigenous populations of Australia and New Guinea, both of whom have presumed in situ Pleistocene antecedents (see Cox 2008 for Melanesian genetics). Recent research on the genetics of the human bacterial parasite *Helicobacter pylori* also provides strong support to the Out of Taiwan model for Austronesian dispersal espoused here (Moodley et al. 2009).

This chapter has focused archaeologically on Taiwan and the Philippines from 2500 BC onwards, specifically upon a migration, by both land and sea, of speakers of Austronesian languages and of carriers of a variety of 'Neolithic' material culture. It assumes that both of these migrations are likely to have been two sides of one 'event', involving a single ethnolinguistic and genetic population in the final resort, albeit one that was constantly adapting and interacting, but not 'creolizing' itself out of existence. Our views thus differ considerably from those of Donohue and Denham (2010), who claim that Austronesian languages spread by language shift, akin to the modern spread of Tok Pisin as a lingua franca in Papua New Guinea, with limited movement of people. Thus, as stated in the introduction, it is our view that languages, material culture and genes can be transmitted through time and space with relatively high degrees of correlation, *especially during periods of population migration*.

Coverage has also spread further, if thinly owing to space considerations, to accommodate subsequent events within a 9,000 km west to east expanse of Southeast Asia and Oceania (roughly Sumatra to Samoa). The dispersal spread eventually from warm temperate into equatorial latitudes in the northern hemisphere, and out again into temperate latitudes in the southern hemisphere (New Zealand), thus through some major zones of environmental and resource difference, as well as through pre-existing populations with their own long-established cultures and languages in Island Southeast Asia and western Oceania.

Both the archaeology and the linguistics are powerful witnesses that can directly pinpoint the setting and timing for Austronesian migration, whereas the genetics (including that of domestic and commensal animals) offers a more diffuse yet also constraining perspective. Genetics still has to come to terms with the large error ranges inherent in molecular clock dating, as does linguistics with glottochronology, and archaeology with the often fragmentary and tangential nature of its data set with regard to human population migration. The reconstruction of history presented here could never have been put together from one discipline alone, which adds to the feeling of excitement that progress in understanding the human past on a very broad canvas is available to those who are willing to cooperate between historical disciplines.

Acknowledgements The archaeological research in the Batanes Islands described in this paper has been funded by the National Geographic Society and the Australian Research Council and the Chiang Ching-kuo Foundation (Taipei). Bellwood and Hung wish to thank the National Museum of the Philippines and the Archaeological Studies Program at the University of the Philippines, Diliman, for their support. Chambers notes with gratitude that experimental work in his laboratory at Victoria University of Wellington has been supported by a wide variety of New Zealand agencies over many years; see original references for details of sources.

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Chapter 17
What Role for Language Prehistory in Redefining Archaeological "Culture"? A Case Study on New Horizons in the Andes

David Beresford-Jones and Paul Heggarty

Introduction

Since setting aside its youthful indiscretions of culture history, archaeology has cast about more or less continuously for more nuanced definitions of "culture", and mechanisms by which it is transmitted across space and through time. At times, the discipline has seemed almost resigned in the face of the great intrinsic difficulties in extracting the prehistories of *people* from the material culture record. Yet, we are not as helpless as either the puritans of New Archaeology, or the postmodern reactionaries they provoked, would pretend.

For of course archaeology is not the *only* discipline that seeks to understand human prehistory. Genetics and comparative/historical linguistics offer, through their independent data and methods, their own partial windows on the past, and in recent years the so-called 'new synthesis' has sought to converge these various perspectives into a single, coherent, holistic picture of human prehistory. Progress has been fitful, however. All too often these attempts are plagued by the misunderstandings that attend any cross-disciplinary enterprise. Archaeology seems willing to plunder other disciplines almost by instinct. Sadly, like most looting, this typically turns into a blundering affair that seeks nuggets of "value" and is careless of context or methodology. Examples abound of linguistic neophytes hitching some waggon-load of vaguely understood language baggage to their archaeological hobby-horses, then given free rein to gallop across the empty plains of speculation. Nor have linguists been immune from the temptations of deeply dubious "cultural reconstruction" on the flawed assumptions of "linguistic palaeontology".

Simplistic assumptions of the past that "culture equals language (equals genes)" have rightly been cast aside. Yet too many archaeologists, once burnt by this simplistic trap, now show themselves twice shy of *any* attempt to link to historical linguistics. And just as the initial flaw was simplistic, so too is the overreaction.

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B.W. Roberts and M. Vander Linden (eds.), *Investigating Archaeological Cultures: Material Culture, Variability, and Transmission*, DOI 10.1007/978-1-4419-6970-5_17,

Just throwing up our hands that it simply cannot be done and can be nothing more than vacuous speculation will not do. For great language expansions are not simply inconvenient, unproven hypotheses that archaeologists are free to pretend do not exist and are safe to ignore as "nothing to do with our discipline". They are incontestable facts about the human past. Not to account for them is not an option; it is nothing less than an abnegation of our duty as prehistorians.

In this chapter we wish to set out some fresh methodological principles for how one might after all go about linking findings from archaeology and linguistics, so that together they might better inform our understanding of prehistory. We shall illustrate how these principles can be applied by means of a case–study set in a part of the world which, despite its significance as one of humanity's rare independent hearths of agriculture and cradles of "pristine" civilization development, has been conspicuous by its absence from attempts at cross-disciplinary synthesis so far: the Central Andes. Indeed, the story of archaeology here makes for a useful vignette of the vicissitudes of archaeological theory more widely.

The great pioneers who led Andean archaeology beyond the frontiers of Inca mytho-history – Middendorf, Uhle, Tello and Rowe – could confidently distinguish "Horizons" in the vast archaeological record they surveyed: periods for which that record showed some degree of unity or interaction across great expanses of the Central Andes. Each of three successive Horizons originated high in the Andes, in urban centres far inland (Fig. 17.1). Best known is the Late Horizon, alias the Inca

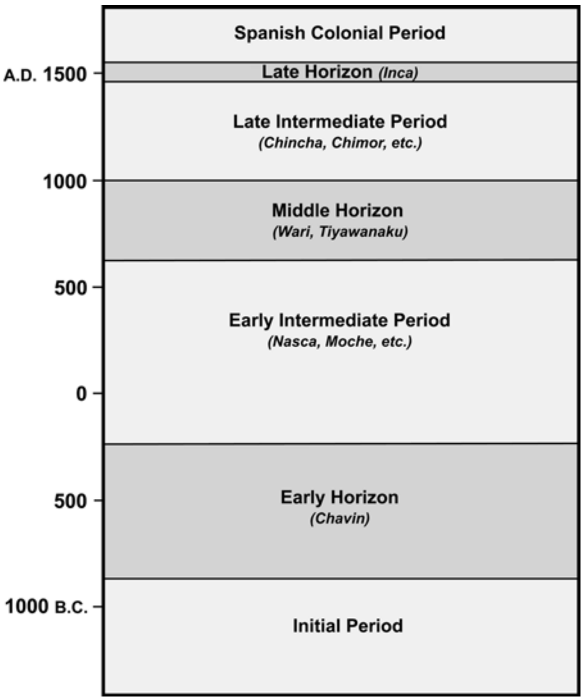

Fig. 17.1 Simplified archaeological chronology for the Central Andes

Empire, stretching from Ecuador to Argentina and Chile, with its capital in Cuzco in the southern highlands of Peru. The Middle Horizon, meanwhile, was a bipolar affair: a Wari "Empire", with its heartland in Ayacucho, south-central Peru; and a more vaguely perceived sphere of influence of Tiyawanaku[1], by the shores of Lake Titicaca in the Bolivian "Altiplano" (high-altitude plain). The Early Horizon was centred on the monumental site of Chavín de Huantar in Ancash, north-central Peru. Between the Horizons were the so-called "Intermediate" periods, for which the archaeological record is more fragmented.

Often it proved possible to impute these Horizons to expansionist conquest empires, by uncritical analogy with the Inca Empire. Inevitably, this "poetic licence" bred a puritan reaction. Culture history, with its predilection for mapping "diffusion" and "migration" on grand geographical scales, fell from grace, to be succeeded by a "New Archaeology" with its emphasis on processes of culture evolution, within a wider movement of processual archaeology. Rather than ripples of cultural influence, this saw the past in terms of long autochthonous cultural trajectories within tightly circumscribed geographical regions (see Isbell and Silverman 2008, 500).

This was not an approach that sat easily with the concept of "Horizons" (Rice 1993, 362). Soon, doubts were raised as to the extents and natures of the putative Horizons: the "Problems" of the Middle and Early Horizons, respectively (Schreiber 1992, 71; Willey 1951, 103). Eventually, those doubts extended to the very utility of the Horizon idea itself, such that Boone (1993, vii) could write that "in the end, the judgement is that the horizon concept is too broad and simple for the scholar, but that it is useful for the student".

Yet, like the concept of "culture" more widely, that of the Horizon stubbornly persists in Andean archaeology. Burger (1993, 41), for instance, acknowledges that "the concept of horizon style has fallen out of fashion in North American archaeology"; but he can nonetheless go on to conclude his succinct review of the evidence by insisting that "the Chavin horizon is not a stylistic chimera as some have contended, but a real pattern" (Burger 1993, 74).

Great Language Expansions Do Not "Just Happen"

We shall argue here that the Horizon concept does indeed remain of great value in Andean archaeology, not least in how it underlies a new proposal to link archaeological and linguistic patterns in the region. This entails a radical revision of the traditional view of the linguistic prehistory of the Andes, which in turn informs the archaeological debate about the nature of the Andean Horizons. Indeed, we hope to make plain that while the oversimplifications of culture history are indeed

[1] We eschew here the popular "pseudo-indigenous" spelling *Tiwanaku* as doubtless erroneous, omitting the second syllable *-ya-* suggested both by etymology and by the original Hispanicized version *Tiahuanaco*.

bathwater to be discarded, we should take care not to throw out the baby with it: the perspective that culture history provided on the wider geographical scales necessary for comparing the data of archaeology with those of linguistics.

Our new proposal for Andean prehistory is based firstly on a major reclassification, long overdue, of the relationships between the various languages and "dialects" within the main indigenous language family of the region, Quechua; and secondly, on establishing a far more satisfactory correlation with the archaeological record. We have here the space to present only its bare bones; for a full elaboration see Beresford-Jones and Heggarty (forthcoming a & b). Nonetheless, this skeleton still serves to illustrate some fundamental methodological guidelines by which archaeological and linguistic visions of prehistory might be linked in a principled and more sophisticated way than has so often been the case.

Fifty years on from the supposed demise of culture history, as hailed by the "New Archaeology", these first principles must start by setting aside any simplistic assumption that "culture equals language (equals genes)". Here, we seek to link archaeology and linguistics not through "cultures", nor even populations, but through driving *forces*. As we shall shortly elaborate, our founding principle is the linguistic "fact of life" that language expansions do not "just happen"; rather, they happen only for those very same reasons of real human demography and sociocultural context that archaeology seeks to describe through its own, independent data: the material culture record.

It follows, too, that *great* language dispersals must have been driven by real-world processes *of* commensurate scale: the great expanse of the *Roman*ce language family today, from the Black Sea to the Atlantic, is none other than the direct linguistic reflex of the strength and lasting impact of Rome. Similarly in the Andes, the dispersal of the Quechua and Aymara families cannot have happened in a social and demographic vacuum. This principle is one we can make use of to identify correspondences between archaeological and linguistic patterns on three levels: *chronology*, *geography*, and above all *causation* – or in other words, *when*, *where* and *why*?

We begin by clarifying briefly a number of principles from historical linguistics that are indispensable to an understanding of how language data can inform us of prehistory at all. Or in other words: for the purpose of archaeology, what does historical linguistics actually *say*? (for a fuller treatment of these principles than is possible here, see Heggarty 2007, 2008; Heggarty and Beresford-Jones 2010).

What Does Historical Linguistics Actually Say?

Among scholars outside the discipline, the single most common misconception about historical linguistics is that it looks for correspondences between different languages simply in order to demonstrate thereby that those languages have a common origin. Or in other words, to imagine that language correspondences necessarily indicate *relatedness*.

In fact, there are *two* processes that give rise to patterns of correspondences between languages; and not only are these processes separate, they are but all the reverse of each other.

- The first does indeed start out from a single original source language, which over time *di*verges into different "daughter" languages. Given that all languages inevitably change through time, once the same original language is implanted in two or more different regions, and *contacts are lost* (or at least reduced) between the populations in each, their speech begins to change in different ways from region to region. That is, they gradually *lose* some of their original correspondences, though still retaining others.
- The second process, conversely, *begins* with multiple different source languages, which *con*verge over time when their originally different speaker populations *come into contact* and interact with each other. These languages thereby *acquire* some correspondences which they did not originally have.

It follows that just because one can identify correspondences between two languages on its own means nothing. Everything depends on which particular *type* of correspondence one finds. The business of comparative–historical linguistics is to compare languages to identify which type of correspondence they show (if any), and from that information to go on to work out the histories of those languages as either *di*vergence or *con*vergence.

This distinction matters for other disciplines because the two processes reflect very different real-world (pre)histories of the human populations. Languages do not determine the external contexts in which their speaker populations live; on the contrary, languages, particularly the patterns of divergence and convergence between them, are moulded by and reflect those contexts. While it may be somewhat contrary to popular perception, it is a founding axiom of linguistics that all natural languages are, to all intents and purposes, effectively equal in their communicative utility (for clarification, see Heggarty 2007, 338, endnote 6). Whether certain languages "succeed" over time, and spread and diverge into families at the expense of others that become marginalized and extinct, is nothing to do with any intrinsic linguistic qualities of their vocabularies, grammars or sound systems. For speakers of any language to imagine the contrary is only to delude themselves as to the relationship between language and "culture". Any of a panoply of Quechua derivational suffixes soon makes a mockery of attempts to count a language's "wealth" by how many "words" it can boast. Quechua borrows Spanish words, just as Spanish borrows English ones, for obvious real-world reasons that have nothing to do with the languages themselves.

Rather, language expansions are entirely a function of demographic, social, cultural and political forces, created by and acting upon the communities that speak those languages. The relationship here is one of cause-and-effect: real-world forces leaving linguistic effects. Among these forces are: the size, density and growth of a population; the degree and nature of its contact with, or isolation from, other populations; and its relative socio-cultural or political power and/or prestige. Language patterns are thus a reflection – and a valuable surviving record, a linguistic "history" – of how such forces operated on given populations over time. It is precisely these same forces that archaeology seeks to track and explain through its own record of the past.

And just as that archaeological record is but partial and fragmented, so too is the linguistic one: many language lineages – Pictish, Etruscan, in fact countless indigenous languages across most of the world – have gone extinct without leaving any significant traces. In seeking to correlate these two fragmentary records, we can hope to use the strengths of each to mitigate the weaknesses of each. For an example of how to go about this, we can return to the two processes by which different languages can come to show correspondences, to set each in its corresponding real-world context. Language *convergence* reflects more or less intense *contacts* between what were originally separate population groups. By contrast, *divergence* of a single ancestor language into a language family reflects a past *expansion* of what had once been just a single population group. Our case study of the Central Andes serves well to illustrate these two different mechanisms.

Two major indigenous language families survive in the region, as mapped in Fig. 17.2. The largest, Quechua, is by number of speakers our greatest surviving link to the speech of the New World before the European conquest. Today, the various languages and "dialects" within the family can still be heard over a patchwork of territories extending almost 4,000 km from southern Colombia to northwest Argentina, equivalent to the distance between Morocco and Moscow. The second language family of the Andes, Aymara, today dominates the vast *Altiplano*, the high-altitude plains of Bolivia, though another quite different variety is still spoken in a tiny pocket some 800 km to the north, in the mountains of central Peru.

Correspondences both within and between these Andean language families make for a rich mine of information about their prehistories. Those of the first type attest to processes of divergence. The respective linguistic records of Quechua and Aymara divergence unfailingly tell us that each family goes back to its own separate single ancestor language, each spoken (necessarily) only in some narrowly circumscribed geographical area. From these respective homelands – wherever they were – each began to *expand*, such that in due course the two came to occupy their known ranges over vast areas of the Andes. The stage of a language lineage at the point in time just before it first diverged is known as that family's *proto-language*; stages long before divergence as the *pre-proto-language*.

There is no language divergence without geographical expansion. But the language families of the world vary greatly in both the degrees of divergence within them, and the geographical extents across which they are spoken. Furthermore, since language change and divergence tend to increase cumulatively with the passage of time, the degree of divergence across a family gives some indication of the time-depth of the geographical expansion that gave rise to it. Indeed, methods have even been proposed, including the so-called *glottochronology*, which try to derive from this approximate correlation a means of actually pinning hard dates on divergence time-depths. We discuss elsewhere the severe limitations on these methods (Heggarty 2007, 321–325; Heggarty and Beresford-Jones 2010, 165).

What matters for our purpose here is that while measures of intra-family diversity certainly do not produce "dates" that can in any sense be regarded as absolute, comparisons between different language families do at least provide a useful guide

Fig. 17.2 The two major language families of the Andes: present-day distribution

to their *relative* time-depths. And such order-of-magnitude measures indicate that Quechua and Aymara, despite their wide geographical extents, are *not* particularly deep families in time. Each encompasses a degree of internal diversity that is distinctly limited by the standards of the six to nine millennia variously estimated for the Indo-European language family. Estimates for Quechua range from just 1,200 to c. 2,500 years of divergence, comparable with just the very "last generation" of Indo-European, such as Slavic, Romance or Germanic (Heggarty and Beresford-Jones 2010, Figure 1). For a start, then, such a time-depth is more than enough to dismiss the popular myth that attributes all Quechua's diversity and expansion to the Inca Empire of c. AD 1450–1535. For what drove most of Quechua's expansion, we must look deeper into the archaeological record.

As regards the Aymara family, attempts to assess its internal diversity and time-depth are hampered by how little of its former diversity now survives outside its modern *Altiplano* heartland. Within that heartland, Aymara exhibits such limited variation that linguists can be all but certain that its expansion there is of relatively recent date. Again, long-established linguistic consensus serves to contradict widespread belief within archaeology, wrapped up in Bolivian national ethos, that Aymara "must have been" the language of the Tiyawanaku polity of the Middle Horizon. Measures of diversity that include the other surviving branch of the family, the Central Aymara (alias "Jaqaru-Kawki") of the Central highlands of Peru suggest a time-depth of a similar order to that of Quechua. Moreover, place name studies and early Spanish colonial reports attest that Aymara was once spoken widely across many other regions, in forms now lost to us (Figs. 17.3 and 17.4), and suggest that its expansion across the region *predates* that of its now larger partner in Andean linguistic domination.

Indeed, on first impressions "partnership" seems an appropriate term for the relationship between speakers of these two language families, for they do show some striking structural parallels, and share a great deal of vocabulary (estimated at as much as 30% for certain of their dialects in closest contact with each other: see Cerrón-Palomino 2000, 311). In the heyday of earlier, now long discredited approaches in linguistics (glottochronology and "multilateral comparison"), these inter-family correspondences led some to propose that Quechua and Aymara's proto-languages in turn go back ultimately to a single common ancestor, i.e. that they are related, and that the correspondences between them are survivals from divergence at some great remove (Büttner 1983; Greenberg 1987). This so-called "Quechumaran" hypothesis is still occasionally entertained among Americanist linguists who do not specialize in the languages of the Andes, but among those who do, not one signs up to it (Torero 2002, 154; Adelaar and Muysken 2004, 35; Heggarty 2005, 2008; Heggarty and Beresford-Jones 2010, 170). Certainly for all practical purposes here, Quechua and Aymara can safely be taken as *not* related.

Rather, the remarkable correspondences between them are of the opposite type: those that attest to that other, all but diametrically opposed process: *convergence*. Countless languages across the Andes and Amazonia, in fact, have gradually come to share in certain general, abstract, structural characteristics, though without any one language recognizable as the source (see for instance the typological criteria in Torero 2002, 539). Such "areal" features are of precisely the type that typically denote chains of localized interactions – over prolonged time-scales and across extensive territories – between small-scale groups speaking a mosaic of different languages. They absolutely do not denote expansions of single languages into broad families, which would leave quite the opposite linguistic signal. That is, correspondences of this type reliably denote only areal proximity and contacts, *not relatedness* (Torero 2002, 154; Adelaar and Muysken 2004, 34–36; Heggarty 2006, 185–188).

Fig. 17.3 Current and assumed earlier distributions of Aymara, by nature and strength of evidence

Between Quechua and Aymara in particular, however, such correspondences are exceptionally strong. Far beyond simply sharing in general areal characteristics, they extend to a host of identifiable borrowings and specific structural "calques" throughout both families, which denote an especially intense interaction and convergence between early stages of their lineages. But even with this type of linguistic effect we remain firmly in the realm of *interactions* that bring about a degree of *con*vergence between originally *un*related languages. Interpretations of which historical scenarios might account for this vary (Heggarty and Beresford-Jones forthcoming, Muysken forthcoming, Urton forthcoming), but certainly this intimacy between the two language families dictates that any satisfactory explanation of the history of the one must be coherent with the history of the other, and

Fig. 17.4 Chavín (Ocucaje 3/4) ceramic (c. 750 BC) excavated by the author (DBJ) in Ullujaya, Ica – the periphery of the Early Horizon. Ullujaya is one of many place names in southern Peru that lends itself to a convincing Aymara etymology, with the meaning "look out" or "vantage point" (R. Cerrón-Palomino, personal communication)

incorporate intense contact between them (see for instance Cerrón-Palomino 2000, 337; Cerrón-Palomino 2001, 140). Ignoring this linguistic fact is the one failing common to all previous attempts by Andean archaeologists to synthesize language prehistory into their interpretations.

Put most simply, then, what historical linguistics says is not that there *may* have been one or more language expansions at given times over given extents of territory – but only provided we can find signals in the archaeological record that we feel are so overwhelmingly strong and match so perfectly as to account for them. Imperfect archaeological evidence is not in a position to deny these language expansions: they are facts.

In our Andean case, for instance, linguistics establishes that at some stages during a time-frame from the Middle Horizon back perhaps as far as the end of the Early Horizon, out of some points within the Central Andes two language dispersals spread across wide and overlapping territorial extents: Aymara and Quechua (most likely in that order). Their expansions were spectacular, and their driving force(s) very real.

It is not a question, then, of *whether any* expansive forces *might* have existed and have left such perfectly clear and matching signals in the archaeological record as to satisfy even the most sophisticated sceptic so that we might "dare" entertain any language-archaeology association. Rather, the burden of proof lies far more heavily on the overcautious sceptic to explain the irrefutable language dispersal while denying any real-world expansive forces to drive it. Of course, material culture (at least in preliterate societies) cannot of itself identify for us who spoke which language and when. But that is beside the point. Let us stress once more: the task is not to work out *whether* some expansive forces in human demography and society propelled particular language expansions, but only *which* of those we can discern in the archaeological record are able to account for the established linguistic facts *most plausibly*. And thus, methodologically, of how and on what levels that plausibility might be judged.

Linking Archaeology and Linguistics in the Andes: The Traditional Model

What, then, is the traditional model for associating the linguistic and archaeological records in the Andes, and how does it fare when we apply our proposed methodology for linking the two? We shall review it briefly and draw attention to certain infelicities in the associations it proposes.

There is some consensus that the homelands of the respective separate ancestor languages of the Quechua and Aymara families lay somewhere in Central Peru, although considerable uncertainty remains as to more precisely where within this rather broadly defined region. Proposals from the two key authorities in the field – Alfredo Torero, generally seconded by Rodolfo Cerrón-Palomino – have acquired at least the status of the most convincing expounded so far.

For Aymara, both authors argue for a homeland in the Nazca region on the south-central coast, from where it is imagined to have expanded during the Early Intermediate Period into its highland hinterlands, including the Ayacucho region (Fig. 17.3). Some time later, that region was to become the heartland of the Wari Middle Horizon, to which they attribute the spread of Aymara more widely across southern Peru, including to the Cuzco region. Finally, during the Late Intermediate, Aymara expanded further south into the Altiplano, where it survives most strongly today, perhaps by the so-called "Aymara Kingdoms" of the Lupaqa and Qulla (although some of these, like their predecessor Tiyawanaku, may not in fact have spoken Aymara at all, but Puquina).

The two main authorities differ, however, as to the likely location of the Quechua homeland. Torero places it on the central coast, immediately to the north of Aymara, while Cerrón-Palomino prefers to set it inland, in the central highlands. Torero (2002, 42) even entertains the suggestion that much earlier, during the Late Preceramic period, i.e. long before its expansion, the pre-proto stage of the Quechua lineage was spoken in the Norte Chico area (Fig. 17.5a), but this is generally regarded as extremely speculative (see Cerrón-Palomino 2003, 22, and Heggarty and Beresford-Jones 2010, 179).

In the traditional view, the details of the earliest Quechua expansions remain rather unclear. What the linguistic data do show unequivocally is that certain of its secondary expansions (to Bolivia, Argentina and arguably Ecuador too) date to the relatively recent past – the Inca Late Horizon and Spanish Colonial periods (Heggarty 2007, 2008). Once these are "peeled back", the picture left, as in Fig. 17.5a, shows that long before them, Quechua had *already* come to be spoken across a great swathe of Peru: the central and southern coasts, and from the north-central through to the southern

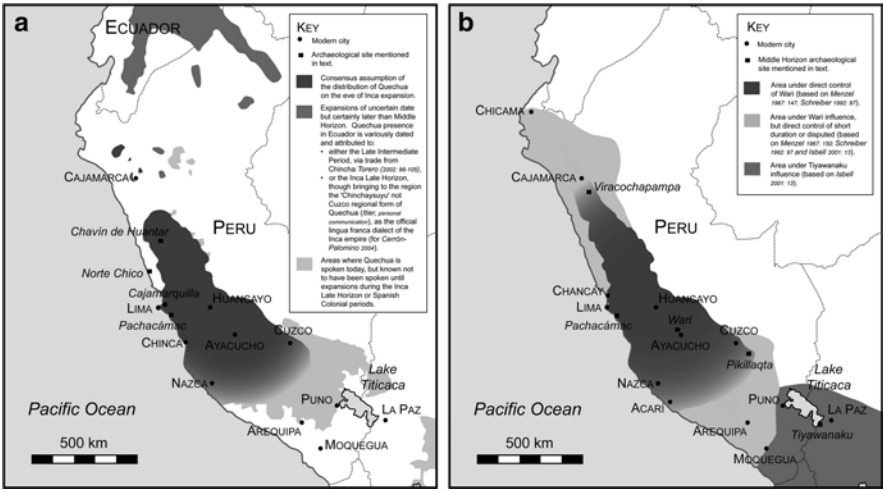

Fig. 17.5 (**a**) Assumed expansions of Quechua prior to and since the rise of the Incas. (**b**) Extents of Wari or Tiyawanaku direct control or influence during the Middle Horizon

highlands, from Ancash to Cuzco. It is also clear that the greatest degree of divergence within Quechua is between the northernmost and southernmost extremes of this broad territory of its earlier expansion. The traditional model would have this divergence explained by a series of vaguely defined migratory expansion stages, not least a first radical split into two separate branches: alias QI and QII, in the terminology proposed by Torero (1964), who envisages these putative expansion stages as stretching from the Early through to the Late Intermediate.

Whichever homeland they started out from, the first stage is seen as an expansion which left Quechua spoken on the central coast and through the north-central highlands of Peru, from Ancash south to Huánuco (but in the traditional view, not yet as far south as the Ayacucho region, which is associated at the time with Aymara instead). This expansion is taken to have given rise to the Central Quechua or QI branch. The remaining QII or North–South Quechua branch, meanwhile, is taken to have formed out of a later expansion to the south coasts and highlands of Peru.

What is unclear in the traditional model, however, is exactly what particular demographic and/or cultural driving *forces* in the archaeological record might account for this initial major expansion. Torero (2002, 124) identifies no particular driver for the first stage at all, though he imagines the split into central (QI) and north–south (QII) subgroups to have begun with an expansion of QII-speakers southwards, culminating in the foundation of the city of Cajamarquilla, at the southern extremity of his proposed proto-Quechua homeland in the Lima Valley, around the fourth century AD (Torero 2002, 127). QII is imagined to spread further south first during the Middle Horizon, along the south-central *coast* and its immediate highland hinterland, an expansion attributed to cult and trading influences of the great oracle of Pachacámac in the Lurín Valley near Lima. Next, from here and driven by the Chincha culture that flourished on the south coast during the Late Intermediate, Torero (2002, 127) posits that QII encroached into the *highlands* of the south (overwriting the Aymara imagined to have been spread there earlier by Wari).

This relatively late spread of Quechua into the southern highlands is invoked to explain the linguistic evidence that until quite late in prehistory the Cuzco region, including the Incas themselves, spoke not just (nor even predominantly) Quechua, as popular perception would have it, but Aymara. Strong evidence for this emerges from a number of linguistic and Spanish documentary sources (see Heggarty 2007). Certainly, as Quechua spread southwards, it picked up increasingly heavy influences from Aymara, especially by the stage that it eventually reached into the Cuzco region, at around the time that the Inca state was beginning to form.

How does the traditional model fulfil our criteria for establishing correspondences with archaeology, on the three levels of geography, chronology and causation? Firstly, on the level of chronology, there are at least some linguistic indications that Aymara began its expansion *before* Quechua, including Torero's own lexicostatistical measures of divergence across each family (see Torero 2002, 88 and the discussions in Cerrón-Palomino 2000, 287; Cerrón-Palomino 2003, 333–334). The traditional model does not account for this; indeed Torero effectively discards this inconvenient datum entirely (see Cerrón-Palomino 2003, 331). That said, even its advocates repeatedly identify Aymara as a *sub*strate (i.e. earlier) language to the present-day Quechua in areas, such as Central Peru – ignoring how this contradicts and inverts the relative chronology of their own model. Torero's (2002, 124) chronology of the various migratory expansions behind Quechua's initial and greatest expansion is defined in particularly vague terms in any case.

On the level of geography too, the traditional model is uncomfortable on a number of counts. It posits Ayacucho as the source of Aymara's most significant expansion during the Middle Horizon; but today Ayacucho is the heartland of Quechua, not Aymara. Indeed, of Southern Quechua's regional varieties today, that of Ayacucho is the one that shows the *least* specific influence from Aymara. Furthermore, Torero's model locates the original, pre-expansion homelands for both Quechua and Aymara on the coast. Yet the entire prehistory of the Central Andes seems to present *no* instances of coastal societies expanding to dominate their highland hinterlands over any significant territory. As Julio C. Tello (1923) long since observed, *major* expansions clearly visible in the archaeological record all proceeded the other way around, spreading *out of* the highlands.

But it is on the third level for linking archaeology to linguistics – that of causation – that we find the most serious objections to the traditional model. For it imputes *major* stages of the language family expansions not to the Horizons but to the *smaller-scale* polities of the so-called "Intermediate" Periods: Nazca, Cajamarquilla and Chincha. Torero's model is particularly implausible on driving forces invoked for the main expansions of Quechua – so far as one can understand his often vague and inconsistent presentation.

Torero locates the starting point of Quechua expansion on the Central coast, in the closely spaced valleys between Chancay and Lurín. These were indeed densely populated during the Early Intermediate, with a major urban centre at Cajamarquilla in the Rímac Valley. But the archaeological record for this period shows nothing here that might correspond with a major language *expansion*, particularly in the direction Torero envisages, into the north-central highlands as far as Ancash (Torero 2002, 124). In fact, Torero never offers clear explanation for this expansion of his QI, Central Quechua. He does, however, invoke a period of intense commercial interaction between Cajamarquilla and a number of other, independent regional polities during the sixth and seventh centuries AD (Torero 2002, 48). In this he follows an interpretation of the archaeological record for this period which effectively disputes its characterization as a Middle "Horizon" at all (see Shady 1982, 1989). The widespread dissemination of a material culture style at the time, which led to this "Middle Horizon" being identified in the first place, is for Shady just the result of a *trading* network, and Torero invokes this to account for intense early contact between Quechua and Aymara speakers. But few of the "urban centres" Torero lists show any coherence with the geography he claims for Quechua's expansion at this stage. Moreover, no sooner did the Wari Middle Horizon first extend into the Rímac Valley but Cajamarquilla was abruptly abandoned (Shady 1989; Mogrovejo and Segura 2001).

Torero is clearer in setting out his explanation of the driving forces for the southwards expansion of his QII, viz. Pachacámac and Chincha; though these turn out to be even less compatible with any evidence presented by the archaeological record. Pachacámac was founded early in the first epoch of the Middle Horizon. So similar is its material culture style to that of Wari that it prompted Uhle's first recognition of a Middle Horizon (even if the precise relationship underlying those styles is still debated: Isbell 1988; Schreiber 1992; Kaulicke 2001). During the Middle Horizon, the Pachacámac style became widely distributed along the coast as far south as Nazca, and into its immediate highland hinterland to Huancayo (Menzel 1967, 151). But during the subsequent Late Intermediate Period, the extent of this influence collapsed, back to just the immediate vicinity of the oracle itself. There is no evidence in the way of material culture remains for major population movements into the Wari heartland following its demise, as required by Torero's model. Nor is there any archaeological evidence that the influence of the rich Chincha society of the Late Intermediate Period extended much into its sierra hinterlands – certainly not as far as the Cuzco region, as Torero's model requires to take Quechua there. On the contrary, in the south this was a time of intense, small-scale conflict and tension, and a breakdown of pre-existing networks. We shall argue that what forces the traditional model to perform these serpentine and unhappy pastiches with the archaeological record is a flawed model of divergence relationships within the Quechua language family.

Linking Archaeology and Linguistics in the Andes: A New Proposal

The traditional model, then, betrays multiple flaws on the various levels of chronology, geography and causation. To overcome them, we propose instead a strong, straightforward new model starting out from the logic that it is the *Horizons* in Andean prehistory, not the Intermediate periods, that offer by far the best evidence of significant geographical expansions of people and ideas – and thereby also the best candidates for drivers to account for the major language expansions too. This broad-scale observation stands, regardless of one's position on the many important debates within archaeology as to what were the driving forces behind these Horizons, and their exact extents.

Indeed we should clarify how we use the term "horizon" here, for different scholars have taken it in two different senses, focusing either on the distribution of a material culture style, or on a phase in chronology during which that style appears (see the discussion in Silverman 2004, 11–14). Originally the geography–chronology match was thought to be so consistent that the two were effectively synonymous, and the stylistic horizons could thus be taken also to define fixed blocks of time, valid for all the regions concerned together. Naturally though, when and how stylistic horizons manifest themselves in the material culture record can in practice vary considerably from place to place. Archaeologists working at ever finer time-scales and within more limited geographical areas have become increasingly sensitive to these variations. Much of the radiocarbon evidence on Wari now falls outside Rowe's original chronological specification of the "Middle Horizon". We need to progress to a more sophisticated and flexible definition which accepts that a horizon applies over different time-spans in different areas – though without that undermining the essential unity of the phenomenon. In concepts such as the "Roman Empire", the same subtlety is of course widely understood and implicit. It is in this more sophisticated sense of core geographical and chronological overlaps that we employ the terms "Chavín Early Horizon" and "Wari Middle Horizon".

The Andean archaeological record shows three Horizon epochs, while its linguistic record reflects just two major language dispersals. For the reasons already discussed, however, we can rule out the Late Horizon as too late to account for the main expansions of either Aymara or Quechua. Thus we are left with a strikingly straightforward picture of *two* Horizons and *two* language dispersals. Our proposal overturns the traditional model's vision of Wari as Aymara-speaking and associates it with the dispersal of Quechua instead, leaving the Aymara spread to be accounted for by the Chavín Early Horizon.

We see the simplicity of this proposal as its great strength: it satisfies Occam's injunction by providing the most parsimonious match between the relative strengths and timings of the key socio-cultural and demographic driving forces in Andean prehistory, and their effects in propelling language expansions. We are of course aware that the instinctive reaction of archaeologists familiar with the not inconsiderable debates on Andean chronology may well be that our proposed match is more simplistic than simple. We shall argue, however, that under the methodology for linking archaeology and linguistics put forward here, our proposal in fact stands up far better than the traditional model also in its *detail*, on all three levels of geography, chronology and causation.

Chronology

Firstly, associating Aymara with the Early Horizon is in line with those indicators we do have that suggest that Aymara expanded earlier than Quechua. More significantly, the chronology of our proposal would have Aymara dispersing widely with the Early Horizon, out of its Chavín homeland. Its associated prestige would, in the Ayacucho region, exert on the pre-proto-Quechua language spoken there at the time a powerful "superstrate" influence (whereby speakers of a lower-status language refashion aspects of it on the model of a higher-status one). Yet in due course, Quechua would undergo its own expansion, driven by the Middle Horizon and thus "overwriting" the earlier dispersal of Aymara, to leave it surviving only in small isolated highland pockets across its original range in Central Peru.

We envisage, then, a heavy Aymara superstrate impact on the ancestor language of the entire Quechua family, which then in turn expanded across Aymara's previous extent. This pattern in fact far better explains the particular form and strength of the convergence between the two families than does the traditional model's vaguer proposal, of simply "adjacent" homelands for ancestors of both families on the Peruvian coast. Indeed, their convergence is often explicitly described in terms of one of the two being restructured in the image of the other (e.g. Cerrón-Palomino 2000, 337), though different views are expressed on which "remodelled" which (Muysken, forthcoming).

Geography

On the level of geography, the linguistic data are, as Adelaar and Muysken (2004, 263) put it, "not incompatible with the alternative hypothesis of an original Aymaran homeland further north, in the heart of central Peru itself". Our association of Aymara with the Early Horizon would put that homeland in the central highlands of Ancash, centred on its core site of Chavín de Huantar (Fig. 17.3). Furthermore, Cerrón-Palomino (2000, 378) reports "significant" Aymara toponymy across precisely this region of central Peru. As he observes, these place names and other linguistic evidence provide "indirect evidence of the presence of a prior Aru [Aymara] substrate [in Ancash]" (Cerrón-Palomino 2003, 333, personal translation). His mapping of Aymara toponymy extends even further north in Peru, though here characterized only as "tenuous" ("tenue"), over an area that would correspond well with the known extent of the Cupisnique material culture style, forerunner to the Chavín Early Horizon.

Outside its Ancash heartland, the frontiers of the Early Horizon (so far as they existed) remain to be precisely determined. On the coast, distinctively "Chavinoid" material culture certainly extends as far south as the Ica and Nazca river drainages. Witness for instance the classically Chavín "fanged feline" iconography in Fig. 17.4, excavated as far south as Ullujaya (Ica). In the highlands, meanwhile, a major Chavín site is currently being excavated near Vilcashuamán, 60 km south-east of Ayacucho (R.L. Burger, personal communication). This southern limit of the Early Horizon is thus entirely consistent with an Aymara superstrate "remodelling" the language ancestral to the Quechua family, as per section

"Chronology" above. It also takes Aymara far enough south to be at least "within range" of what would become its southernmost regions, Cuzco and eventually – over a millennium later – the Altiplano.

Strikingly, on the level of toponymy, several of the place names just mentioned lend themselves to convincing Aymara etymologies, not least Ica, Ullujaya, Vilcashuamán and Cuzco itself (Cerrón-Palomino 2008; R. Cerrón-Palomino, personal communication). Indeed, Cerrón-Palomino's (2003, 292–293) inspection of toponymic dictionaries "indicates the presence of such elements diagnostic of Aymara in the departments of Lima, Junín, Pasco, Huánuco, Ancash, and to a lesser degree, in La Libertad, Piura and Cajamarca" – respectively, we note, the core and periphery of the Early Horizon.

For the Middle Horizon, the geographical correlation between archaeology and the historical linguistics of Quechua is even stronger. As we shall shortly see, there are several starkly contrasting interpretations of the archaeological record for the Middle Horizon. But for our purposes here, at least a partial consensus has been established: "most researchers agree that Wari was an expansive state, an empire that consolidated power rapidly", as Cook (2004, 146) puts it.

The Wari heartland lay in the Ayacucho highlands of the south-central Andes, centred on the eponymous urban centre there. The consensus would have Wari expanding rapidly out of this region during its so-called Epoch 1B, to control directly the central and southern coast between Chancay and Acarí, and the Peruvian highlands over an even greater extent, from Ancash to Sicuani (Fig. 17.5b; Menzel 1967, 147). At its apogee around 800 AD, its capital at Wari was vast, covering some 15 km^2. It remains to this day the largest archaeological site in South America (Isbell et al. 1991, 24).

We have not the space here to review in any detail the evidence or associated controversy behind this model of a Wari Empire. Suffice it to say that it includes the identification of permanent Wari administrative architecture (Fig. 17.6); distributions of mobile Wari material culture; evidence that much of the Inca road network was rehabilitated from an earlier Wari system; Middle Horizon antecedents of the Incas' *khipu* knotted-string accounting device; and even fragmentary hints in ethnohistory (Lumbreras 1974; Isbell 1987; Schreiber 1992, 2001; McEwan 1991, forthcoming; Hiltunen and McEwan 2004; D'Altroy and Schreiber 2004; Urton, forthcoming). Such evidence is deployed to support a model of "direct control" by Wari of the vast area of central and southern Peru defined above. Further north, beyond Ancash and into La Libertad and the Cajamarca basin, evidence for that direct control becomes fragmentary. There are scattered sites here that offer tantalizing hints of Wari administrative architecture, but they are still to be fully investigated (Schreiber 1992, 96; Watanabe 2001). On the densely populated north and north-central coasts, meanwhile, with their own long, independent prehistoric trajectories, evidence for Wari's presence is still more ambiguous – limited to mobile material culture and mortuary remains, leading archaeologists to infer only "indirect control" or influence (D'Altroy and Schreiber 2004).

South of the Cuzco region, Wari confronted the other major pole of the Middle Horizon: Tiyawanaku in the Titicaca basin. The archaeological story

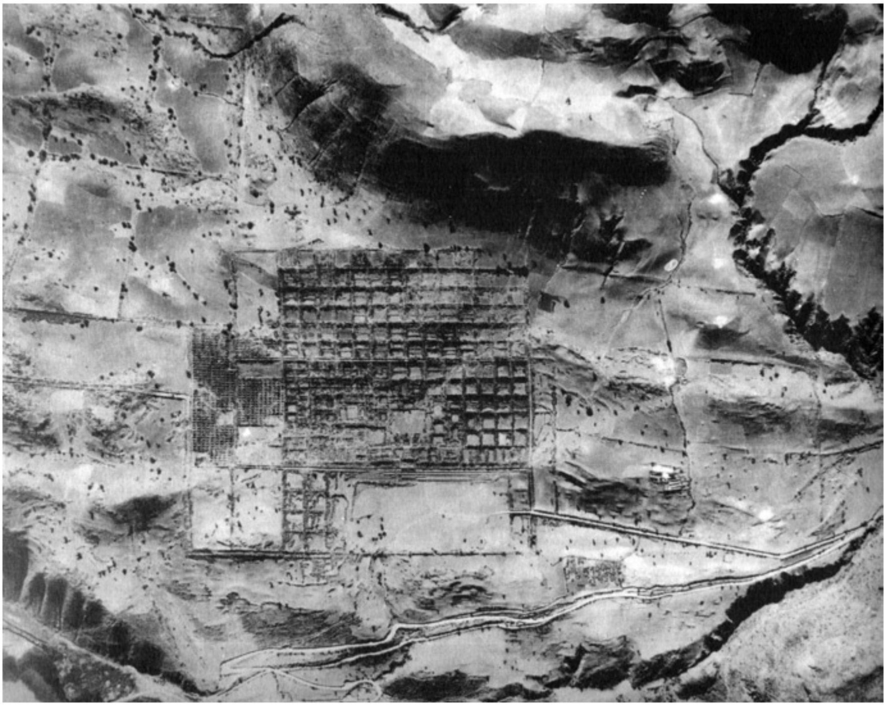

Fig. 17.6 Aerial view of Pikillaqta, a Wari outpost south-east of Cuzco: "such incredibly regimented planning…is not otherwise known in the world history of human environments" (Conklin 1991: 287). Courtesy of the Servicio Aerofotográfico Nacional, Peru

behind the Wari–Tiyawanaku relationship is another subject of considerable complexity. So similar is their iconography that both were initially conflated under the label "Tiahuanacoid". Clearly both partook of a shared ideological tradition widespread across the south-central Andes and which dates to the Early Intermediate or even before (Isbell and Knobloch 2008), and at times the two appear to have interacted intensely. Yet in all other aspects of their economy and society, they show entirely distinct prehistoric trajectories (Conklin 1991; Schreiber 1992; Isbell and Vranich 2004). Indeed, there is evidence from around Cerro Baúl (Moquegua, S.E. Peru) that their "interactions" at times took the form of open military conflict (Moseley et al. 1991; Isbell 2001; Williams and Nash 2002). Finally, and most importantly for our purpose here, their respective influences extended over mutually exclusive territories (Fig. 17.5b).

How does this geographical extent of the Wari Middle Horizon compare with that of the Quechua language family? We observe a remarkably close correlation between them. As already discussed, linguistics can reliably identify those parts of the Quechua expansion that date to more recent periods: into Ecuador during the Late Intermediate or Late Horizon; into Bolivia and north-western Argentina with the Inca Late Horizon and early colonial period; down the eastern flanks of the Andes into the Amazon during colonial times. It has long been appreciated, meanwhile, that other parts of Peru where today only Spanish is spoken, in particular the south-central coast, were Quechua-speaking at the time of Spanish conquest, and indeed well before that of the Incas too (Cerrón-Palomino 2003, 327–328). Once these later expansions and disappearances are respectively stripped away from and added back to the map of Quechua’s current distribution, we are left with a map bearing a strikingly close resemblance to the extent of the Wari Middle Horizon. Quechua dominates the highlands of Peru from Ancash in the north towards Cuzco in the south. Further north in the highlands it persists – and seems only ever to have been present – in just a few isolated pockets in Cajamarca and highland Lambayeque, within wider regions that previously spoke a patchwork of non-Quechua languages. And while Quechua dominated the south-central coast, north of Lima it never gained much foothold among the densely populated valleys of the north coast.

The only part of the map that shows uncertain correspondence with the Wari Middle Horizon is that area of the southern sierra towards the Titicaca basin that is Quechua-speaking today but lies somewhat beyond the southern limits of where archaeology would define Wari’s influence, and its abutment on the Tiyawanaku sphere (see for instance Williams and Nash 2002; Tung and Owen 2008). Yet here too, linguistics tells a revealing tale, for the region is seen as a “linguistic battleground”, where Quechua’s predominance over Aymara came rather late in prehistory, perhaps only during the Inca Late Horizon and early Spanish control.

Causation

Though the correspondences in chronology and geography may well be striking, they are but circumstantial evidence for our core case. Its real strength lies on the level of causation. We reiterate the axiom of our methodology: language expansions do not “just happen”, they happen only for those very same reasons of real human demography and socio-cultural context that archaeology seeks to perceive through the material culture record. And great language dispersals like those of Quechua and Aymara can only have been driven by processes of commensurate scale.

For our proposal to stand we need not attempt to link the expansions of these language families in the Andes to particular “cultures”, nor indeed infer their association with the spread of a particular “people” and their genes. By our proposed methodology we seek to link them through *driving forces*; and the only ones of commensurate *scale* evident in the archaeological record of the Andes are those associated with the Horizons.

Quechua has long replaced Aymara throughout Peru, save for the southernmost strip bordering Bolivia and the tiny central highland enclaves of Jaqaru-Kawki. All other indigenous languages are extinct from the highlands. Quechua’s early expansion closely matches that of the ancient Wari Empire (even today the most heavily

Quechua-speaking region is that of Ayacucho, the heartland of Wari.) And while archaeologists have proposed very different interpretations to explain the material culture record left by the Middle Horizon, all have in common an agreement that the period was characterized by major social change and upheaval.

The size, complexity and poor state of preservation of the site of Wari itself have defied comprehensive archaeological survey to date (see Isbell 1988, 167). Speculative population estimates for the city range between 50,000 and 100,000 (Benavides 1991, 56), or 20,000 and 34,000 (Isbell 1988, 173). Whatever, it was by any measure an enormous ancient city: even half a millennium later, Venice, the largest city in Europe, had a population of under 50,000 (McEvedy 1992, 75). Survey data indicate how the populations in surrounding areas gradually became drawn into the vast agglomeration that was Wari (Schreiber 1992, 88; Isbell and Vranich 2004, 177).

Furthermore, the Middle Horizon archaeological record indicates much larger scale movements of people across the Andes than do those of the Intermediate Periods that preceded and succeeded it. Mobile material culture indicative of Wari influence is distributed throughout the highlands and along the south-central coast of Central Peru, but for the very far south (Fig. 17.5b; Menzel 1967). Sites in the Cuzco region, meanwhile, and Wari itself, have yielded significant quantities of ceramics from Cajamarca, almost 1,000 km to the north. As Menzel (1967, 152) observes, "their abundance [at Wari itself] is such as to suggest that there were colonies of northerners established at the imperial capital". In non-mobile material culture, for Isbell (1987, 86) "perhaps the dominant feature of Wari architecture" is that of "barracks-like residential facilities", interpreted as housing for mobile labour or military personnel (see also McEwan 1991, 117). D'Altroy and Schreiber (2004, 274) summarize the consensus view thus: "it is now clear that these sites were occupied by large numbers of people, both foreigners from Wari and local peoples". And some of these sites were huge. Pikillaqta, for instance, shown in Fig. 17.6, is but a part of the intense Wari occupation of the Cuzco region (McEwan 1991; Glowacki 2002). Yet this single component of the Wari periphery is larger than the later Inca imperial *capital* of Cuzco (see McEwan 1991, Fig. 17.2).

There is strong evidence also that the Wari Middle Horizon was, in large part, based upon the intensification of food production in the intermontane valleys of the highlands: the "quichua ecozone". The extent of Huari direct control seems intimately associated with this zone – the same area over which Quechua saw its first major expansion, and from which, incidentally, it acquired its very name. (Cerrón-Palomino 2008, 33–49), however, for a path through the minefield of popular myths surrounding the etymologies of the terms *Quechua* and *Aymara*. The evidence includes large-scale shifts in settlement and in subsistence regimes from tubers to maize cereals; massive terracing construction (Schreiber 1992, 2001); and the introduction of new maize varieties (Grobman et al. 1961; Bird et al. 1984). All appear to have been instigated by the "state" in order to supply distant urban populations. Isbell (1988, 182) credits the Wari Middle Horizon with the development of a uniquely Andean form of "state finance" – what Godelier (1977, 188) had first called the "Inca mode of production" – whereby long-standing local traditions of reciprocal exchanges of labour were elaborated into a system of labour-taxation, in exchange for food and drink provided at state-sponsored feasts. Much Wari iconography, particularly that of gigantic ceramic vessels associated with feasting, reflects an apparent preoccupation with agricultural themes (see for instance Shady 1989, 13). Indeed, some see the innovations wrought by Wari in these respects as so fundamental and enduring that they defined the course of subsequent Andean civilization (Isbell 1988, 182), and settlement patterns still today (Schreiber 1992, 260; Williams and Nash 2002, 255).

We argue here that so too did they define language patterns. Indeed, that Quechua was later adopted by the Incas as the language of administration for their empire was not because it was their own original tongue. Rather, there are strong indications that the Incas themselves may well originally have spoken Aymara, and switched their "official" language of empire to Quechua only relatively late in their trajectory of imperial expansion, during the reign of Tupac Inca Yupanqui (1471–1493). Their reason may have been an entirely pragmatic one: that most northern lands that fell to the Incas were already speaking it, precisely because the earlier Wari Empire had so dramatically spread it there. It even appears that the form of Quechua the Incas selected for this purpose was not that ultimately spoken in the Cuzco region, but a more northerly "Chincha" version (for details on all these issues, see Cerrón-Palomino 1998, 1999; Cerrón-Palomino 2003, 342; Cerrón-Palomino 2004).

As for the longevity of the Wari Middle Horizon, Menzel's original relative chronology based only on ceramic typology would have us believe that its rise and fall were swift. As Schreiber (1992, 276) puts it: "each phase of the Middle Horizon 1B, 2A and 2B, is estimated to have lasted only about 50 years…If this accurate, the Wari Empire lasted only about 150 years". Yet in the face of mounting ^{14}C evidence (see for instance Williams 2001), this view has now been abandoned. As Cook (2004, 158) summarises: "instead of a 200-year span (approximately 650–850 AD) during which time the empire flourished, the time frame has doubled (approximately 550–1000 AD)". This relatively recent understanding that the time-depth of the Middle Horizon approached half a millennium is far more compatible with the dramatic linguistic impact that our proposal would attribute to it.

For the Early Horizon, both the archaeological signature and linguistic traces are naturally far fainter. In linguistics, no language expansion has to date been explicitly attributed to the Chavín Early Horizon. It is true that Cerrón-Palomino's reflections on a homeland in the central highlands, not the coast, might at least hint at Chavín as a possible candidate homeland for Quechua (Cerrón-Palomino 2003, 22). His reluctance to formalize such a claim, however, is based on the same objection that most other writers see: the Early Horizon seems just too far back in time to correspond with the expansion of so shallow and compact a family as Quechua. On this last point we could not agree more, but it remains deeply problematic in the traditional thinking that such a defining archaeological signal as the Early Horizon is left without any significant linguistic correlate, when the linguistics is crying out for a driver for Aymara – and before the spread of Quechua.

Elsewhere (Heggarty and Beresford-Jones 2010), we have drawn attention to the correspondences in broad scale and timing between: the first major language family expansion in the Andes; the "real pattern" (Burger 1993, 74) that the archaeological record of the Early Horizon represents; and the first point in time for which we can unequivocally assert that maize has "suddenly" become *ubiquitous* in the archaeobotanical record. We have also argued that it was only at this point in Andean prehistory that a number of gradual processes in the development of food production in the region finally intersected to tip agriculture here across an *expansive* intensification threshold. We hypothesize that it was the significant incorporation at last of a true cereal, maize, that was crucial to this coalescence of a geographically expansive agricultural package – a "mobile food chain", to use Jones' term (Jones 2007, 144).

In the end, however, given its far fainter traces, our association of the Early Horizon with the expansion of Aymara on this level of causation relies also on reductive reasoning. If our proposal to ascribe Quechua to the Middle Horizon stands, then our axiom associating major language family expansions with Horizons leaves us with one major Andean language family expansion to explain, and only the Early Horizon to explain it with. It fits, moreover, with the clear indications that over almost all its core range in Peru, Quechua seems to overlie an *earlier* Aymara spread.

We also explicitly associate both the Early and Middle Horizons with step-changes in food production: two different agricultural intensification thresholds (Pearsall 2008). What we do not advocate, of course, is a simplistic argument that the spread of the major language families of the Andes is to be sought *uniquely* in agriculture (Heggarty and Beresford-Jones 2010). Both Horizons were unquestionably much more than merely intensifications of maize agriculture. We shall return shortly to the debate in archaeology about the nature of each of the Andean Horizons, and what our proposed "new synthesis" of archaeology and linguistics here might mean for those debates.

Not Seeing the Web for the Trees

There have been two partial precedents for our proposal: Isbell (1974) and Bird et al. (1984). Isbell suggested that the expansion of Quechua was associated with that of maize agriculture, though at a time remove too great to be reconciled with linguistic data (Cerrón-Palomino 2003, 336–338). Bird et al., meanwhile, recast Torero's original language data alongside a biogeography of maize varieties – data radically different to our own here, but arriving at a conclusion in part the same: linking the expansion of Quechua to the Middle Horizon. Neither linguists nor archaeologists have engaged meaningfully with this proposal beyond merely citing it, except for Cerrón-Palomino (2001) and Isbell (1984), respectively, both of whom are highly critical. We too disagree strongly with Bird and colleagues' unorthodox vision of the linguistics, not least the disconcerting methodological liberties they take with "linguistic dating" (Bird et al. 1984); nor does their proposal take into account the deep correspondences between Quechua and Aymara that any model needs to explain.

Certainly there were serious flaws in the linguistic argumentations of these previous proposals, then. But why is it that Andean linguists themselves have never yet entertained so straightforward a proposal of Quechua as driven by the Middle Horizon, and Aymara by the Early Horizon? The key obstacles, in our view, have been two: the traditional view of the classificatory structure of each language family as a branching tree; and the assumption that since they attest to deep contacts between each other early in their histories, they must have their origins in territorially neighbouring homelands.

The traditional classification of the relationships between the different regional "dialects" and languages within the Quechua envisages a "family tree", with a series of binary branches: first, the original Proto-Quechua ancestor is imagined to have split into so-called QI and QII branches; QII then split in turn into QIIa vs. QIIb/c; the latter then into QIIb and QIIc and so on. This tree model seeks to explain the various degrees of difference between modern Quechua varieties primarily in terms of greater or lesser time-depths since their respective ancestral lineages separated from each other.

In real world terms, this concept of a language "split" typically corresponds to a stark division of an original population into two groups, thereafter no longer in contact and whose speech thus develops separately into different varieties (Heggarty et al. 2010). So to link his branching-tree view of Quechua prehistory with the archaeological record, Torero is compelled to seek a whole string of separate population splits and expansions by "migration". The result is an over-extended chronology which needs to invoke driving forces from multiple periods through Andean prehistory, and assign major roles to relatively minor regional polities in the Early and Late Intermediate Periods. Moreover, assuming that the Quechua and Aymara homelands must be placed near each other therefore pushes Torero to hypothesize that Aymara's origins were on the south coast of Peru, and requires the Wari Middle Horizon to be a key second-stage driver of its expansion, ruling out a role for it in spreading Quechua instead. There are a great many infelicities in the complexities of these multiple expansion stages, and in attributing major linguistic impacts to minor polities in the archaeological record; but they are forced on the traditional model by its insistence on a branching tree as the classificatory structure of the Quechua, and also Aymara, language families.

True, as an intellectual model, binary branches may seem more "elegant" – for which however read "simple", indeed "simplistic", in linguistic and indeed real-world terms. For binary branches are by no means the only pattern in which languages diverge in practice. On the contrary, for many language families it is well known that no family tree classification is viable at all, and a quite different model is needed: the "dialect continuum". This applies to large swathes of all four major language families of Europe (Romance, Germanic, Slavic and Celtic), as well as to Arabic, Bantu, Turkic, the languages of northern India, China and elsewhere. Great swathes of human linguistic diversity cannot be represented by family trees, but only by dialect continua.

For Quechua too, the initial family-tree classification has fallen increasingly into disarray as our knowledge of the geographical diversity across the family has grown over recent decades, especially with the documentation of dialects intermediate between the supposed two main branches, or that classify well with neither (Adelaar 1977, 1987; Taylor 1984a, b). As early as Landerman (1991), it was demonstrated that the two-way QI–QII split is untenable; but because he still kept faith with the tree idealization in principle, he remained unable to offer any alternative.

To propose and justify one, more radical steps were required. Heggarty (2005) applied new network-type phylogenetic analyses to Torero's own measures of divergence across the family, and to new datasets and quantification methods of his own. All of these consistently favour a view of Quechua not as a branching tree at all, but as a "network" or "web" of cross-cutting linguistic relationships, the signal typical of a dialect continuum. We have called, then, for the traditional family tree classification to be abandoned altogether, in favour of a dialect continuum model instead. It then remained to set this new view of Quechua's origins in a real-world context that might explain how and why the family diverged into such a pattern, if not by Torero's sequence of migrations.

In dialect continua, the respective degrees of difference between language varieties within a family are typically explained not by chronological differences in the stages at which their lineages diverged, but by degrees of coherence across a wide "speech community", determined in large part simply by geographical distance. Romance – and we argue also Wari – provide classic examples. Typically, a single ancestor language – in our cases Proto-Romance (i.e. Latin) and Proto-Quechua – is spread in what is effectively a single, contemporaneous expansion, across a continuous geographical area. If extensive enough, the speech in different sub-regions will naturally diverge, and all the faster after the collapse of "political" unity across it (the fall of Rome, and of Wari). Nonetheless, local-level contacts continue, allowing new linguistic developments to spread by "waves", overlapping across different parts of the overall region, so that the original ancestor language at length turns into a dialect continuum. From one village to the next, minor differences do not disrupt mutual intelligibility, but between the distant poles of the continuum so many differences accumulate that they speak what are effectively different languages, albeit related. A useful analogy is a colour spectrum, where colours contrast starkly at the extremes, even if between them there is never a sharp break in the shading from red to orange to yellow to green, etc. Portuguese is most similar to Spanish, then to Catalan, Provençal and so on the further one travels eastwards. But all these territories were settled by Latin speakers at more or less the *same* time: a single, all but contemporaneous expansion with no "splits".

As what best accounts for the core structure of the Quechua language family, we propose a similar process: an initial major expansion propelled *only* by the Wari Middle Horizon. The increasing differences in Quechua from Ancash in the north to Cuzco in the south go back not to a chronological sequence of separate migrations, but merely to the greater geographical distance between them across this Quechua continuum. The intermediate varieties of Yauyos are in their due place, in the middle of that continuum, while the "unclassifiable" north Peruvian varieties such as

Cajamarca reflect the isolated northernmost Wari outposts, beyond the continuous zone and thus developing more independently of the wave changes spreading across it (the closest Romance equivalent is the isolated "outlier" that is Romanian).

Certainly, a dialect continuum picture can be complicated by later disruptions: political frontiers draw fault-lines within it; intermediate dialects die out or are "standardised" towards others (especially in modern western nations). Indeed, later expansion episodes can emerge from just points within it: late Medieval Spanish to the New World, for instance; or Cuzco Quechua to Bolivia. Nonetheless, for the core of Quechua's divergence history, our vision of a single Horizon expansion in practice makes for a far more economical and straightforward explanation in real-world terms than the supposedly "elegant" binary tree, with its need for a string of successive migrations, first in one direction and then in others. For more on this issue of the real-world (pre)historical correlates of dialect continua vs. branching trees, see Heggarty et al. (2010) and, for the Quechua case, Heggarty and Pearce (forthcoming).

What Does It Mean for Archaeology?

Finally we turn to the archaeological debate about the nature of each of the Andean "Horizons". It is one thing, of course, to identify a "Horizon" (alias "stylistic coherence over a broad region": Rice 1993, 9) and to describe its extent and variation in time and space as culture history wished to (difficult tasks in themselves). It is quite another to tease apart and explain, as archaeology now aspires to do, those cultural *forces* – economic, political, ideological, and so forth – that shaped that archaeological record.

We have so far presented Wari as a military, expansionist empire – the partial consensus within Andean archaeology. We have also alluded to how archaeologists have nonetheless read from the same material culture record stories of the Middle Horizon that can be very different. Various primary driving forces have been invoked to account for that record, which Schreiber (2001, 443) usefully summarises as (1) political expansion/conquest; (2) religion; and (3) commerce.

The idea that the mainspring of the Middle Horizon was a religious movement has a long pedigree in archaeology, dating back to Menzel herself, and is still advocated in several forms, with considerable archaeological evidence marshalled in its support (see for instance Topic and Topic 2001). Certainly there is little doubting the significance of religious ideology in its material culture record. Another alternative to the leading model envisages a number of independent regional polities, but linked by a substantial *trading* network (e.g. Shady 1982, 1989). As we have seen, it is this model that seems to underlie part of Torero's scenario for Quechua expansion, but confusingly also that of Aymara (see also Isbell 1984 for criticism).

For the Early Horizon, meanwhile, sunk in far deeper recesses of Andean prehistory, speculation is lent an even freer rein. Most archaeologists see it as the expression of little more than a proselytizing cult, radiating out from (or in towards) the monumental site of Chavín de Huántar (Burger 1993; Kembel and Rick 2004). Some would even deny its very existence as a "Horizon" (e.g. Pozorski and Pozorski 1987).

Here, our proposed synthesis between archaeological and linguistic data proves instructive in making an explicit claim: that whatever our interpretations of the Early and Middle Horizons, each should include one or more forces capable of driving a *major* language expansion. This leads us in turn to question an archaeological model for either that "places the vital motor of cultural change in ideology rather than in the material realm", as Willey (1999, 86) puts it. For despite popular impressions to the contrary, on closer inspection history offers surprisingly few, if any, precedents for major language expansions driven by religion.

To understand this, one must avoid confusing two utterly different linguistic realities: on the one hand, the territorial expansions of natural, changing, native-tongue language families (such as Quechua, Aymara, Romance or Indic); on the other hand, the use of a particular "fossilised" language as an "élite" medium of religious discourse (such as Church Latin or Classical Sanskrit), often among communities speaking various different native languages. Certainly, across Europe a fossilized form of Latin was once widespread in liturgical and scholarly uses; but even by early mediaeval times it was a learned code, the native tongue of no one, and restricted to contexts which, for all their status, were always sociolinguistically highly marked, marginal, and ultimately largely doomed.

Such language uses are but an artificial sideshow to the vast real-world expansion and lasting survival of the Latin (i.e. *Roman*ce) lineage as living, native tongues. The populations of much of modern Europe speak Romance languages not because of Christianity, but because of the very temporal, *pre*-Christian powers of Rome; so too, now, do those of "Latin" America and much of Africa, because of the later power of imperialist European empires. Christianity may have *accompanied* these later language expansions, but it did not *drive* them. On the contrary, both were driven together by other, much more material forces. Notwithstanding Europeans' appeals to Christianity to "legitimise" their conquests, it was not religion that provided the primary incentive in practice, and much less still conferred the key "germs and steel" advantages that made the conquests possible at all. Similarly in India, the continued use of fossilized Sanskrit for religious and administrative purposes pales alongside the spread of the Indic family of native languages, with almost a billion native speakers across Pakistan, Northern India and Bangladesh, derived instead from the living *Prakrits*.

The net linguistic effects of religions per se, even proselytizing ones, have been very modest. The one oft-cited case of major language dispersal apparently in step with a religious one, that of Arabic and Islam, likewise turns out to be a chimaera which only reinforces this principle. For again, the lasting spread of Arabic as a native language across parts of the Near East and North Africa was driven far less by religion than by one of the most crushing military conquests in history. Elsewhere, wherever the Arabs' military conquest did not reach, and even across Persia where it did, their language failed to make headway except as a medium of religion discourse, and a source of loanwords into the regions' native languages. From Senegal to Sulawesi, Islamic populations continue to speak African, Indo-European and Oriental tongues – not Arabic.

Religious ideology doubtless was an important trapping of both the Chavín Early Horizon and Wari Middle Horizon; it is certainly conspicuous in the archaeological record. Yet for all that, it signally fails as a "vital motor" to drive major language expansion. Perhaps archaeology has been misled by the all-too visible representation of ideology in the Andean material culture record; upending Hawkes' (in)famous "ladder of inference" (Hawkes 1954).

More generally, prior to the transformations brought about by the rise of the nation state and associated phenomena of mass education and literacy, language standardization, transport and (tele-)communications, through most of human history mechanisms of language dispersal have been radically different to those we observe in the modern era.

In this light, and as with religion, a strong case can be made that trade too is altogether too weak a driving force to explain Quechua's expansion. Until these modern transformations (and indeed arguably even since then), the sorts of contacts made through trade and exchange have had surprisingly little linguistic impact. The Phoenicians, for instance, may have dominated trade for over a millennium in the Mediterranean, but they have left precious little linguistic trace (Ostler 2005, 45–46, 68–78). Trade, like religion, has through history had far less linguistic impact than many would like to believe. As a stock explanation to invoke for archaeology–linguistics links, trade too is all too facile, and smacks of instincts from our context of the modern world, rather than those that prevailed through most of human history.

Some authors, meanwhile, have made much of the existence of "bilingual" or "multilingual empires", known from historical times, to challenge the claim that, as Isbell (1984, 246) puts it, "conquest states and empires spread single languages, establishing linguistic uniformity". Empires are – by definition – multilingual at their inception, though for our purposes what matters is that they typically drive the expansion of just one language. The supposed counterexample inevitably invoked is that of the Roman Empire, which did indeed eventually split into a western Latin-speaking half, and an Eastern, predominantly Greek-speaking one. Yet this is to convey a misleading impression of Rome's linguistic impact, for over the course of its great expansion and consolidation Rome really drove just *one* language of empire: Latin, from its humble origins as but one among the patchwork of many languages of Iron Age Italy to what would ultimately turn into the vast Romance language family.

Other than in administration (and ultimately, increasingly there too) the two halves of the Roman Empire were largely independent entities, not least linguistically speaking. The so-called "Jireček Line" across the Balkans marks the sharp divide between the Latin and Greek-speaking halves, visible to this day in the inscriptions and archaeological records on either side. The Roman Empire was thus never a truly bilingual empire operating in, or driving the expansion of, two languages side-by-side. Greek had *already* been spread by earlier seaborne expansions and by Alexander, which is what made it a useful *lingua franca* for the Romans to avail themselves of in the east in the first place. It also assured Greek sufficient status in the face of Latin that the latter failed to spread significantly here at Greek's expense, as it had eclipsed most languages in the West. A final key difference is that while in the West Latin was the everyday language of the populace and a major demographic expansion, this was never the case for either Latin or Greek in most of the East; hence their very different eventual fates.

A synthesis of linguistics and archaeology has considerable implications, then, for how we interpret what the Andean "Horizons" really were. For if Wari spoke Aymara, as proposed by the traditional model, then the modesty of its linguistic legacy across the Central Andes – toponymy and a few scattered pockets of speakers – is *incompatible* with the model of a military expansionist empire establishing direct control over vast territories, in the manner of the Incas or the Romans. On the other hand, if our new proposal is correct and Wari spoke Quechua, then the model of direct control Empire is almost *required* in order to account for its linguistic impact. The fainter surviving trace of Aymara's expansion across the Central Andes is then explained by the greater time-depth (and relatively weaker impact?) of the Early Horizon.

Conclusions

By their very nature, interdisciplinary syntheses run a risk of circularity. One model, weakly substantiated in one discipline, is invoked to bolster its counterpart model in the other; which then, by definition, feeds back to support the first. Such dangers lurk in the forests of phylogenetic trees proposed for human genetic and linguistic lineages – not least the linguistically infamous, simplistic and invalid match for all of humanity proposed by Cavalli-Sforza (1997, Fig. 17.3). Many of the trees, moreover, turn out to be "rooted" in archaeological manure.

The distinction between a coherent picture between the disciplines, and a "just-so" story based on circular argument, can lie ultimately only in judgements passed independently in each discipline on the strength of the data and their interpretations. Here this includes the case for abandoning outdated and inappropriate tree models of how each of the major language families of the Andes diverged, and replacing them with dialect continuum scenarios.

The risks can be mitigated also through the principled new methodology we propose: seeking to correlate the disciplines independently on each of our three levels of chronology, geography and causation. Nonetheless, in setting sail for any new synthesis goal, it is *causation* that stands as mast and sail, with the other levels adding little more than ropes and rigging. Links between our archaeological and linguistic records of the past should first and foremost be cast in terms of those same *forces* that affect human populations, and to which both those records attest; and they must be coherent above all in the *commensurate scale* of their impacts in each.

As culture history fell out of favour and the material record came to be viewed through the processual lens of culture evolution, archaeology lost some of its perspective on those appropriate scales. Unfortunately so, for a principled synthesis of the archaeological and linguistic stories has great potential to enrich our understanding of prehistory. Moreover, for all the elaborations in semantics of more recent archaeological theory, in the case of the Andes the current leading model of the Wari Middle Horizon turns out to be no different in essence to that founded by the culture-historical work of Menzel and Rowe (see for example Rowe 1956; Menzel 1967). The Wari "Empire" fell only for the "state" to be built upon its theoretical ruin. Our own proposal strengthens still further this interpretation of Wari as an expansionist, military conquest empire, akin to the Incas. To the many elements of "Inca" statecraft that archaeology has gradually revealed to have had their roots in the lost empire of the Middle Horizon, we now propose to add one more: the expansion of the greatest surviving language of the New World, Quechua.

Acknowledgements This chapter was made possible thanks to research funding from the Leverhulme Trust for the multidisciplinary *Languages and Origins* project (Heggarty), and a postdoctoral fellowship from the British Academy (Beresford-Jones), both hosted at the McDonald Institute for Archaeological Research, University of Cambridge. For detailed and constructive comments on this manuscript we extend particularly thanks and acknowledgement to Gordon McEwan.

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B.W. Roberts and M. Vander Linden (eds.), *Investigating Archaeological Cultures: Material Culture, Variability, and Transmission*, DOI 10.1007/978-1-4419-6970-5,