

Chapter 13

The Future of Lithic Analysis in Palaeolithic Archaeology: A View from the Old World

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Abstract Archaeology has to rise to the challenge of projecting itself, accommodating new relationships with disciplines such as evolutionary psychology and anthropology, primatology and genetics. This task requires a reorganisation of approach, so that archaeology does not seem to take purely minimalist views, based simply on the current record of preserved finds. Early archaeology in the Old World divides overall into the dynamics of big evolutionary outlines, and scenarios of local detail. Both are equally important in building a record. The first is more subject to major changes of perspective, and the second offers more continuity in its analytical techniques. The chapter explores recent developments in Palaeolithic archaeology as hints of changes to come.

Paleolithic Archaeology – centrally depending on the analysis of stone tools which is our concern – is to all Palaeolithic archaeologists the indispensable way of looking at the past. People who do not partake of it are missing the most essential part of human experience, the shaping of humanity. But more than usually at present, we need to show the World that this is so (or the World may not notice).

Stone Age archaeology – the archaeology of the more distant human past – was shaped as an idea by Christian Thomsen (1836), from the finds of Denmark, and began to find its time depth with the work of Boucher de Perthes 150 years ago (Boucher de Perthes 1864; Gamble and Kruszynski 2009; Gowlett 2009). Since then it has constructed a huge picture and from time to time we need to step back from it, to try and look forward. Glynn Isaac did so in 1971, in a competition organised by *Antiquity* (*Whither archaeology?* Reputedly Isaac who had spelling difficulties headed it “Wither Archaeology”). It is tempting to begin from that paper (Isaac 1971) so as to survey the next generation’s progress across the Old World, but I would rather hold its conclusions for comparison at the end and make a fresh start now. But we

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should note here the catalytic role of Isaac's other ideas (e.g., Isaac 1969, 1972), and the significance of his close association with David Clarke, one of the most influential figures in the New Archaeology. This volume in part marks the 40th anniversary of David Clarke's *Analytical Archaeology* and by chance the time of writing marks also another anniversary – 50 years since the Leakeys recovered the association of stone tools and an early hominid at Olduvai Gorge (Leakey 1959). From their finds, Isaac drove forward the idea that changing timescales had major implications (Isaac 1969, 1972), and along with Clarke he tackled another point that we can make thematic – the importance of rates of change. Stewart (1995: 55) emphasises the broad mathematical principle established by Newton that “laws are formulated as equations that relate not to the physical quantities of primary interest but the rates at which those quantities change with time”.

In Palaeolithic research, we seem to have two main aims in looking at this past. One is to carry out the duty of making a record of each and every bit, as in history. This mapping is akin to wallpapering each bit of space and time, with a record that consists largely of stone tools, but also of their contexts. Preservation permitting, everywhere has the potential for this detail. This approach is similar to that of regional local history – charting the *événements* of Braüdel (Bintliff 1991, 2003). It implies that from the start all events are equal, and no one's actions are more or less important. In archaeology, it can lead to a search for the individual as such (Gamble and Porr 2005).

Our other major approach is geared to mapping out the dynamics of human evolution, to tackling the biggest picture. Many Palaeolithic archaeologists have this aim. They are more interested in the problems of human evolution than the archaeological record itself. In most cases, they tackle a slice of the whole: an inspection of the pages of *Evolutionary Anthropology* or *Current Anthropology* shows that mostly we tend to focus on early human origins or modern human origins, one at a time, but the big frame is the goal. Many of the papers in this book clearly also have this goal, despite their equal focus on detail and methodology.

It is worth adding that just as Braüdel (1972) sketched out a series of historical levels, so had Clarke and Isaac and colleagues in their writings on culture groups and technocomplexes (Clark et al. 1966; Clarke 1968; Isaac 1972). Their scale of entities forms a bridge between the large and small pictures.

The detailed approach is of course at least as valid as the major evolutionary one, and it is going to soak up a huge amount of the day-to-day efforts. Indeed the larger picture must be built up from its blocks. If you are studying the Upper Palaeolithic in Italy or the LSA in Namibia, the questions are different, but not less important: documentation of each relevant aspect – the building of a full palaeoecological and sociotechnical picture – is vital, whether it is at Olduvai or Etiolles.

Big Picture Dynamics

I will concentrate first on this bigger picture, which actually embraces all the human revolutions (Gamble 2007; Gowlett 2009a), except those of agriculture and civilization. What was it, and how has it changed?

A generation ago, the timescale of archaeology was freshly set by Olduvai Gorge, to about 1.8 million years. Omo broke the two million year barrier. The associations were with early *Homo* (*Homo habilis*) (Leakey 1978). Further back in time was the idea of more primitive australopithecines, and then... a blank. The blank added an exotic sense of mystery, but it did not really matter, because the golden egg of culture started with the early tools. Further back somewhere were early apes, as at Rusinga or Fayum. As comparative framework, chimpanzees were nowhere. Savanna was everywhere – baboons dominated the field of modelling. Some of these views have been lost (and the task is to see not just what has replaced them, but what pointer that is to what will come next).

Were the problems solved? It seems more, as Arber (1985) shows, that problems in science are often not directly solved, but tackled until eventually some development leads to them being bypassed. It is sometimes said that major new ideas have a 15-year timescale before they rejoin the mainstream. In that case, we have been through several phases. In palaeoanthropology, we might have the Leakey effect 1960–1975; then the Isaac ideas – 1970–1985. The Binford middle rangeism was dominant 1980–1995 (see e.g. Binford 1983; Isaac 1969, 1972; Leakey 1978). David Clarke’s version of the New Archaeology had wider implications, but its greatest influence was in the same years as Isaac’s. There has been less postprocessualism, which has had indirect effects in the Palaeolithic. Arguably, we have been in our present “modern” phase since the late 1990s, without any one dominant paradigm, and perhaps we should be looking for a new “-ism”.

Newer Ideas

The current set of ideas represents a definite departure – they crystallised with the new millennium. They include the new importance of primatology, which through chimpanzee activities brings a new life to the stone ages. Then there is a vista opened up by the new suite of very early hominids (*Orrorin*, *Sahelanthropus* and *Ardipithecus*). From 2000 too came a deep extension of declared interests in early modern humans – a perspective of 300,000 rather than 50,000 years (McBrearty and Brooks 2000; Barham 2002). Equally or more important is the extension of a genetic framework, with the first Neanderthal DNA (from 1997) paving the way to a Neanderthal genome project, which will fundamentally alter our understanding of the last million years (i.e. the time back to a common ancestor of Neanderthals and modern humans) (Green et al. 2006; Krause et al. 2007; Krings et al. 1997). Then there is the intervention of evolutionary psychology, which paints broad brush across the past, projecting in interpretations seemingly without a direct need for archaeology. As long ago as 1997, I noted that archaeology needs to come to terms with the power of the new disciplines (Gowlett 1997). It cannot just operate on its own. If it tries to do so, it will be bypassed.

The New Developments

These new phenomena have done much to reshape the subject area. As hinted, we can take a modern phase from 2000, and try to follow it through. The changing big picture of human evolution seems almost equally driven by the several developments. The fossil record is key, because nothing else would give us views of brain size, teeth, and bipedalism. Very gradually, I would predict, it will link up with genetic insights. That is, science will begin to know the genetic basis underpinning particular characters – and then triangulating from the human, chimpanzee, and Neanderthal genomes, it may become possible to fix the points at which certain evolutionary changes occurred. We also know that (apart from the major component of drift) the genetic changes are largely driven by behavioural changes, which of course are partly documented by the very artefacts we study.

In the near future the preoccupation will be with understanding the Neanderthals as Neanderthals. Eventually, however, triangulation from moderns and Neanderthals should get us close to comprehending the genome of *Homo erectus*, at the head of the two diverging lines about one million years ago. That is a time of major change – with a *heidelbergensis*-like *Homo* perhaps appearing widely before the final divergence, and almost certainly being linked with some of the precocious events visible in the Acheulean (e.g. at Atapuerca or Bodo). The genetics is clearly a two-edged sword, if not used with great care. Authors have assumed that signs of a population bottleneck in humans were related to a recent squeeze, perhaps linked with the Toba eruption at 70,000 years ago (Ambrose 1998). Then similarly the discovery that the *FOXP2* gene had a mutated form in modern humans was immediately linked with a recent “language event” in modern humans. The discovery that Neanderthal variation has similar narrow bandwidth, and that they too had our version of *FOXP2* (Krause et al. 2007) completely alters such interpretations, and moves the developments way beyond the range of Toba or recent language.

Such readjustments tell us not just about themselves, but about the likely need for many similar corrections of current views, especially those reached rather hastily. Even so, the corrections too are coming from genetic evidence, and if we compare 1989 with 2009, then 2029 should have a juicy menu on offer.

More fare will certainly come from the hominin remains. Wang and Crompton’s (2004) analyses of hominin carrying show the kind of work that can be done, that relates to artefacts, as do Trinkaus’ longer-standing explorations of Neanderthal life style (see Trinkaus and Shipman 1993). In a similar vein, Aiello and Dunbar (1993), Aiello and Wheeler (1995), and Aiello and Key (2002) are all working towards what *must* have happened in some particular way, as in the expensive tissue hypothesis, which asserts that we could not have acquired our large expensive brains without reduction in other key tissue, with further knock-on implications.

The same can be said for evolutionary psychology, although it is anathema to some archaeologists. As with the genetics, it sometimes marches too confidently across areas which have already been explored by archaeologists, and whose data strongly point towards other views.

The importance of social factors is not new, but the evolutionary psychology gives it a new focus beyond the “ordinary” social archaeology. Indeed the Machiavellian intelligence and Social Brain ideas make a direct link with primatology, which also enters the picture through studies of primate material culture (Byrne and Whiten 1988; Dunbar 1998, 2003; Dunbar et al. 2010; Gowlett 2009b; Lycett et al. 2009; Whiten et al. 2009). The Social Brain makes plain that there were always larger worlds than archaeology can see directly (Dunbar et al. 2009). So when somebody, like Gargett (1989, 1999), could say that a body might be disposed of just because it was in the way or smelled, we can counter that brain evidence suggests that these hominins had four or five levels of intentionality and that they would fully understand what they were doing. We can look to other archaeological signs – such as the spatial separation of infants and adult burials – and say that this fits. Social brain tells us about changes in group sizes, and also to expect a long and gradual emergence of language as a replacement for primate grooming. Where can it take us next? The main thing may be for archaeology to keep working through the ideas. We need to articulate more fully in our theory that “WYSWTW” (What you see is what there was) is a fundamentally unsatisfactory approach. To say “We cannot believe in x or y until we have seen it at least three times” is no longer a good approach, if other disciplines will say “it should be there and you archaeologists just did not find it.”

Part of an answer can come from a fuller exploration of sampling issues, as urged in the original New Archaeology (e.g. Clarke 1968: 549–551). The social brain estimates roughly the size of “intellectual container” that we need for encapsulating these events. If we see event-type A twice, and 1,000 times we do not, we can feel more comfortable in accepting the evidence at face value, saying, “Well that fits; they were capable of it; but preservation really is as odd a thing as Lyell realised in 1863 (Lyell 1863)”. The Social Brain idea may also help us to see what drove the major changes, but this point is less clear. It sees group sizes changing through changes in ecological variables. Increasingly, humans inherited changes caused by their own evolution, and it seems strange that feedback models have largely gone out of fashion. They must surely be indispensable to working out the nature of long-term evolutionary trends. The most striking thing about the evolution of *Homo* is the rapidity of change consistently maintained in continuing trends. We can profit from returning to an interest in their mechanisms.

Material culture has not gone away. It belongs both in the evolutionary dynamics and the detailed picture. In the first, a new comparative picture is emerging as the variations in chimpanzee culture become more apparent, and as the known chimpanzee traits become extended, sometimes dramatically as through the Fongoli spears (Pruetz and Bertolani 2007). Chimpanzee artefacts can be studied in the same way as human ones (e.g. Carvalho et al. 2009; Gowlett 2009b; Lycett et al. 2010), and it is the variation in their culture which will offer some of the best analogies and comparators for human and early human artefact patterning.

The relationship between artefact distribution and cultural boundaries then becomes a central issue: what is proxy for what? Hodder has shown that artefact type and ethnic boundary do not necessarily coincide (Hodder 1977, 1978).

As the interest moves from ethnic units to social networks, further studies are required on the relationship among movement, material value, distance, and area. For the large picture of human evolution, there is more to gain from further studies of social transmission, “culture” and imitation, and many of these will depend directly on artefacts.

Typology is one of the areas which – in the fashion which Arber (1985) describes – has been eclipsed, largely replaced by concepts of social technology (in which signatures extend through time in the making of an artefact, rather than being crystallised in its final form). But the death of typology can be exaggerated – we should go back and take from it what we need, unashamedly, recognising now far more easily that there is no fixed boundary between the static (declarative) and dynamic (procedural and *savoir-faire*) aspects of making and using tools. The area is also bolstered by the new need for classification and analysis of ape tools. The human ability to handle “many” side by side, whether it is human relationships or artefacts, is another fundamental part of our evolution and helps to justify typology as an agent for studying “multiplicity”.

With their large numbers and many characteristics artefacts lend themselves to quantitative study. Some degree of fashion change is seen in the move from univariate and bivariate statistics to multivariate approaches (first made practicable on mainframe computers in the 1970s), and then in the development of new multivariate techniques. Principal components and discriminant analysis remain with us, but cluster analysis seems less used, while the new Morphometrics has gained in popularity. There seems a valid use for all these, again with advantages coming from using pairs of techniques to triangulate on a solution. Hierarchical cluster analysis was used by Daniel Cahen to study Acheulean cleavers as early as 1969 and has been employed recently for the study of matriarchal and patriarchal lineages (Holden and Mace 2003), as well as in genetics; it surely still has more to offer when applied to artefacts.

New is often better, and the Morphometrics has many possibilities. For instance, its techniques allow the analysis of form free of size variation (effects), with particular benefits for archaeology’s yearning to explore templates. Such an “ideal” form should not exist in biology, as natural selection is primarily undirected, but in cultural phenomena (and here we hark back to Plato’s *Ideals*) the pressures towards norms can create the situation where everyone agrees about the same thing (“it should be just like this”). The implication is that we need to know a great deal more about stereotypes and templates, and how they operate in modern humans, to get even more out of these techniques (see Hodgson 2006 for recent discussion of related issues).

The Detailed View: Slices of Space and Time

The more detailed Archaeology does not offer similar benchmarks, more a continuity of change. The “great archaeologists” set the framework – Francois Bordes’ major excavations and his typology dominated the 1950s (Bordes 1972). Leroi-Gourhan’s

palaeoethnology and social approach carried greater weight from 1960, when the Pincevent excavations began (Leroi-Gourhan and Brézillon 1972). Clark (1962, 2001) had paved the way at Kalambo Falls, with the concept of area excavations and living surfaces – but no doubt he too had learnt from the earlier great Russian excavations and those of Alfred Rust at the Pinnberg (Rust 1958). Clark Howell grappled with the Somme sequence as well as the problems of Ambrona and Torralba (Howell 1966). Charles McBurney excavated the Haua Fteah and La Cotte de St Brelade (Callow and Cornford 1986; McBurney 1967). This was the excavation landscape. All their works show that quantitative archaeology had begun to filter through ahead of the classic New Archaeology. All in all, there was an accumulation of techniques allowing a broader and better record to be built. Looking forward, the signs are – the inevitability is – that this is what archaeologists will build on in a continuous tradition.

The archaeology of detail is steadily moving towards a wider range of proxies for the past. They still divide essentially into those that are part of the human behaviour and those that are part of the environment. A stone tool is in the first, but its material drawn from the second. A bone with cut marks and hyena tooth marks manages to be part of both.

The frame still divides into site and landscape, the first dominated by denser swarms of artefacts. Then, typology and measurement are still first weapons in the artefact armoury. Microwear study has rarely displaced them and does not look likely to. In artefact study, there is a great separation of form study and function study. The latter is usually studied quite simply as “cutting” or “scraping”. We can hope to move towards a more linked-through approach in which the whole artefact is related with biomechanics of the hominin. We can measure things as work that needs to be done. Now that the whole artefact can be captured by 3D scanning it can be studied in many ways as a virtual solid object – as a single specimen, in its biomechanical framework, or as part of a varied horde.

Style studies, as analysed by Sackett, will also have a place. Sackett (1977, 1982) has given a theoretical basis for the relationship between style and function, which has a renewed relevance as we begin to look at variability again in the light of the local variations in chimpanzee culture (Lycett et al. 2009; Whiten et al. 1999). For Sackett, style and function are complementary, everything in the artefact to be accounted for by one or the other. There is great scope for testing such ideas as our data sets and comparative framework improve. Looking across for a moment to the New World (Chap. 14), it is evident that North American scholars have been particularly successful in selecting key aspects of a problem (e.g. style, curation, the *chaîne opératoire*, etc.) and providing detailed thoughtful analysis (e.g. Sackett 1977; Shott 1996; Shott and Sillitoe 2005; Chap. 8).

On the scale of whole site, coordinate plotting of finds continues and has been made far easier by electronic theodolites. Point-pattern studies will thus continue, and often they can be interpreted visually, as when the finds from two adjacent layers are represented with different colours. Such studies are easier and more economic of time than some of the old number crunching. Refitting of artefacts can be shown on the plots, but it still depends on time-intensive searching of collections

and the experienced eye. In the very long term, we might hope for its processes to become automated. The benefits of site-wide studies of patterning and refits are clear – it is simply that the costs are high. Kroll has shown the advantages and pointed out how helpful these analyses can be on the larger sites, such as the Olduvai and East Turkana surfaces (Kroll 1994, 1997).

Landscape studies can sometimes be made on the basis of reconstruction of palaeolandscape, as at Olduvai or East Turkana (Hay 1976; Blumenshine and Peters, 1998; Chap. 7). Very often the site may be in a landscape too changed for direct interpretation. Then we rely even more on the study of raw material transport. Leakey and Hay's work has been followed and amplified in Europe by Geneste (1991) and acute accent: Féblot-Augustins (1999). Linking site and landscape, Schick and Toth emphasise artefact imports and exports that tell us of dynamics (Schick 1987; Schick and Toth 1993; Toth 1987; Toth and Schick 2004). Gamble (1999) has told us of networks and social landscapes, Aureli et al. (2008) of the fission and fusion which underlie the patterns of movement. There is vast scope to put these together. Essentially population groups hold ground. The further an individual travels from that ground, in linear movement, the more there needs to be negotiation – a social passport. Artefacts offer just a dim shadow of these movements, emphasising a need for new modelling of potential frameworks, some of it at least by agent-based techniques (e.g. Sellers et al. 2007, in the case of baboon foraging). Traditionally much of the interpretation has been made in terms of exchange and alliance – but there may of course be other explanations. Hodder (1977, 1978) explained the difficulty of distinguishing between different “fall off” distributions, as their feature in common tends to be very poor sampling of the fringes (i.e. the flange of the bell in a bell curve).

We do know for sure that larger scales of group have emerged in human evolution. They have acknowledged importance as “superbands” and “dialect tribes” (Tindale 1940; Wobst 1974). How can we see them? An issue emerging via evolutionary psychology is that such groups are scaled, rising with a common factor of ca. 3–4 (Zhou et al. 2005). In chimpanzees, the community is the largest visible entity. Modern humans always reckon part of their identity from groups measured in hundreds or thousands. Archaeological evidence for such larger groups may turn out to be indirect (i.e. not expressed as larger sites, *pace* those archaeologists who have expected to see these in the “human revolution” – those who write in these terms are often unaware of the sheer scale of early African sites). A mixture of empirical and modelling approaches may be necessary to gain better understanding of this crucial group scaling (Clarke 1968; Isaac 1972; Steele 1994; Grove 2009; Zhou et al. 2005).

Alongside our own artefact studies, the area more traditionally known to the Physical Sciences and Chemistry as “Analysis” now forms a major area of “archaeological science”, but not in a very systematic way for the Palaeolithic. In its application, we might expect to find by now a sort of “rule book” applicable to every situation. In practice, the needs vary enormously according to situation. On one site chemical analysis and sourcing of rocks may be necessary; on another the key facts may be visible to the naked eye. But these techniques are often the means of identification of rocks, tuffs, and raw materials as already mentioned. Other problems

are tackled successfully through studies of stable isotopes (cf. Backwell and d'Errico 2005). Although these include especially studies of diet from bone, other techniques can be brought into play such as analysis of habitat association (de Ruiter et al. 2008). Occasionally phytoliths on lithic artefacts have given clues of function (Domínguez-Rodrigo et al. 2001), and old staples such as pollen analysis sometimes bear fruit in environmental reconstruction, so we may expect a constant reworking and refinements of such techniques, applied singly or in conjunction.

While studies are still largely “site-centred” a generation after “off-site archaeology” was first mooted (Foley 1977), the pictures that emerge seem much more complete and relate more to inputs and outputs.

Debit and Credit

Mary Leakey used to talk of putting finds of doubtful status into the “suspense account”. Here she placed the first derived artefacts from Hadar, found in gravels (Corvinus and Roche 1976). In a similar valuation, we might talk of debit and credit in newer approaches.

Now we can return for a moment to Isaac’s *Whither archaeology?* (1971), to put his ideas in the scales. First, Isaac defended modernization in Archaeology – it was necessary, and still is. He also noted, above all, the need for a discipline to maintain its factual basis, and again that still helps us. But then he noted especially Archaeology’s new sense of building itself as a separate discipline – here I believe danger began. Over the years, Archaeology as a whole, through its obsessions with theory, and the heavy load of detail, has addressed its own community rather than reaching out, and so has lost impact. It must learn to regain it.

Then, we have a battery of powerful and sometimes new techniques, but Archaeology and analysis are not necessarily improved with the passage of time. Practitioners do not necessarily grasp better perspectives. In Britain at least, those coming into the discipline have less and less scientific and mathematical background, and they sometimes fall into traps that they would have seen a generation ago. Although the New Archaeology sets an excellent direction on the topic, often we do not handle sampling issues well. We do not look out to disciplines with comparable problems, for example, astronomy. Astronomy has all our uncertainties – sometimes orders of magnitude more – and is more efficient and fair-minded in selecting its working hypotheses. Archaeologists are discomfited by having a 500,000-year range of doubt in the origins of this or that technique or practice. They tend to respond by taking a conservative view, thinking that “late is safe”, even though this often means choosing to be wrong. Astronomers will simply mark out a range of uncertainty and work on reducing it. Good science takes out value judgements.

These issues are becoming more important, because Archaeology has felt that it had first right to control interpretation of the past, but it is having to admit the claims of other disciplines to paint in the record. In many ways, the development is

good, because it is leading to a very gradual erosion of “WYSWTW”, and a better appreciation of past worlds that are bigger than our own materially restricted one.

Last, on the debit side, we seem not very transparent about the law of diminishing returns. We still fill in time doing *something* to an archaeological assemblage, rather than nothing – a great deal of measuring goes on that will not have a measurable return. On the other hand, to be positive, our position is somewhat like that in police work, where there can be a decision to concentrate on serious crime, and huge investment in a murder committed years ago may have unexpected payoffs. My strictures should not have too serious a ring, because part of what we need to do is to put the fun back into Archaeology.

Where Is this Going Now? Concluding Thoughts

So what is our task in the future? This will be shaped by many archaeologists, not one person’s opinion. It seems inevitable that there will be much more of the same – the construction of detailed pasts from local artefact records, and the search for grander scale evolutionary dynamics. The second depends on the first – the basis of sound data.

Helping with this development is a gradual extension of involvement with material culture, beyond the old preoccupation with lithics, to other materials (e.g. Chap.2), and even beyond human tools per se (e.g. Chap.5).

In all this is a gradual engagement with the issues of other disciplines. We are not alone in noting the blindspots in the coming together. Often they are even worse than us in neglecting relevant literature. Note how Bickerton (2003) calls for a broader cross-disciplinary appreciation in early language studies: it should not, he urges, be beyond us to look around and master the basics of several disciplines – and we need to do so, because our explanations have to be valid in all of them.

Such “whole world” considerations (perhaps best not use the word “holistic” which now smacks of alternative therapies) – turning to language itself – can help us to a better understanding of how language begins to operate on social tradition. Recent definitions of culture (e.g., Boesch 2003) include “shared understanding” – but as anthropologists we can also ask how do we know that the understanding is shared? What does sharing mean? As “jointly partaking” it means far less than “exchanging insights”, and archaeological data might often show the first, only hinting at the second. As archaeologists are well used to analysing data and ideas in a very painstaking way, they should be able to investigate these issues as well as any discipline – we can only give detail to the rise of intentionality if we have our own good insights into its nature.

We also need a better understanding of what trends entail. How surprisingly little is written about the *nature* of evolutionary trends (with Clarke 1968 and Janis and Damuth 1990 as rare exceptions). In the strictest sense, they have no meaning because evolution has no purposeful direction. At each moment, different selection pressures would lead to a different direction of response. But the trends do occur,

and they do have significance in human interpretation – we are interested in how things have happened over long periods, and any prolonged response helps us to discern factors which have operated consistently through time.

This interpretation of trends – the study of rates of change – is of course crucial in the discipline. It goes back to our measured quantities, such as stone artefacts and dates, and highlights the importance of mathematical models (cf. Chaps. 5, 8, 12). Its principle is also what allows us to hazard a guess at the future – from a plot of past changes. To summarise, here are some few things that we might aim for.

To seize again the impact that is due to our discipline – to emphasise the role of Archaeology as the main custodian of the past record.

To operate in our discipline with more strategic purpose, by grand design (as do astronomers and even primatologists), setting out goals for future study.

To articulate better and more explicitly with neighbouring disciplines, such as evolutionary anthropology, primatology, genetics, and evolutionary psychology.

To give strong support to varying scales of projects: the smaller are often highly focused, and give very good returns, the larger offer a scale that provides answers otherwise completely unavailable.

In detailed analysis, we could aim to operate with a more readily available manual of rigorous approaches, looking to earlier work for the best in it, rather than discarding much of our own record simply because it was reported more than a few years ago. There is the real challenge – a mature archaeology cannot operate with a time bar. But if there is any risk of analytical impoverishment, this volume shows the contrary.

And finally, should we indeed be looking for a new -ism? That might be available in the “social brain” or in a new generation of cultural studies inspired by developments in the primate world. I think it will come from somewhere else. It will probably be introduced by younger archaeologists. It will have good and bad points, but it will lever our past into the future.

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