

LITHICS

The Newsletter of the
Lithic Studies Society

No. 16

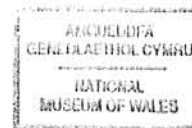
for 1995

The Lithic Studies Society was founded in 1979, and seeks to advance the international study of lithic industries in the broadest possible context. New members are welcome, and details of the Society's activities may be obtained from the Secretary, Elizabeth Walker, Department of Archaeology and Nusimatics, The National Museum and Gallery, Cathays Park, Cardiff, CF1 3NP.

The officers of the Society for 1995 were: Frances Healy (Chairperson); John Humble (Vice-Chairman); John Lewis (Hon Secretary); Louise Austin (Hon Treasurer and Membership Secretary); Alison Roberts (Hon Editor) and Nick Ashton and John McNabb (Hon Guest Editors). Members serving on the General Committee were: Peter Berridge; Lynn Bevan; Pippa Bradley; Andrew David; Phil Harding; Paul Pettitt; John Schofield; and John Wymer.

The annual subscription, due on October 1st, is £5, and is payable to the Treasurer, Louise Austin, 126A Gilbert Road, Cambridge, from whom Banker's Order forms may be obtained. The Newsletter is circulated free of charge to all members of the Society. Back issues may be obtained by sending orders with payment to the Treasurer. Cheques should be made payable to The Lithic Studies Society.

Paul Pettitt will be the editor of *Lithics 17* and contributions should be sent to Research Laboratory for Archaeology and the History of Art, 6 Keble Road, Oxford. Manuscripts should be submitted in duplicate following the 'Notes to Authors' printed in *Lithics 13*.



Editors Note

This volume of *Lithics* is dedicated to the archaeology of pre-modern humans, effectively a Lower and Middle Palaeolithic special edition. Thematic volumes are a new experiment for *Lithics*, and it is up to you, the members, to tell us whether or not you like the idea.

The genesis of this particular experiment is unusual. In 1994 the Royal Archaeological Institute published a volume called *Building on the Past*, to celebrate its 150th anniversary. It gave a 'state of the nation' type coverage for British archaeology from the earliest occupation of Britain (c 500,000 years ago), down to 1750 AD. Of the 237 pages devoted to this immense period of time, the Lower and Middle Palaeolithic (for the sake of argument 500,000-35,000) received just one page. In effect, 0.4% of a volume on British archaeology for a period encompassing c.93% of that time. Not good enough!

To be fair, the author of the Palaeolithic section, Peter Rowley-Conwy is not to blame. He fairly and squarely states his biases, pre-modern humans are not his thing. Nonetheless, he gives the impression, unintentionally, of a thin and esoteric subject; an archaeological landscape barren of people but littered with endless artefacts as unknowable as their makers. The spectres of missing cultural agendas and a past too remote to be measured, and hence understood, loom large.

For those of us who are into the pre-modern thing, that gauntlet can not be allowed to lie. In the fifteen years that we have been involved in Lower and Middle Palaeolithic archaeology the subject has changed out of all proportion. We have lived and are living through a revolution. These very unknowables are now the challenges for the future. New chronologies, behavioural reconstructions, the integration of artefact research with assemblage based studies, and multi-disciplinary approaches, are the new research goals. This volume reflects one aspect of these new horizons, the contribution stone tools have to make to the debate. The choice of papers in this volume deliberately reflect a range of subjects, theoretical, speculative, contextual, artefactual, all highlighting the diversity with which we can begin to discuss the remote past. It is this scope to the debate that reveals the dynamism inherent in the archaeology of pre-modern humans on both physical and conceptual levels. We hope that you will agree that these papers do reveal something of the potential of stone tool studies in humanising the Palaeolithic, as well as dispelling the shadow that some believe a distant antiquity seems to throw.

Ultimately archaeology is a dialogue between people, about people. The diversity of approach reflected in the following pages is what gives that dialogue its richness, and it is what makes the study of ancient humans one of the most exciting in archaeology today.

John McNabb and Nick Ashton

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A MATTER OF FORM: INSTRUCTION SETS AND THE SHAPING OF EARLY TECHNOLOGY

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This paper is about form in the record of the early human past. After a period when it was out of favour, technology has become prominent in recent studies, but form is analytically difficult to explore (at least for the author), and so this is offered as a discussion document rather than as any kind of *Diktat*.

We can start by looking at the general framework of human evolution: where is there the opportunity to look at technology, which might record form? The comparative framework of biology is now widely held to suggest a common ancestor of humans and chimpanzees perhaps 6-8 million years ago; to this we can add chimpanzee technology in the present; and a record of human technology going back about 2.5 million years. What then were the abilities of the common ancestor? Although this is a question of major interest, there seems no way at present of determining whether the common ancestor had simple technology, or whether chimpanzees and hominids have acquired it independently. Certain detailed anatomical similarities in the forelimbs, and in the tooth enamel, suggest that gorillas and chimpanzees had a common ancestry after the hominids diverged (although other evidence, reviewed for example in Andrews 1986, points to chimps and humans having a later common ancestor). As gorillas are not tool-users this is a first warning of the complexity of the situation.

The 'common ancestor question' nevertheless highlights some of our problems. If we envisage the frame of the few million years since the common ancestor as a 'black box', then a sort of stalactite of technology hangs down into it (Fig. 1). This is hominid technology, and there is no trace of an equivalent record for the chimpanzee. Fossils are the only other tracer, and again the picture is remarkably one sided: there is hardly one ape tooth that can be set alongside the hominid remains of the past few million years. The main field of the box can thus only be handled by general modelling, in a comparative framework (e.g. Alexander 1979; papers in Steele and Shennan 1996). In contrast the 'spike' of technology gives very specific information which is therefore difficult to integrate. There is a very tricky steep interface between the two areas of knowledge, which perhaps explains some of the difficulties of reconciling academic views.

My aim is to examine these problems through a series of propositions.

1. The Stone Age has been rewritten

In the last generation the Stone Age technology side of this picture has been rewritten. Scholars have turned away from 'a narrow concern with artefact morphology' (Isaac 1967). So what now are the concerns of Palaeolithic

A matter of form

archaeology? We might expect them to be matters of behaviour and culture on a social basis, in accordance with developments both in archaeology and primate studies.

But paradoxically what we have is actually largely a record of TECHNOLOGY, and this almost exclusively in the form of stone artefacts. Some authors, including Foley (1987), prefer altogether to talk of technology rather than culture for the early periods. The actual terminology is probably not important, since technology is culturally transmitted in any case.

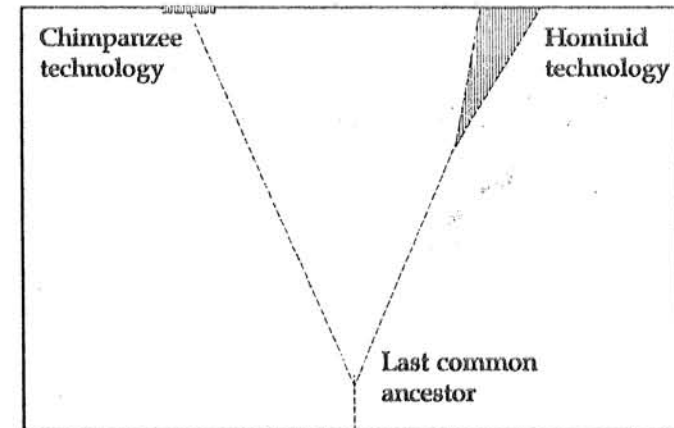


Fig. 1 The technological spike in the 'black box': the difficulties of reconciling specific and general knowledge

2. Technology is very much form and function

It is difficult to see how it can be otherwise, though this proposition is intentionally put in a provocative form. Holloway (1969) stressed the importance of 'arbitrary form' as an essential element of culture, and some notion of this appears in earlier anthropology. The term 'arbitrary' seems, all the same, to cause real difficulty: surely, people say, if a cultural solution is chosen for a reason, it is not arbitrary. It seems to me that arbitrary in this sense means something like 'not known in nature, and potentially variable'. It is rare that any solution achieved through imposed form is the only solution, and hence the one chosen is 'arbitrary'. Perhaps 'projected' would be a better word.

This basic concept is immensely important because of what it allows today in human society. It embraces all elements of cultural activity, including

artefacts, physical structures and also social structures or institutions.

If it has this importance in western society, would it be equally important to, say, a hunting and gathering Australian aborigine? The answer must be yes, because of language and kinship. These are imposed structures with definite form. Language demands an innate capacity to handle it, but the content of an individual language is 'arbitrary' (i.e. *cheval* is as effective as *horse*). In some societies technology might be so simple that it hardly offers a framework, but imposed form is still there in a social sense.

3. Rewriting of Archaeology means challenges

The rewriting of archaeology involves challenges. I take as examples three recent challenges to the concept of form.

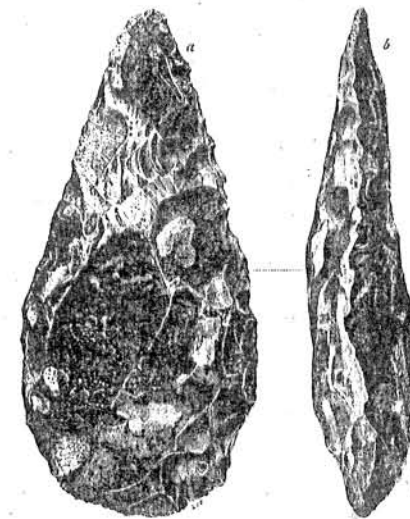
(1) the challenge by Toth (1985) for the Oldowan. In this well-known paper Toth takes the view that in the earliest industries form is only a consequence of the stone worker following paths of least resistance. The knapper does not have a mental template of a shaped core-tool, and flake production is the main goal.

(2) the challenge by Dibble (e.g. 1988) for the Mousterian. The Bordes typology included more than a dozen 'types' of sidescraper, but Dibble has shown that these are largely the product of a retrimming sequence. To put them into separate type categories makes little more sense than doing the same with pencil stubs of different lengths. Dibble proceeds to argue that in the Middle and Lower Palaeolithic all apparently fixed form is the result of functional needs, not of design. He believes that archaeologists who find 'types' and pattern do so largely as a function of their own expectation (Dibble 1989).

(3) the challenge by Davidson and Noble (1993) for industries from early times through to the Middle Palaeolithic presents similar views. These authors consider that 'as late as the Middle Palaeolithic, stone tools derive most of the apparent patterning observed by archaeologists from functional aspects of manufacture and use...' Thus they prefer to see the Acheulean hand-axes as cores, 'often discarded when no further flakes could be easily detached (and therefore with a 'finished appearance')'. Similarly the leafpoints of the late Mousterian can be regarded as cores, and 'Tranchet axes such as those from Boxgrove would be viewed not as specialist tools, but as bifacial cores for which tranchet blows were the next appropriate source of flakes' (Davidson 1991).

A Reappraisal

These challenges are a legitimate part of the effort to rewrite Palaeolithic archaeology. But how far can they go? In the modern world design underlies every artefact. And how far can we cut through the 'great sleep' of 1900-1960, before we encounter the bedrock of evolutionary archaeology?



Flint implement from St. Acheul, near Amiens, of the spear-head shape.

Fig. 2 Lanceolate biface figured by Lyell (shown by him at half actual size)

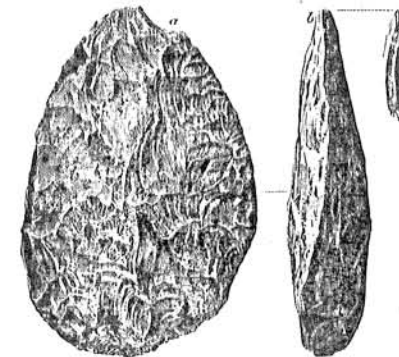


Fig. 3 Ovate biface figured by Lyell (shown by him at half actual size)

Archaeologists of the 1860s could not make easy assumptions. For evolutionists of the 19th century every battle was hard fought as they sought to persuade the world that 'antediluvian' artefacts existed. We can take as one example the work of the geologist Lyell (1863):

As much doubt has been cast on the question, whether the so-called flint hatchets have really been shaped by the hands of Man, it will be desirable to begin by satisfying the reader's mind on that point...

Lyell toured the pits around Abbeville in the Somme valley accompanied by M. Boucher de Perthes himself, and records that he 'obtained seventy flint instruments... The two prevailing forms of these tools are represented...' (Figs. 2 and 3). He continues:

Respecting the authenticity of the tools as works of art, Professor Ramsay, than whom no one could be a more competent judge, observes: 'For more than twenty years, like others of my craft, I have daily handled stones, whether fashioned by nature or art; and the flint hatchets of Amiens and Abbeville seem to me as clearly works of art as any Sheffield whittle.'

Mr Evans observes that there is a uniformity of shape, a correctness of outline, and a sharpness about the cutting edges and points, which cannot be due to anything but design.

These points were argued with success, otherwise we would not have the Palaeolithic archaeology which we know today. Lyell's scientific approach is interesting: he knew that to convince one must present actual evidence, not just as single specimens, but in a series, and that it must be illustrated: a verbal picture is insufficient.

The proposition I draw from this is that:

Essentially stone artefacts must have form - or we could not recognise them.

And then as a corollary:

If it was the same as natural form - we would not know that they were artefacts

How can these arguments be sustained if there are circumstances where natural activities can produce exactly the same effect as those intended by a human agent? This might arise if a flake is struck when stones clash together in a stream bed or on a beach. Few archaeologists would be sure that they can always, without exception, distinguish between the natural and artificial products. This is so even though nature has no pattern in its strikes. If there are enough cases in total, there will be those where one strike produces a

platform, and a later one lands a blow in exactly the direction and with exactly the force that a human would intend.

The implication is surely that complex instruction sets cannot be mimicked by nature, with any reasonable chance, but that simple ones could be. So the longer the artificial chain, the greater the instruction set, the less the likelihood that random strikes would add up to the same effect. Any statistical study of a natural series should find only a very small proportion of pieces which show anything like the pattern of strikes found in any human-generated assemblage. This is indeed the pattern of colith assemblages from beaches.

The challenge of chimpanzee tool-making: towards a comparative framework?

Studies of chimpanzees show that they, as well as humans, regularly make tools. This knowledge seemingly stems from recent studies, but McGrew (1993) reminds us that much of the knowledge is not new. Chimpanzees do not flake artefacts in the wild, but recent experimental studies show that the bonobo Kanzi can be induced to strike flakes (Schick and Toth 1993; Toth *et al.* 1993) presenting a further challenge to traditional ideas and making it much plainer that we do have a potential comparative framework.

Holloway (1969) made a very important step in linking arbitrary form and culture, and arguing that they represented a uniquely human domain. In one respect I differ with this in a further proposition:

Arbitrary form need not be diagnostic of humans - it is diagnostic of artefacts.

Consider for example the chimpanzee and its termite-fishing stick; or the stones used by the chimpanzees studied by Boesch and Boesch (Boesch 1993) - there is a desired form which is created or at least chosen. This is especially so for the termite-fishing stick, where a stem of the appropriate plant is selected, the right length is broken off, and side-stems removed (McGrew 1993; McGrew *et al.* 1979).

Some readers may feel that a 'culturalist' would be unhappy at the idea of humans having to share their cultural pedestal - but in my case actually no. I have long been convinced that routines operated as skills are a prime link between humans and non-humans. If arbitrary form is common to more than one species, then this gives us a major opportunity to analyse its complexity in a comparative frame, and in the early past. The process, however may raise many objections. Why should one do this? How can you set about mapping out past worlds of mental activity? Does one not need appropriate models, which may not be to hand?

I would seek to answer as follows:

Why? - because arbitrary form contains so much information, and gives us so much information.

Doesn't one need a model? - Yes, one needs *models*.

These models can be provided by psychological approaches. One example is the work of Wynn, much of which is formally based on the ideas of Piaget (e.g. Piaget 1953; Wynn 1985, 1993, 1995). Although Piaget's stages of child development are regarded as somewhat fixed simplifications by some psychologists, they may nevertheless reflect levels of abilities relevant to human evolution. Wynn has developed his approach beyond Piagetian principles, and shown for example the spatial abilities that are needed for handling bifacial edges, or constructing straight lines in a stone artefact (Wynn 1993; cf. fig. 4).

Decision step approach - study of routines

I have advocated an alternative approach that is based on the general analytical measurement techniques of modern cognitive psychology (cf. Blakemore 1988; MacKay 1991). This involves several models, and has a pedigree going back to the (apparently) first user of the term model, Craik (e.g. Sherwood 1966). This approach can employ analysis into routines, such as are widely used in the cognitive sciences. Examples are studies of how babies learn to use spoons (Connolly and Dalgleish 1989), or studies in man-machine interfaces (Annett 1969). For some the approach is descriptive, and lacking models such as used by Piaget. But let us consider, say, a biologist working on problems of domestication. If he uses some archaeological evidence, we don't say 'are you using Childe's model?' or something similar. Archaeology, and likewise psychology, use many models.

We are, inevitably, working with a number of models as basic assumptions: the fundamental one is that artefacts are made according to an instruction set, which must take into account functional demands. It is natural to ask how elaborate an instruction set was being used; and to what extent this appears to have been constrained by function, e.g. at different sizes (as in allometric studies). Such work is directly relevant to human evolution, since a chimpanzee, for all its abilities, cannot handle an instruction set to the extent that a human being can. Our abilities to handle instruction sets are fundamental to all modern technological abilities, but quite how they are handled is a topic for much further study. How is form imposed in the course of operating a routine? That is, how many variables is the maker considering at each stage? A mix of the two approaches mentioned above may be necessary for investigating this. Amongst other things research in these topics should help us to determine what model is more appropriate for the evolution of hominid intelligence:

- (a) a steady increase model over perhaps several million years
- (b) a late spurt model, as is now much favoured.
- (c) a combined model, in which some aspects of intelligence evolve before others.

It seems highly desirable to bring such choices of model out into the open: a good deal of recent work implicitly assumes model (b), but work such as that of Wynn, mentioned above, encourages us to concentrate on specific aspects of ability, such as decentration, rather than something as broad as 'intelligence'.

Examination of the challenges

I return now to examining the challenges noted earlier, in terms of these propositions. First, the diversity of opinion is absolutely necessary at the moment for exploring a field in which, as Sackett (1977, 1986) has pointed out, elements of style and function are not necessarily separable. Of course a first approach is just to illustrate the material (Figs 2-4). Many may feel that no more is necessary: the material could not acquire these shapes by accident, and the case is closed. Yet that approach does not allow us to explore the problem - the gradient from most rudimentary artefact to most sophisticated, and how it was traversed.

Toth's arguments for the Oldowan have achieved widespread support. The Leakey (1971) typology did not include cores in its list - although use of terms such as 'spheroid' did not necessarily imply their use as tools - and apparently put little emphasis on flakes. Toth's work redressed that balance, and went so far as to suggest that the Oldowan tool-maker need have no 'mental template' of form at all for shaping a core.

Does this mean that cores and flakes do not have formal mental schemata underlying them? The use of a term such as instruction set helps to clarify the position. The formula for striking a flake demands that the maker finds a suitable platform, presenting a suitable angle with a face of the core which allows an adequate flake run (i.e. pieces probably a few centimetres long). Nature can also achieve that formula accidentally.

The formula for working a core need not prescribe that the core should have a particular end-shape, but only that it should be made to yield a series of flakes, with characteristics within a certain range. The maker thus needs a certain foresight, or ability to manage a routine of action, otherwise subsequent flake-striking opportunities will be lost. Just practising such a routine may well tend to produce a core of a certain shape, but it need not be shaped for its own sake. What it can be said to share with a hand-axe is the existence of an underlying instruction set - a pattern of behaviour extended through time, embodying the knapper's experience, and not worked out afresh by the knapper each time.

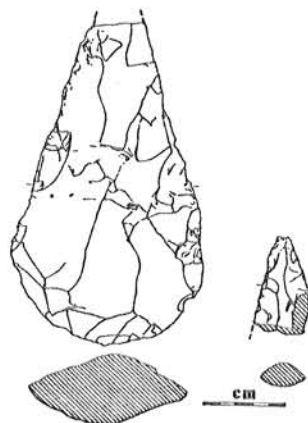


Fig. 4. Bifaces from Chesowanja, showing carefully made straight edges. These specimens (unfortunately broken) probably date within the last half-million years. The earliest date of such straight edges is not well-established

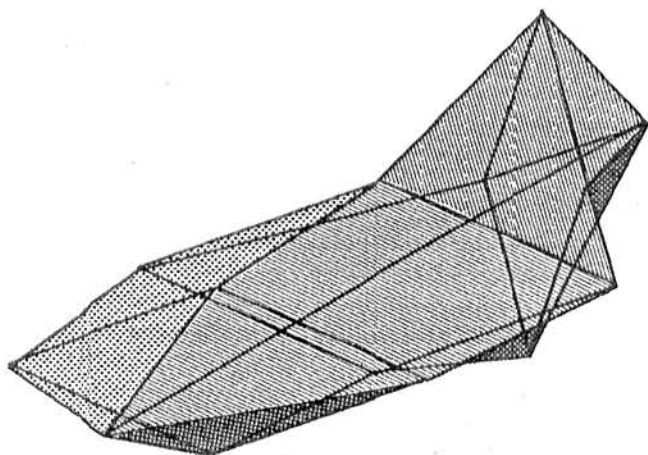


Fig. 5. A biface that looks 'wrong': the shape created by an accidental transposition in a computer program.

European and African contexts show us some variety even in Lower Palaeolithic core-working. In Britain this often entailed part-rotation of a piece to engage new platforms. McNabb and Ashton (1995) cite examples where each core preserves on average only two flaking episodes, each involving the removal of about three flakes. My impression is that African flaking at 0.8-1.0 million years often involves more definite and elaborate reduction sequences. The nature of available blanks may be an important factor: the cobble blanks (often of lava or quartzite) which are common in Africa may have been more amenable to working in set ways than the flint nodules of European material: sometimes from one platform, as in a 'core-scraper'; sometimes from two faces, as in 'discoids' and 'choppers'; or in other cases from any convenient platform as in the simple 'cores', which resemble much more those of the Clactonian. Such simple cores are not well-advertised in the African Acheulean, but at Olorgesailie Isaac (1977) isolated nearly 300 specimens from the various sites. These have a mean weight of about 240 grams, close to that of c. 200 grams on the sites mentioned by McNabb and Ashton. Isaac, however, operated a different procedure of typological separation, eliminating 'casual cores' with less than five flake scars, and attributing other pieces to shaped-tool categories (such as heavy-duty scraper) wherever possible.

One can summarise that the procedures favoured for flaking cores varied from Lower Palaeolithic industry to industry, but that in all of them it was guided by learned routines, which can be termed procedural templates (or something similar); and that these do not have to be sophisticated to be real.

Can such suggestions of pattern be reconciled with newer ideas about the Middle Palaeolithic, which place the emphasis not on design form and categories, but on response to use, as in the retrimming of worn artefacts? Dibble's work is a useful challenge which has received support from others (e.g. Otte 1990). His view of the Mousterian in South-Western Europe or the Middle East is however far more difficult to apply to roughly contemporary industries such as the Szeletian, the Aterian or the subsaharan MSA, where Blattspitzen, tanged points or finely-shaped Stillbay points look as much like carefully fashioned products made to design as do many recent artefacts.

Davidson and Noble (1993) have taken the bravest path in virtually denying the existence of finished artefacts prior to the Upper Pleistocene. The primary motivation in their work was to explore the origins of language, and the extent to which the archaeological record could be used as evidence. They concluded that no compelling evidence for language was embodied in the stone artefacts of the Lower and Middle Pleistocene, but that the later discovery of 'signs' is a major threshold.

Arguments in favour of design form need not be repeated (*vide* Lyell above, and for example: Hayden 1993; Wynn 1993, 1995; Gowlett 1996a, 1996b). There seems, however, to be less of a 'finished tool fallacy' than an 'unfinished tool fallacy': almost by definition a tool is made finished enough to work, otherwise it does not work.

But the disagreement has no need to be total: the benefit of the challenge is that it can prompt us to better analyses. Isaac's work at Olorgesailie suggested that there are modes of bifaces, heavy-duty artefacts and cores, but probably also morphological clines between them. Similar gradations between classes have been postulated at Terra Amata (Villa 1983:fig. 24), implicitly at Ubeidiya (Bar-Yosef and Goren-Inbar 1993: fig. 93) and between choppers and cores of the English Clactonian (Ashton *et al.* 1992).

How can there be such gradients: if we are sure that there are designed tools, then should we not be able to separate them clearly from mere cores? At the moment - if we take as an example Isaac's way of dividing up the possible continuum at Olorgesailie - it would appear that we cannot do this. A programme of comparative work, including ethnoarchaeological and experimental studies on standardisation, could vastly improve our abilities to discriminate between, say, unfinished-looking tools that are actually functionally adequate, classic specimens, those that are abandoned rejects, and those that are just cores.

Even within the biface category there are similar issues. In a re-examination of the issues, mainly in respect of European evidence, Ashton and McNabb (1994) accept the presence of a 'mental construct', but point out that many bifaces are 'non-classic'. These may have been underemphasised or ignored in past studies in Europe where most finds did not come from systematic excavation. In contrast, we often have all the artefacts from a given area on African sites, as on the Kalambo Falls or Isimila 'occupation surfaces'. Both Keller (1973) and Isaac (1977) assigned bifaces to sub-categories, recognising non-classic or 'other' bifaces as a significant component.

What is this phenomenon of the 'other biface'? It could be evidence which conflicts with a rigid notion of 'mental template', but so indeed does the variation seen in any biface set. Again, I would argue that the term 'instruction set' points towards a solution, because it is clear that this is not held as a kind of solid object in the brain. It is easily amenable to variation. The idea of set is central here: construct is a useful term, but it helps if it is taken to represent more than a single idea. The S-twist of profile, for example, is a single idea which was added to the ordinary biface design in a certain domain of space and time. Perhaps the easiest way to demonstrate this point is to offer a modern mistake: an accidental transposition in a computer program gives us a biface like the one in Fig. 5. Only the two parameters are exchanged, but the conflict with expectation is quite dramatic.

Thus the 'other biface' may be important evidence, as Ashton and McNabb suggest, not necessarily because it indicates absence of a construct, or even a template, but because it illustrates selective use of a subset of the normal classic biface ideas. The 'other bifaces' may have been *ad hoc* tools, made for the moment, but they are informative, because if we can separate their design elements objectively, we can compare these with the classic set, and gain a better idea of the components which were important to the makers.

The Acheulean extends a million years deep and three continents wide, so such factors may not be universals. Nevertheless Davidson (1991) felt that the biface phenomenon was too widespread and constant to be an indicator of linguistic tradition. There is a consensus that it would be very difficult to demonstrate language on the basis of early artefacts (although they may well reveal other abilities, and language may well have existed), but it is arguable that the differences of opinion over design form do not relate to the problem. The routines or instruction sets governing core-working and the routines governing biface production may or may not need to be passed on through language, but it surely hazardous to assert that one would connote language and the other would not. Chimpanzees surely know the distinction between a leaf-sponge and a termite-fishing stick, but they do not have language as we know it.

Overview

Now let us look again at some of the major problems raised by this survey:

(1) *Can function determine the form of a tool so that design form does not enter the matter?*

Perhaps this could be argued for a hammerstone, but in nearly every case the very intent of preparing a tool implies a mental test - the selection of what meets the intended needs from among all the things that do not: any extensive modification therefore entails working to design, if not to 'a design'.

(2) *Can we argue that the chimpanzee does not use design form?*

Everything indicates that chimpanzees prepare artefacts such as termite-fishing sticks according to an instruction set, which may dictate breaking of the stem to length, stripping of leaves, and peeling of bark: this is surely the execution of a design.

(3) *Can we seriously argue that early hominids were less able than modern chimpanzees?*

Several million years ago all hominoids may have been somewhat less intelligent. But the orang-utan, gorilla and chimpanzee have a similar grade of intelligence, although orang ancestors may have diverged 15 million years ago. Cranial endocasts may indicate changes of brain organisation in early hominids, but these cannot be linked directly with changes in abilities. Early hominids from 2.5 Ma were capable of lithic production at a significantly more advanced level than any living ape has (yet!) demonstrated. They also transported materials more systematically and over greater distances. It seems unwarranted

to suppose that they performed these tasks with less insight than modern chimpanzees display in comparable activities.

Conclusion: how effective are measures of intelligence?

A great deal of our work in human evolution is concerned one way or another with evaluating intelligence. We use this general label; 'intelligence' but perhaps it is misleadingly broad in its cover. For example, do more and less intelligent animals demonstrate more and less intelligence when they carry out identical tasks? Surely a chimpanzee which has learnt how to turn a tap on and off to get water, can do so just as well as we can.

The argument can be summarised in two points:

We cannot equate expressed intelligence with actual intelligence.

This is widely appreciated in archaeological argument (e.g. Mellars and Stringer 1989). Human beings are not more intelligent now than they were ten years ago, but the expression of intelligence in technology has developed even in that short period.

Early hominids might rival modern humans in one facet of intelligence but not another.

This helps to reconcile certain points. Wynn, for example, has seen the Acheulean as representing the same level of abilities as in modern humans. Certainly there is an overlap in skills, so that a modern human needs considerable practice to make a satisfactory hand-axe. Yet most of us would not see *Homo erectus* as being modern in intelligence. The proposition above entails a formal recognition that intelligence need not be averaged across all fields.

I would like to draw two final conclusions from this discussion:

We cannot make one-to-one conclusions over intelligence, but expressed intelligence at least indicates minimum abilities in one facet.

It may be helpful formally to recognise mosaic evolution in brain/mental evolution, just as we are accustomed to doing so in physical evolution.

Just as in hominid evolution bipedalism appears to have evolved before dental specialisation, which came before cranial enlargement, so, it seems likely, the brain/mind may have evolved in facets. With all its limitations, archaeological evidence offers some prospect of disentangling these facets and the time-

relationships in their evolution.

Author's note: This paper has emerged from a draft first read at an HEIR (Human Evolution Interdisciplinary Research Group) meeting in Sheffield early in 1992. The propositions were summarised on slides. Since then some related arguments have been expanded in two much longer papers (Gowlett 1996a, 1996b); here I have tried to keep to the original informal format, but to incorporate discussion of recent work. I am grateful to Chris Chippindale and Nick Ashton for thoughts on an earlier draft.

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