The methodology of examining very early engravings

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The identification and interpretation of groove markings on bone and other materials of the Middle and Lower Palaeolithic has been the subject of greatly varying standards of documentation and proof. This has significantly retarded discussion and research of cognitive aspects of human evolution, and has led to a considerable polarisation among scholars considering related subjects. This brief paper is an attempt to formulate a uniform standard for recognising very early intentional engravings, especially those on bone.

Introduction

The study of purported engravings of the Pleistocene, most particularly of the Lower and Middle Palaeolithic (L./M.P) periods, has since the 19th century been conducted in a largely idiosyncratic and inconsistent fashion, i.e. without a standard methodology. For portable palaeoart material of the Upper Palaeolithic, Marshack (e.g. 1985, 1989), d'Errico (e.g. 1994) and others have done extensive work in recognising anthropogenic markings and assessing their intentionality in a testable, repeatable format. By comparison, the systematic study of such aspects of earlier material has been neglected, and traditionally such specimens have been considered in biased frameworks of investigation. While some commentators have sought to demonstrate the anthropic origin or intentionality of undetermined markings without adequately documented evidence, others have brought to the task prejudices that would have severely limited the effectiveness of their judgment. In recent decades the latter bias stemmed from an uncritical acceptance of archaeological dogma generated by the replacement hypothesis of human evolution. This involved the view that no symbolic material could exist prior to the 'Upper Palaeolithic', therefore reports of such finds had to be false.

It is obvious that when such a belief system curtails effective consideration of an issue, scientific inquiry cannot proceed and the presentation of falsifiable propositions is usurped by beliefs. By the same token, false claims of very early palaeoart are equally damaging to a rigorous study of the origins of human cognition and related matters (e.g. language, symbolling), because they would lead to the formulation of invalid hypotheses. As a precaution, many scholars have adopted an excessively conservative attitude towards palaeoart claims of the L./M.P., which is equally biased (cf. Bednarik 1992a and debate). Yet this kind of polarisation has been instrumental in forming opinions about the material evidence for non-physical human evolution. This is clearly an unsatisfactory state, and to ameliorate it an agreed and uniform methodology for assessing such claims and materials scientifically and systematically rather than through the vibes or prejudices of commentators would be of great help.

In the case of Upper Palaeolithic linear portable engravings, d'Errico has created an appropriate methodology, although concerning his work on purported notational signs I have to reiterate that he is not capable of determining notational intent (nor is Marshack's methodology). This is merely a specific, albeit in one sense far-reaching objection to their interpretations, but in terms of the general method to be adopted, I am certainly in agreement with these two scholars. I also share d'Errico's predilection for replicative approaches, in which we both follow Semenov (1964), but again a note of caution: replication per se does not refute propositions or create refutable hypotheses, it is merely a method of investigation that *can* facilitate the formulation of such scientific propositions. I have used replication on a wide range of materials and technologies, but I insist that such work, including the study of replicate work traces and by-products, does not by itself yield scientific pronouncements. It needs to be part of an epistemologically sound framework.

Having also examined thousands of purported Pleistocene engravings on bone, dozens on ostrich eggshell fragments, and many more on ivory and stone, I have come to focus on those that predate the Upper Palaeolithic mode of technology — for reasons to do with my principal research interest, the initial formulation of human concepts of reality. This requires that we first securely identify those objects and markings of the earliest periods that might conceivably provide relevant clues. The complete lack of an appropriate methodology to securely identify such items as early beads, petroglyphs and portable engravings rendered it necessary that such means be developed. In respect of the last-mentioned class of evidence I present here such a system for discussion and, hopefully, improvement. It is based not on theory but on actual specimens and practical issues encountered over several decades of work with such materials.

Essentially, I divide the task of assessing a purported very early engraving on bone and similar material into *three successive steps*, each leading to the next and eventually to an informed hypothesis of how the marking was created, and whether it can ultimately be expected to illuminate cognitive, intellectual or cultural variables. Typically one is presented with a bone specimen, or a series of specimens from the same occupation deposit, suggested to bear engravings. The first task must be to determine whether there are among these usually linear grooves markings that were made with either the point or the cutting edge of a stone tool. If there are no anthropic marks present, no further analytical effort is needed, unless one has resolved to also study natural markings. If there are, it needs to be established whether these stone tool markings are intentional or are the unintended by-product of a utilitarian process — which is often rather more difficult. In principle, the basic procedure is fairly similar to that of discriminating between petroglyphs and natural markings resembling them. It is one of systematic elimination of potential interpretations, i.e. of falsification (Bednarik 1994).

Morphological study

For the purpose of archaeological study, we first need to exclude the possibility that the marking we see on a piece of bone was not occasioned by any taphonomic event or process. Typical forms of taphonomic markings on bone are those occasioned by gastric acids of carnivores (especially hyenas), by mycorrhyzal action (solution by aqueous solution of carbon-dioxide exhaled by micro-organisms active on the surfaces of plant rootlets), by kinetic action within the sediment (solifluction and cryoturbation are common candidates), by water or gravitational transport (e.g. fluvial action in the presence of angular clasts, or transport within a scree talus), gnawing by animals (rodents or carnivores), kinetic effects from the burrowing of animals, effects of drying and heat, and by trampling within the uppermost layer of a sediment. The effects of all these and other natural agencies can often be individually identified by the specialist, but it is certainly not a task for the novice (e.g. Binford 1981; Bunn and Kroll 1986; Potts 1988: 88; Bednarik 1992b). Nor is it central to the present consideration, which is how to recognise marks made with lithic tools, most often those of flint or other chert.

The markings a stone tool creates on fresh ('green' or 'greasy') bone are quite different from those made on a dry and brittle bone surface. The former have a better rounded cross-section and are of smooth appearance, whereas in the latter case, the greater brittleness of the bone is reflected in jagged edges and coarse groove floors. Naturally one encounters intermediate forms, and recognising them provides useful information about the markings. Every stone tool point has a characteristic cross-section, which changes its form with any change in the direction it is drawn over the surface, as well as with its inclination relative to the bone surface, or with a change or direction of pressure applied to it. There are often tell-tale changes relative to features the tool point passes on the bone surface, such as barely perceptible ridges, pre-existing cracks or previous grooves it crosses over. These include the *stries parasites laterales, stries parasites finales* and *stries de sortie de l'outil* of d'Errico (1994: 27–32) and they will be particularly important for the *second step* in the analytical process attempted here, the 'internal analysis'. In some cases one can observe in the 'flow' of a groove where the tool was either suddenly or gradually turned. In rare instances there is evidence that a stone tool point crumbled under the pressure, recognisable by an impact-like mark and a complete change, perhaps a widening, of the groove section. Engraved lines often cross others, permitting the detection of features that indicate not only which marking was made first, but also the direction in which the second mark was executed. One of the most important features of stone tool grooves, on bone and other materials, is when a line follows a curvilinear course, because at curves the cross-section of the groove changes gradually if the tool is not turned relative to the direction of movement. If a marking is particularly well preserved one may find longitudinal striations on the floor of the groove, or where the pressure applied was adequate to leave behind slightly compressed spongy bone interior, the direction of its jaggedness always points in the direction of tool movement. These features are, however, much more frequently found on Upper Palaeolithic specimens than on older material, which is typically more corroded and weathered.

These are the typical variables one looks for, and this work always involves optical light microscopy. Often magnifications of 5× to 10× are quite adequate to observe the required details, but where surface preservation is particularly good, magnifications of up to 40× and even 80× can be of the greatest value. A number of alternative techniques have been used on more recent bone material, including scanning electron microscopy (d'Errico 1994) and microtopography with laser-scan microscopy (Steguweit 1999). Also, the topographical method Kitzler (2000) used on stone could be applied to grooves on bone, but while such detailed and laborious techniques have been shown to be most useful under specific and difficult conditions, or to illuminate more sophisticated issues than those usually required to determine the type of bone marking, they are rarely necessary. Optical microscopy at low to moderate magnification serves the purpose fully in most cases. Indeed, bone markings of the L./M.P. are almost always so heavily modified or weathered that more refined methods of observation were quite superfluous in most cases I have dealt with.

Internal analysis

This term was coined by Alexander Marshack to describe the microscopic study designed to determine various technological details of engravings, such as direction of tool application, handedness of operator and multiple tool application (Marshack 1972, 1985, 1989). Internal analysis has been very successfully used and tested by both d'Errico (1988, 1991, 1994) and myself (Bednarik 1984, 1988, 1992c). Whereas Marshack and d'Errico applied this form of analysis only to portable objects, I extended it to rock art, especially engravings and finger flutings in limestone caves. The principal difference between the work of Marshack and d'Errico is that the former researcher conducted little if any replicative work to underpin his theoretical constructs and pronouncements, relying essentially on visual judgment acquired from close familiarity with the materials. D'Errico has undertaken a great deal of experimental work in mark production and related fields. He also made extensive use of the scanning electron microscope, whereas Marshack and I only used optical microscopy. To examine striations and other details, d'Errico used not only the reflected-light microscope, he also created transparent cast replicas to study with a transmitted-light microscope, and resin casts to examine by scanning electron microscopy. He focused on incidental striae where tool aspects other than the principal point impacted upon a surface, and on changes in the pattern of contact, e.g. in response to variations in curvature of the engraved surface, or at points where the burin splintered.

D'Errico has shown through painstaking replicative studies that a good understanding of such minute diagnostic features in engraved lines, which can survive for many millennia on some surfaces, provides much reliable and testable information about the circumstances of mark pro-

duction. The repeated application of the same tool point can be identified with great confidence, and it is often possible to determine the direction of tool application. Precedence in superimposition is another easily resolved issue, on sufficiently well preserved specimens.

Marshack's internal analysis has become one of the most effective tools in the scientific interpretation of mark production processes, but I contend that the primary purpose for which it was initially conceived, the recognition of notational markings, is not served by it (d'Errico 1989, 1991; cf. Bednarik 1991). This is because, although one can most certainly recognise whether two engraved lines had been made by the same tool point, one can never determine *that they were not made* by the same tool (Bednarik 1991). While it is certainly possible to determine that two or more marks were made by the same tool point, it is impossible to know if two different tools were used in any two marks. Stone tool points are generally of non-symmetrical and irregular shape, so if the tool is put down briefly between applications, or if the grip or direction of application, even the angle of application, is changed between two marks, it may be impossible to recognise the repeated use of the same tool. Since a principal characteristic of notations is that a sequence of similar marks was made with interruptions, often using different tools, it follows that the issue cannot be resolved conclusively by this method: it cannot be demonstrated that two morphologically different markings could not have been made by the very same tool point.

Analysis of arrangement

Steps one and two have thus established whether the marks we examine were made with stone tools, and precisely *how* they were made. We have gained a great deal of insight into the physics and history of their production, but we may not yet be in a position of determining whether they were made deliberately, or are simply incidental, like the grooves on the oft-cited kitchen cutting board. If the arrangement is undeniably iconographic and not a fake or a recent intrusion into an older deposit, no further examination may be warranted. But so far we have only one possibly iconographic engraved motif from the L./M.P., one of the three Oldisleben specimens (Bednarik 2006). All other materials of this kind reported to date are merely arrangements or quasi-geometric compositions of linear grooves. It is therefore necessary to establish acceptable guidelines for recognising this thing 'intentionality' — itself a slippery concept in any scientific context — in a set of engraved grooves.

The fact that we have already established, as far as this is possible, in what direction the lines were made and possibly even in what sequence now becomes most helpful. I have divided the aspects that favour their interpretation as intentional engravings into the following groups: (A) those that suggest intentionality but do not demand it, because incidental utilitarian markings could conceivably be found with similar properties, however remote that may seem; (B) those that demonstrate intentionality because they cannot reasonably be expected to occur in utilitarian stone tool marks; and (C) those that are not intrinsic to the possibility of the markings being utilitarian. Importantly, the first two groups are of attributes whose strength is enhanced if it can be demonstrated that all or many of the individual marks of an arrangement have been made by the same stone tool:

A1. Fairly equal spacings between sub-parallel grooves;
A2. Individual marks of a set, such as sub-parallel grooves, were made in a spatial sequence (e.g. from left to right, suggesting right-handedness);
A3. Repeated shape or angling of the elements of a set of markings;
A4. Arrangement or orientation of the elements of a set of marks appears to reflect spatial attributes of the given surface geometry;
A5. A set of elements, such as straight grooves, is orientated so that its components point to or emphasise a common focus.
A6. Individual grooves exhibit consistent profile depth and margins over their full length, which reduces the probability that the tool cut through a secondary material, such as hide (Steguweit 1999).

B1. Individual elements of a set of marks are so arranged that their ends meet or 'connect';
B2. Some elements of a set or group of marks have been made by the repeated application of a tool, either deepening a groove or duplicating or extending it;
B3. The arrangement of the elements of a set of marks appears to have been marked out before execution, as if to secure equal spacing of a given number of marks;
B4. The arrangement of marks is accompanied by subsidiary markings suggesting that, before the principal grooves were produced, the maker hesitated or deliberated the placement of each groove.

C1. The graphic pattern evident in the arrangement has been previously recognised in another ancient find;
C2. The repeated use of the same stone tool point or edge is evident among the elements of a set of marks.

My factors B3 and B4 may in practice be hard to separate, and C1 would be expected to apply only in exceptional cases that would be of special interest to further research. Once any of the above factors have been detected, the arrangement of engraved marks becomes a candidate for deliberate execution. In a strictly scientific sense this cannot, however, be proved, rather it needs to be refuted that it is a non-intentional, fortuitous or utilitarian set of marks made with stone tools. As is usual in science, this can never be demonstrated in a finite sense, it can only be expressed in a formulation of relative plausibility. This, I propose, is achieved in the following manner.

Factors of the first group (A) are weaker evidence than those in the two other groups, because each attribute of the latter does, by itself, almost exclude the possibility that we are dealing with a fortuitous product. In my work, if one of the B or C factors occurs in combination with at least two other factors, I am satisfied that intentionality has been demonstrated. If only one of the A factors is present, but no other supporting evidence, I would consider the marking to be a candidate but one whose status as deliberate

engraving should not be accepted. Any ranking intermediate between these two would indicate a commensurate strength of the evidence. For instance, if any group B variable co-occurs with just one group A factor, the acceptance of intentionality would be justified, though not as decisively as if two group B factors were present in an arrangement.

This is a realistic way of judging the evidence. It is certainly superior to the ad hoc opinions we have so far seen dominate this field, and the variable standards of assessment all commentators, including I, have presented so far. It is also a sensible procedure because 'intentionality' (whatever this may mean in this context, or any other) in mark production must inevitably 'fade out' as we proceed back in time in human evolution, to a point where it was not present and had not yet been 'introduced'. The application of the system I propose here to material evidence of the kind being considered will, in my experience, find a gradual decrease in the number of relevant factors with increase in age of the evidence, until a point in time is reached where the above-listed factors will fade out entirely and there will be no evidence of deliberate marking production. This is what we have to expect to be the case, but what was needed was a system of criteria that expresses this state of the evidence in a *precise, rigorous and repeatable* fashion.

Summary

I have presented such a system here. It seeks to replace the long-standing practice of tailoring the defining criteria for purported L./M.P. engravings on bone to fit personal biases or established dogma. The uniformity I advocate in analysis and threshold of determination renders my criteria universally valid; they can be equally well applied to more recent materials, such as Upper Palaeolithic line markings on bone. As a matter of fact, it would be high time that the same standards of discrimination be applied to all such phenomena, irrespective of their age. What we know about the Pleistocene human past with any reasonable degree of reliability is, after all, very much less than what might be expected from a perusal of much of the literature. The simple truism is that our reliable knowledge of this is very limited indeed, but there is certainly no shortage of strongly advocated and defended *opinions*, many of which rest on a most inadequate scientific base.

My discriminating criteria can also be extended to Pleistocene line markings or engravings found on other materials, such as ivory, teeth, ostrich eggshell, amber, and of course stone. Naturally there are some differences in material properties and preservation characteristics to be taken into account, but the underlying issues are indeed universal. The perhaps most pressing need, however, was to determine discriminating criteria especially for line markings on bone of the L./M.P. periods, because these have in the past caused such extreme variations in the opinions of different scholars about the same objects. I hope to have contributed to ameliorating this unacceptable state of affairs in such an important area of research.

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