
On the Impossibility of Close Reading

The Case of Alexander Marshack

by James Elkins

Most disciplines concerned with visual artifacts assume that close reading is universally to be desired: the more accurately objects are described, the more responsibly they can be interpreted. Close reading is said to provide a check on inadvertent projection and inappropriate theorizing, and it is thought to be an ideal that is not specific to any discipline or kind of object. Without the concept of close reading, disciplines such as anthropology, art history, and archaeology would lose an essential interpretive anchor, since a routine or demonstrably misinformed interpretation could then be said to be better than one that attends more precisely to the peculiarities of the object. For these reasons, even though individual close readings may be taken to be flawed or biased and even though the critique of close readings is a central responsibility of any rival interpretation, the idea of close reading itself goes largely unanalyzed. Here I argue three things about close reading, using Alexander Marshack's analyses of prehistoric European artifacts as an example. First, there is no such thing as a truly close reading, because "levels" or "distances" from the artifact cannot be arranged and chosen at will. Second, the reading that seems closest in any given case is the one where it becomes most difficult to distinguish between properties inherent in the artifact and those that are part of the act of interpretation. Third, the closest readings (such as Marshack's) present the best opportunities to understand what a given discipline understands by basic terms such as image, notation, sign, mark, and decoration.

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Close reading of visual images is a constant ideal in art history, archaeology, anthropology, visual theory, and criticism of all sorts—it is virtually never questioned, and in general it seems to be a good idea in any field, for any purpose, and under any theoretical regimen. Its genealogy begins where art history does, with antiquarianism and connoisseurship in the 17th and 18th centuries, and ascends through 19th-century "Morellian methods" (that is, investigations of characteristic details in images), branching and proliferating in the 20th century into style analysis, formalism, and iconography. Carlo Ginzburg's speculative essay on Morelli, Freud, and Sherlock Holmes (Ginzburg 1980) remains the principal source for the history of the idea, though Ginzburg's concatenation of connoisseurship, psychoanalysis, art history, and detective novels could have been expanded by consideration of the way the same term is used in literary criticism—including Ginzburg's own essay, which becomes an example of the method it exposit.

It is questionable how much sense it makes to call such far-flung practices a "method," as Ginzburg does; instead I would say that the different methods used by Panofsky, Morelli, Freud, and others grow from the enabling concept of close reading. The transparent nature of the concept would seem to forestall any direct analysis or critique—whatever a close reading may be in practice, the concept of close reading remains nearly intangible. Accuracy and even insight are intuitively tied to the closeness of a reading, regardless of disputes about evidence or theory. The unobjectionable universality of close reading is most apparent in our inability to think what kind of visual artifact (or text, or crime scene, or narrative) might not be best understood by close examination. In short, the purposive, vigilant scrutiny of the particularities of the object seems to be a foundational principle of understanding, whether the object is the most recent installation art or the oldest markings on stone. After close reading, reading can relax, and there are many ways to interpret quickly, to make epitomes and outlines, to paraphrase and abbreviate, to schematize and summarize and condense and abstract: but each one of them is dependent on the possibility of a logically prior close reading.

When readings are questioned for their closeness or sloppiness, the purpose is usually to elicit the historians' covert agendas and to show how theory determines what and how we see. But that kind of critique has more to do with a local construction of theory and of visual structure than with the idea of closeness or close reading per se. It is possible to doubt that someone's reading is as close as it might be or to claim that it is skewed by prejudiced seeing and at the same time never begin to doubt close reading itself. Here I will be trying to open a critique of close reading, rather than the purposes to which it is put. In the end I will argue that in effect close reading does not exist, not least because any reading consists of echoes of vaguer accounts and suppressed promises of even closer readings. That which appears as close reading in innumerable texts—including Ginz-

burg's—is a moment of incomplete awareness, built on self-contradiction and the resurgent hope of intimacy with the object.

As an example I take the work of the archaeologist and art historian Alexander Marshack, in large part because I believe—though I will not be arguing this directly—that Marshack's methods make him one of the most important art historians of the century, one whose work deserves the attention not only of all archaeologists and art historians but also of anyone involved in the project of seeing and interpreting as finely as possible (Elkins 1993*b*). Marshack's questions are especially exemplary for art historians who are engaged in analyzing gestural brushstrokes, individual marks, color areas, *facture*, and other phenomena that are taken to be minimal components of pictorial meaning—lexemes, in the semiotic term—out of which the larger signifying units of pictures are built. Semiotic art history in particular needs to be clear about the place where “subsemiotic” marks leave off and signs begin (Elkins 1995*a*), and I will suggest that Marshack's analyses press that point as no others have done. Marshack's approach takes some introducing; after examining one of his analyses, I will outline the kinds of response he has received and sketch the reasons his work exemplifies the conceptual impossibility of close reading.

Marshack's Readings

Marshack's accounts of marks on Upper Paleolithic and Mesolithic bones, slate, and amber are among the most careful analyses in all of archaeology as well as in art history and criticism, visual theory, connoisseurship, and conservation. He looks closely, literally with a microscope, but unlike painting conservators he also looks at *every* mark on a surface or artifact, and his looking does not cease until he has satisfied himself that he has distinguished all intentional marks from unintentional or random marks, ordered the intentional marks in chronological sequence, distinguished directions in which marks were made, noted where tools were lifted from the surface and where they remained in contact, and determined how many tools or cutting edges were used to make the marks. His analyses are lessons in looking: forcible patient attempts to see *everything*, together with a concerted effort to conclude *nothing* that cannot be empirically demonstrated.

Marshack's central claim is that many Paleolithic markings—even tiny ones, at the threshold of unaided vision—are notations recording the phases of the moon. It is a hypothesis that might appear to be readily susceptible to various counterclaims, and in fact Marshack presents it as a reasoned scientific hypothesis of exactly the kind that should be open to empirical falsification. Yet that genuinely scientific attitude has gone largely unread, and scholars have been more struck by the persistence with which he has pursued the hypothesis over a period of more than 30 years. The archaeologist Denise Schmandt-Besserat, for example, has said that Marshack's theory of lunar records “cannot be proven or

disproven nor can it be ignored” (Schmandt-Besserat 1992:160). The art historian Whitney Davis dedicated a book on late prehistoric Egyptian art to Marshack, who he says “has been asking questions that I am not sure any archaeology or art history could answer” (Davis 1992:xvi). Another historian, Donald Preziosi, has recently used Marshack's research uncritically, as an example of the most interesting kinds of questions art history can ask (Preziosi 1989:133–42). (It is characteristic that these positive assessments are not accompanied by further close readings; Preziosi's account, for example, is devoid of the detail that supports Marshack's conclusions. He calls Marshack's work “emblematic of the poststructuralist critique of verbocentrist structuralism,” but he omits a discussion of the “detailed arguments” that would support the point [Preziosi 1989:141–42, 231 n. 34 and 44]. It is a typical effect of extremely close readings that they pass outside the normal protocols of reading and are necessarily glossed in more general accounts.)

It was praise like this that first attracted me to Marshack's writing, and what kept my attention was a strange dynamic that has repeated itself over the years as archaeologists have raised objections and Marshack has replied and gone on to make new, even more detailed analyses. The lunar hypothesis often seems patently unlikely, but Marshack's detractors seem to have no purchase on his methods and no power to overturn his conclusions. It seems easy to say that Marshack is seeing too much and trying, for example, to interpret careless decoration or meaningless tool-sharpening marks as deliberated calendrical notation. But when these and other problems are brought up, the result is almost invariably the same: the people who object are led to see that they have been operating with certain preconceptions about how accurate marks should be, or what decoration is, or how it is different from notation. What appear to be glaring mistakes in Marshack's method turn out to be unresolved aspects of our habits of seeing. The critics are silenced, in effect, by unanswerable questions: What should a Paleolithic image look like? What is decoration, and how does it differ from notation? Marshack's analyses, perhaps more than any others in the history of art, force these questions into the open and allow us to begin to see how we see and what we think images are.

In what follows I am going to do what has, I think significantly, seldom been done in the literature up to this point: I am going to make a reading of one of Marshack's readings that is closer than his own. Since the original text I will be analyzing is less than a full page (though it is interrupted by illustrations and occupies parts of four pages), I will be quoting it in full.¹

1. The figure numbers in brackets in my quotations replace figure numbers in parentheses in the original text; they are not interpolated references. I have redrawn the line drawings in Marshack's text. To avoid confusion, since I will be citing the text in its entirety, I have omitted page numbers. The discussion begins on p. 91 (Marshack 1991*b*) and ends on p. 94. Marshack's photographs have been retained here, though their numbers have been changed.

The Bâton in the Musée des Antiquités Nationales

In *The Roots of Civilization* (1972a), from which this example is taken, Marshack sets the stage for each new object with a brief narrative. In this case he has just discussed a large bâton made of bone, sculpted into a fox's head at one end, pierced with a hole, and engraved in rows of notches. The text continues:

In cabinet number one at the Musée des Antiquités Nationales, not far from the large bâton, lies a smaller, fragile, better-worked bit of bone, discolored a dead grey with time. It is the handle of a bâton, with the head and hole broken off, but with the arc of the hole visible at the break. It too comes from Le Placard, apparently from the Magdalenian IV, and may be a few years, a few hundred years, or a few thousand years later than the bâton with the head of a fox. Since it is a fragment, it has not been considered an important find. It is marked with what has been traditionally called decoration in a "geometric" style [fig. 1, top].

Marshack directs the reader to an overall shot of the front face of the bone (fig. 1, top). Then the reader is invited to move in on the object and review its markings more attentively (fig. 1, bottom):

The bâton has been worked to create four flattened sides, and on one of these there is a raised platform

on which markings were made. The handle itself is unbroken and the markings on it are complete. It is possible, then, that we have all the markings originally made.

The next paragraph announces the result of microscopic analysis. Virtually all of Marshack's studies are done with a microscope; he makes photomicrographs and photomicrographs and uses them to help modify and complete the notes he makes in the presence of the object. This particular paragraph is interrupted in a spectacular fashion, since the first sentence does not conclude on this page, and on turning the leaf the reader is confronted with a full-page, marginless illustration of a tiny portion of the bone, at a magnification of about 50 times (fig. 2). Marshack continues:

Microscopic analysis of the main face [fig. 1, bottom; fig. 3] revealed sets of parallel lines made by 10 different [engraving] points, with groups that seem to contain "symbols" and "signs."

As a first step in the microscopic analysis Marshack distinguishes several series of marks made by different burins:

At one place a mark at the end of a series of 14 lines, all made by a sharp, thin point, is crossed over by another point that scratched a flat floor with two small points at each side (V—V) [fig. 2].

The reference here is to the full-page photograph, which is enlarged so much that at first it is difficult to tell that

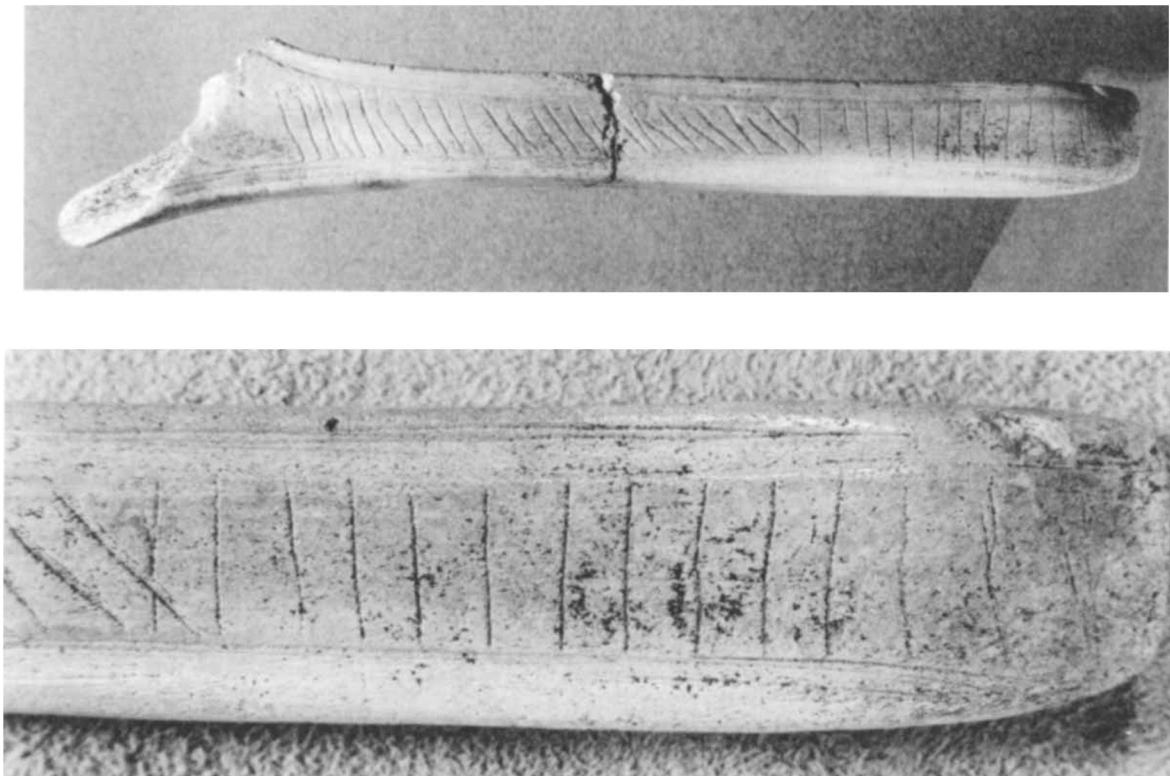


FIG. 1. Broken bâton from Le Placard, Charente, 7 1/4", Musée des Antiquités Nationales. Reprinted by permission from Marshack (1972a:91, figs. 21 and 22a), © Alexander Marshack.



FIG. 2. *Bâton from Le Placard, detail. Reprinted by permission from Marshack (1972a:92, fig. 23), © Alexander Marshack.*

the marks sloping up and to the left are a single stroke of the burin, leaving a double line and a corrugated "floor." The purpose of the large illustration is to make the point that different engraving tools were used—a crucial step in the argument that this is not "decoration in a 'geometric' style." The paragraph continues:

This broad stroke begins the next series. Since the broad stroke occurs over the fine stroke, it must have been made later, and the full sequence on this face begins at the right and reads to the left, towards the hole. The sequence begins with an odd, delicately scratched sign, visible under the microscope (see [fig. 3]):



The reference here is to yet another detail, this time of the righthand end of the bâton. This is the first moment

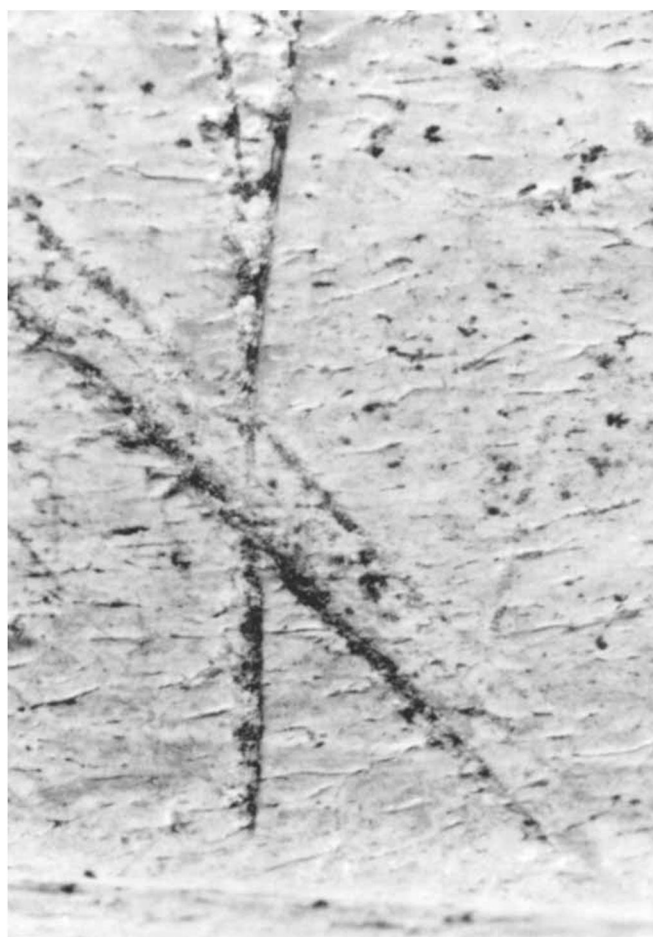


FIG. 3. *Bâton from Le Placard, detail. Reprinted by permission from Marshack (1972a:fig. 22b), © Alexander Marshack.*

that Marshack's reconstructions have to be taken for granted, since in my copy of *The Roots of Civilization* the best that can be discerned from the photograph is something like this:



The marks are, however, very indefinite in the photograph. The word "sign" does not appear in quotation marks this time, since the configuration is presented as the "odd" object that begins the first sequence; but by way of anticipating what he is about to do, he counts the nine component marks in the "odd, delicately scratched sign":

It is composed of 9 strokes. This is followed by a somewhat curved "Y" of 2 strokes, made by another point:

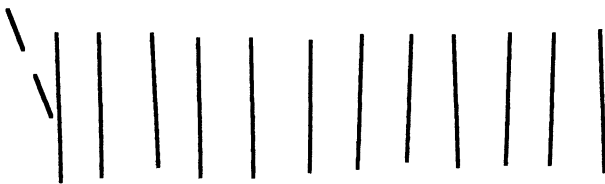


Again the marginal picture in his text implicitly instructs readers about the limits of their participation in the decipherment and about the limits of Marshack's accuracy. The "Y" in Marshack's photograph is clearly different, something more like this (fig. 3):



At this point it is apparent, as it would already have been for a reader who had followed *The Roots of Civilization* up to this point, that Marshack is relatively uninterested in the shape of the marks. I say relatively, since he is concerned with shape insofar as it affects relative position—whether a mark is right or left of another or above or below another in order of marking—and he is concerned with the shape of the floor of the mark itself. But the morphology of each mark is of less interest, because he is not going to be comparing configurations of overlapping or juxtaposed marks with other instances that might represent the same "sign" or "symbol." He continues:

This is followed by a series of 14 strokes, made by still another point; the last ends with 2 small appended strokes:

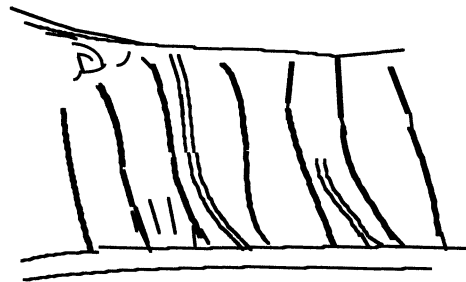


The regularity of the drawing of the 12 vertical strokes, as compared with the wobbly progress of the 12 marks as they appear in the accompanying photograph, makes it clear that Marshack's drawing presents what he calls elsewhere a "schematic rendition" rather than an exact rendering. He uses the phrase for line drawings that employ different symbols (such as X or +) to discriminate

between marks made with different burins, and he uses it in the captions that elucidate his proposed lunar sequences (Marshack 1991b:48, 193). This kind of linear schematization is purposely normative, and it is intended to make an underlying pattern visible. What it does in this example is erase whatever subtleties of arrangement might also have been meaningful in favor of a more or less regular parade of equal marks. Here, more clearly than before, the formal qualities of the bâton are being regimented in favor of the notational hypothesis.

The next series, made by another point, has 1 appended stroke at the end. The full sequence on this face, with the counts by changes and differences of point, is shown in [fig 4, top]. At the end of the series there are 10 strokes made by two points, in a 5 and 5 breakdown. Then, as though this fills out all the available notational space on this side of the bâton, over this, and between the last 5 long strokes, there are added 10 light strokes.

These last 10 strokes place further restrictions on the reader's participation in the analysis, since a look at figure 1, top, shows something entirely different. I would give it approximately like this:



The most important difference is that the two faint parallel lines in Marshack's drawing (fig. 4) seem clearly to be continued upward through the X that he draws above them, and there is a pair of very similar marks a little to the right that he nearly omits altogether. From the photograph it seems irrefutable that these two sets of parallel marks are made by single strokes and their apparent doubleness is merely the double floor created by the burin that made the marks in the middle portion of the bâton. Several dozen other marks can be discerned in the photograph, all of them fugitive and dependent on the quality of reproduction in the book; but the sets of parallel lines seem to be as dependable as any form that he reproduces in his sketch.

So again, in this case for the last time, the reader is warned away from too close a viewing or too close a reading of Marshack's text. It turns out that this last discrepancy is the most important:

All the marks on this face are intentional. It is obvious that this odd sequence of figures, counts, and groups is not ornamental or decorative, and that it is notational.

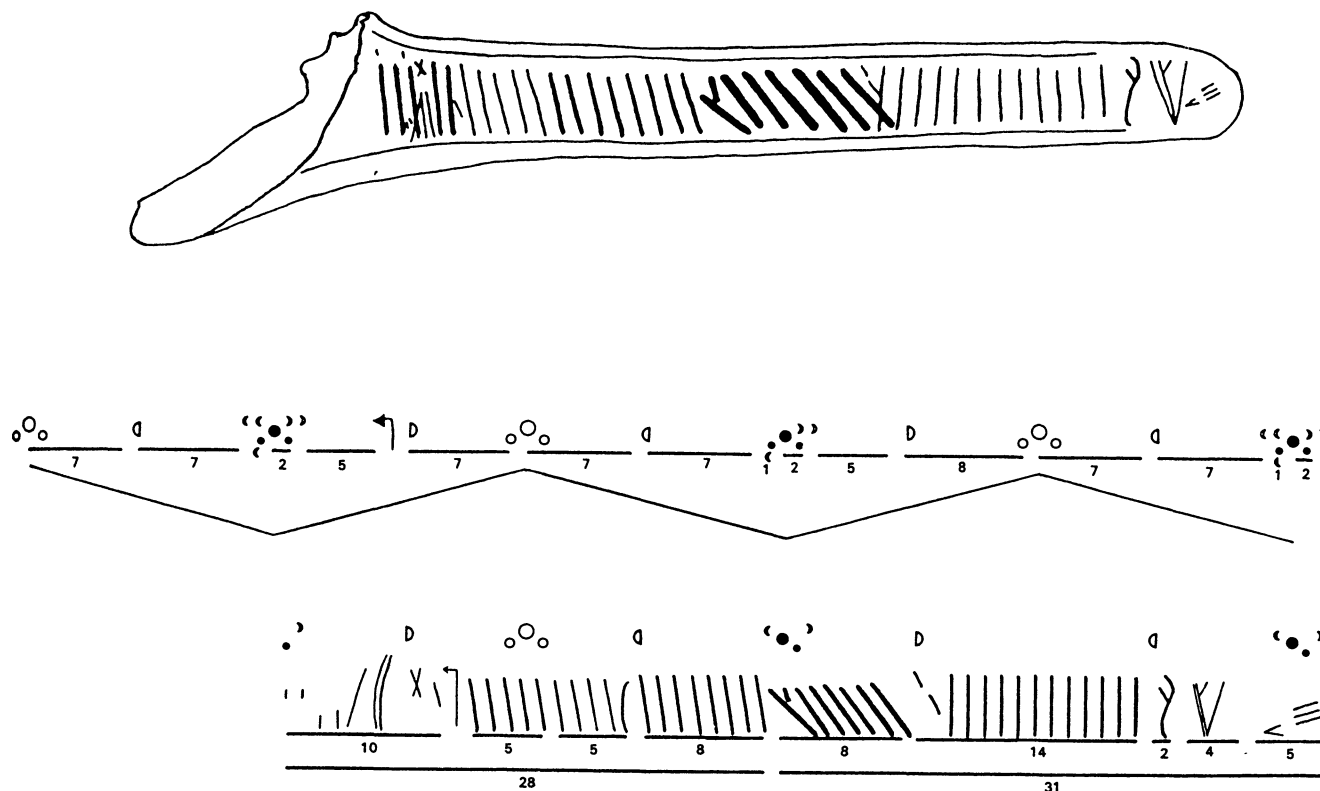


FIG. 4. *Bâton from Le Placard, schematic diagram and correlation with a lunar calendar. Reprinted by permission from Marshack (1972a:93, figs. 24, 25), © Alexander Marshack.*

Here as elsewhere in *The Roots of Civilization*, the “ornamental” and “decorative” are disjoined from the “notational”: nothing can be both decorative and notational. Decoration is imagined as an activity with its own agenda and rules that are incompatible with notation. The word “intentional” has a particular valence in Marshack’s writing, since he does not only mean it in a rigorous philosophic sense (as a mark interpreted to have been made with conscious purpose). He also means something like “careful,” since presumably a decorative treatment of this bâton would have shown evidence of being more sloppily and quickly done, with one burin, at one time. Again the status of the “signs,” “figures,” and “symbols” is in doubt, since there is an implication that the kind of care that went into making this notation is partly evidenced by the “signs”; but Marshack is about to ignore the signs in favor of merely counting their component marks:

Is it lunar? Assuming one stroke per day, we lay the sequence against our lunar model [fig. 4, middle and bottom].

The “lunar model” is the same throughout *The Roots of Civilization*, and it shows the moon’s phases along with a possible count of the days between them. Beneath it Marshack places his schematic rendition of the marks on the bâton. The assumption that one stroke equals one day is consistent throughout the book and never defended as anything more than a working hypothesis.

In order to analyze the notation, Marshack dissects both “signs” into component marks, one for each night’s appearance of the moon. He then concludes:

This is an absolutely perfect tally for 2 months, beginning at last crescent and ending on the first day of invisibility 58 days later.

We seem to have in this series a development of notational technique—recalling the accumulation of cueing marks of the Taï plaque [fig. 5]—in which the seeming “signs” or multiple marks, made by appended strokes of arms and groups of angles, are used to indicate points of change in lunar phase. How are they used? The lunar model reveals that on the day of an expected lunar observation around a phase point a small stroke is used, as though an observation of the moon was impossible on that day and the stroke represented a day of waiting. On the next clear day of observation the new series begins with a different stroking. Interesting also is the indication of a notational significance for the “quarter” moon, and the visible half-moon. None of this is difficult to understand. It is once more a visual notational technique. It is non-arithmetical and it is cumulative. Remember also that the bâton was engraved some 5,000 years before agriculture formally “began” in the Fertile Crescent of the Middle East and some 10,000 years before the formal “beginning” of writing.

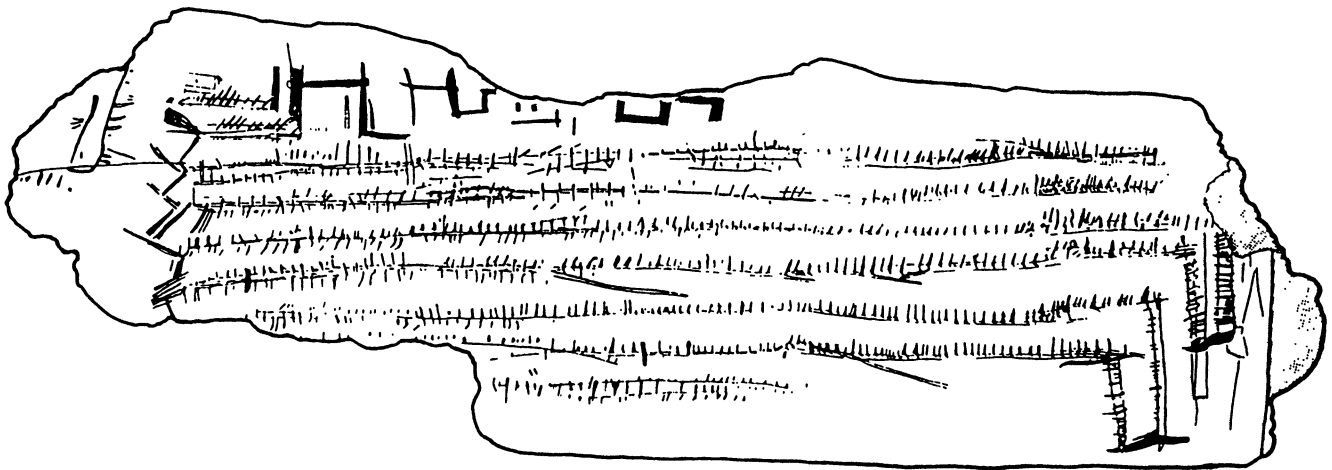


FIG. 5. Engraved fragment of a rib from the Grotte du Taï, schematic diagram. Reprinted by permission from Marshack (1991a:31, fig. 5), © Alexander Marshack.

The account of the bâton winds to a close with an invocation of two further engraved sequences, on the back and on one side:

On the reverse face of this bâton, scratched so faintly that even when held in hand most of the markings are invisible, yet clear under the microscope, is another unusual composition [fig. 6]. Once more there is certainty of notation, apparently for two lunar months. The lines, however, are so faintly scratched that a ballistic determination of point differences is difficult, and some exceedingly faint lines are difficult to prove.

This Magdalenian IV bâton reveals a further stage in the development of the notational technique and a possible development in the ease and precision of lunar observation and use.

A third style of marking, a development of that style of parallel stroking overengraved to make a crosshatching, occurs along one side of this bâton. It, too, gives a sum for two months.

The analysis ends in this way, with the puzzling “unusual composition” given a lunar phrasing of 27 + 30 days, and Marshack moves on to the next example.

Coming to Terms with the Lunar Theory

As Marshack himself has noted on several occasions, a first encounter with the “lunar theory” can leave readers incredulous. I want to try to capture some of that in order to exhibit the unexpected dynamic that leads from the reader’s certainty that Marshack is wrong to a nagging uncertainty about the act of interpretation itself. It is this motion, I think, that accounts for the idea that Marshack is unanswerable or that he cannot be disproved. Each of the important difficulties with his theory can be written as a problem of distinguishing among the mobile lexicon of the rudiments of visual images: marks, icons, signs, schemata, notations, symbols, and representations. Over the course of his career Marshack has explored each of these (Marshack 1976, 1977, 1979a, 1991c; Kobyliński and Kobylińska 1979; Gunn 1979; Elkins 1995b), and they continue to be actively discussed in the field of Paleolithic archaeology; in this context I will mention just three.

1. *The problem of signs.* An initial difficulty, well exemplified in the analysis of the Le Placard bâton, is that the “signs” and “symbols” have no role in the final lunar tally: instead they are picked apart into individual

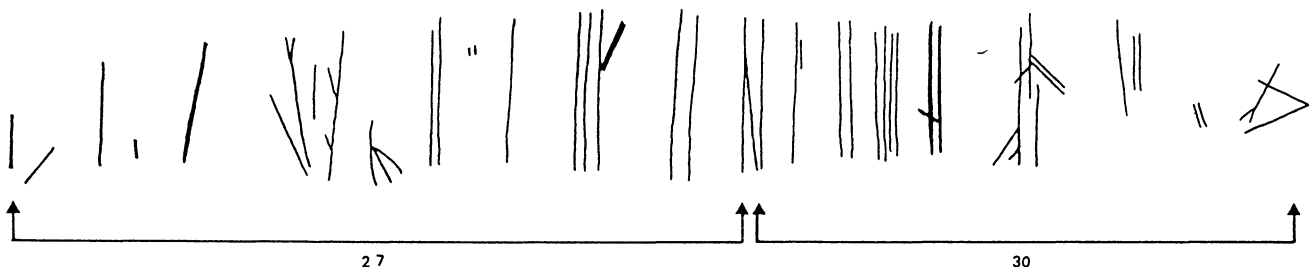


FIG. 6. Bâton from Le Placard, schematic diagram of back. Reprinted by permission from Marshack (1972a:94), © Alexander Marshack.

marks, and each mark is made equal to each other mark. At first that seems patently wrong: Why would a person arrange marks for appearances of the moon (or for any other phenomenon) into a carefully made, apparently intentional configuration if all that was intended was a sequence of equal marks? It does not make sense, on the face of it, to propose that the meaning of the Placard bâton is a series of single marks when it consists of collected and arranged "signs" of various types. The reasoning seems no more defensible than if Marshack had taken apart a print by Albrecht Dürer into its individual lines.

The apparent thoughtlessness of the analysis only seems worse when we consider what Marshack might reply. A reader of *The Roots of Civilization*, for example, might expect him to say that he is merely following good scientific procedure and taking one hypothesis as far as it will go. Marshack himself points out the "signs" and their arrangements—the first is called an "odd, delicately scratched sign"—before he goes on with an analysis that does not take them into account. The problem here, if we are to judge by the criteria of a scientific hypothesis, is not that he chooses to ignore the "signs," because no scientific model need account for all phenomena presented by its sample—no close reading must be exhaustive. The "signs" have no meaning in the lunar schema, since both "signs" at the right of the bâton fall in the continuous period between the new moon and the first quarter. They may be meaningless, or they may have other meanings; Marshack could claim with perfect reason that they are not immediately under consideration.

But that does not justify the way he excludes the "signs." Marshack takes note of the relative positions of marks in two particular senses: he records how they cross over or under one another, and he is interested in the sequence of marks, in this case from the handle of the bâton to its working end. As several readers have pointed out, he does not consider relative positions of marks in any other sense—say, their configuration when they comprise a "sign," or their distances from one another in a straight-line sequence (Lewis-Williams and Dowson 1989:48). The "signs" are read as collections of individual marks made from right to left. But positionality is what defines the shapes of the "signs," and so it does not seem logically sufficient to disassemble them into their component segments without justification. In addition, Marshack does sometimes interpret position in a wider sense. He notes the "appended strokes of arms and groups of angles" of some marks and shows how they fit the lunar sequence. A "small stroke," he suggests, is used "on the day of an unexpected lunar observation around a phase point." These observations have to do with the height, faintness, and spatial positions of marks. Why do they matter in these instances and not in the case of the "signs"?

At this point it may seem that a critic would have a firm purchase on the analysis, both because marks are defined so oddly (in regard to some positions and ignoring others) and also because the definition seems self-

contradictory (since it variably excludes and includes spatial criteria such as size). If size and depth and overlap of mark can matter, then why not also marks that just touch, as in the "signs"? But there are deeper problems that stall this kind of objection. Marshack can appeal to the specificity of the artifact: who is to say the marks were not made in a way that entails exactly this definition of position? After all, we are considering a period before writing and before signs; perhaps the configurations of marks were meaningless to their makers in precisely these ways (White 1982). Everything is recent, and nothing is native, where Paleolithic artifacts are concerned: there is no cultural link, no shared tradition of meaning or marking. An intractable aporia is produced by the act of identifying and then declining to read "signs," since it must ultimately lead us to question the distinction between sign and mark, position-as-sequence and position-as-configuration.

It is also the case that Marshack *does* interpret "signs" and other sets of nonnotational marks in other notational contexts. His longest analysis—and one of the most intense and minute accounts of the semiotic elements of any visual object—concerns a 4-inch bone fragment from the Grotte du Taï scored with hundreds of tiny nicks and grooves (fig. 5). On that artifact Marshack finds notational marks—that is, the little hatches that appear here as vertical lines—and horizontal "containing lines" that serve to anchor the individual marks. But he also mentions "'non-notational' directional cueing marks" that help define the "direction and 'flow'" of the notation (the small empty boxes at the extreme lower right), "an initiating, 'non-notational' symbolic motif," perhaps representing "flow and motion," along the upper margin, and "exceedingly fine engraved 'hair' lines" that "seem to mark the relevance of a day or period, but are distinct from the sequential accumulation of day units" (Marshack 1991a:28, 33, 47, 50, 60).

Analytically, the most interesting moments in the account of the Taï plaque concern three further nonnotational forms in the object's upper-left corner, each one microscopic. The first is "a tiny horizontal containing line with 5 marks." In figure 7 it can be found to the right of the letter *B* and just inside the enclosure of heavy black lines. Another is "a tiny short sequence of 8 to 12 strokes between parallel lines (a 'ladder')," visible immediately to the right of the "tiny horizontal containing line." Both of these, and especially the first, would be difficult to distinguish from the plaque's other containing lines and notational marks, but Marshack says they were "clearly not part of the central notation" (Marshack 1991a:47). In the far upper-left corner, above the letter *A*, is a minuscule rectilinear meander "form" with six marks inside and three more attached to one corner—a configuration similar to the "signs" that Marshack declined to interpret and determined to count in the Placard bâton. Here the configuration is treated as a "form" that may have "positional relevance" but is again not integral to the notation and therefore need not be counted. But why not? Because it is so much smaller than the other marks? But if so, then what determines

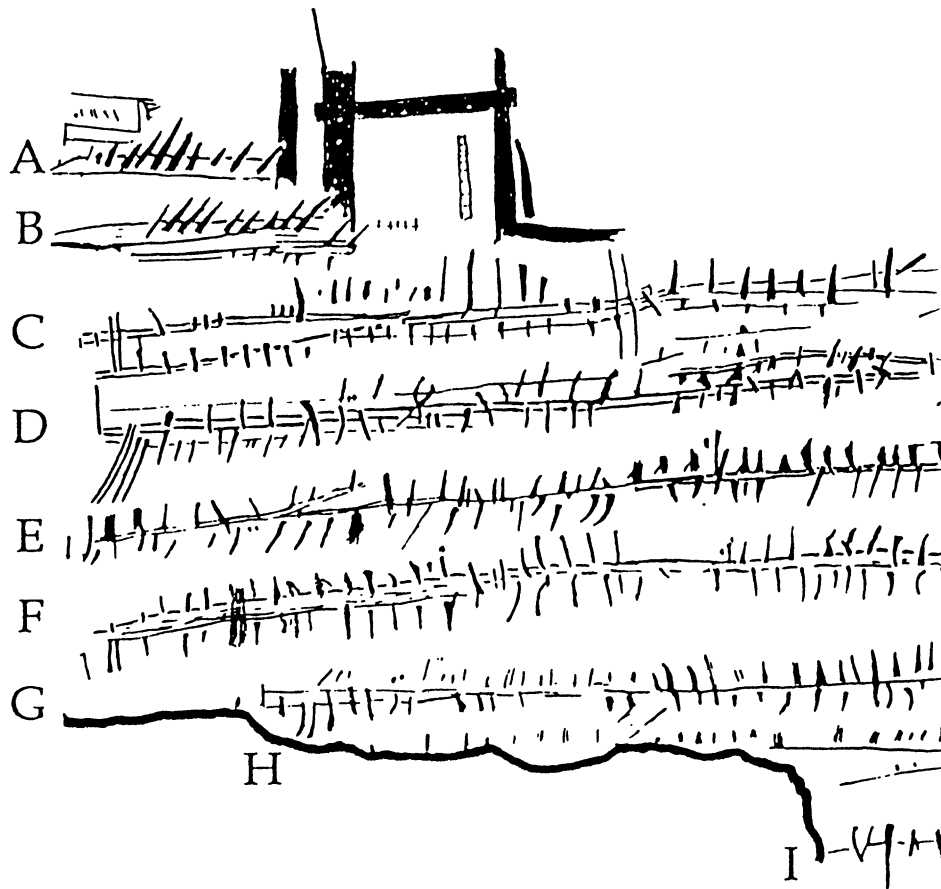


FIG. 7. Detail of the extreme upper-left corner of the rib fragment from the Grotte du Taï, schematic diagram. Reprinted by permission from Marshack (1991a:46, fig. 17), © Alexander Marshack.

the threshold of size beyond which a mark is no longer part of a notational sequence? The meander shape is also indistinguishably similar to the “non-notational” direction cueing marks” at the lower right of the artifact, and the notational marks often miss their containing lines as these marks do. By what criterion is this not part of the sequence?

Before I draw a conclusion from these questions, it is worth noting that the answer cannot be historical. It is not sufficient to argue that “signs” of the type on the bâton are not likely to have meaning until the Neolithic, because at some point artifacts of the same age as the bâton will have had to have been analyzed and found deficient in “signs.” Conversely, when representational signs occur, for example, the plant or “arrow” signs at Lascaux, their interpretation depends on assumptions about the *internal* rules of such images.

Marshack offers only a quick look at the back of the Placard bâton, but if we linger we may begin to see the very “signs” and “pictures” that play such a large role later in *The Roots of Civilization*, where they are interpreted as “seasonal,” “time-factored” images of plants. The little “sign” that looks like a bird’s foot on the back of the bâton is quite similar to a sign at Lascaux that Marshack reads as a “simplified branch,” in opposition to Leroi-Gourhan’s interpretation of it as a schematic

“feminine” vulva sign or the Abbé Breuil’s reading of it as an arrow (Marshack 1972a:223–24):



Meaningless
“sign” on the
Placard bâton



Branch in
Lascaux

Two configurations on the bâton that look like stalks (seventh and ninth from the left) are virtually identical to signs Marshack calls “plants” and “sprouts” in a later chapter (Marshack 1972a:172, 216):



“Meaningless”
configurations on
the Placard bâton



“Plant” on a
bâton from Cueto
de la Mina

Marshack considers the problems of similar signs when it comes to Lascaux, since in that context he wants to show that plant signs on mobiliary artifacts might be close enough to some signs at Lascaux to imply that they should be reinterpreted as plants (Marshack 1971a: 224). He does not reason that way about the Placard bâton because it is too early, but that does not explain why it is inappropriate to consider such possibilities in an analysis that presents itself as internal to the object (Marshack 1979b:303).

To return to the questions about when "signs" should be read: once again—and I am only taking isolated examples from a far larger field of possible issues—the objection about the status of "signs" on the Taï plaque seems cogent, but it can only succeed in raising the issue of assumptions. The "tiny horizontal containing line with 5 marks" at the upper left of the Taï artifact is not read as notation, while the nine lines of the "odd, delicately scratched sign" at the right of the Placard bâton are read as notation. Each reading devolves from different assumptions about the possible relations of signs to individual marks. But what determines when marks are "executed differently" or may "represent a different category of marking"—as he says in another context, justifying the nonnotational character of a set of marks—and therefore require a differing interpretation (Marshack 1991a:59)? Certainly the relation of one mark to others is essential in any assessment, but how is it possible to determine when the "position within the frame" is different *enough* to cross the border between mark and sign? If the small rectangle at the far upper-left of the Taï plaque were slightly larger, or if its marks were closer to their frame, would it be six or nine notational marks instead of a single "form"? Any and all objections of this kind, which are the only objections that are cogent within the terms of the text, must appear as rival hypotheses, and in that capacity they return to the questioner in the form of counterquestions about the reader's axioms concerning the nature, relation, and disposition of signs and marks.

2. *The problem of notation.* The lunar sequences have bothered readers of *The Roots of Civilization* a little each time they are shown, because the fit and the principles of correspondence between marks and model differ with each succeeding example. Absolute accuracy is impossible and therefore meaningless, because as Marshack indicates the lunar cycle does not factor evenly into days, and measurement without instruments is always a matter of guesswork—the crescent moon cannot even be seen earlier than 15 hours after it is new, and there is a corresponding problem judging the hour of fullness (Minnaert 1954). But the absence of an absolute standard does not mean that relative accuracy cannot be measured or that the principles of posited correspondence cannot be compared.

Responding to Marshack's analyses, Arnold R. Pilling has objected that Marshack had failed to provide evidence that "any two objects have on them the *same* notational system" (Pilling 1972; Littauer 1974:328). Ev-

ery calendar is the same, and all astronomers agree on the moon's phases; hence it seems improbable that a "continentwide system of notation" would not show more self-consistency (Fry 1972). The assumption here is that notation of a single phenomenon involves detectably uniform representation. But that is our own notion of notation, and it need not fit the Paleolithic one. In what sense are the lunar phases a single phenomenon, aside from our certainty that they are? To some readers, Marshack's analyses seem to be mathematically indefensible since he accepts so many sequences as lunar cycles (Lewis-Williams and Dowson 1989:49). By modern standards of evidence, he is playing with numerological coincidences rather than requiring determinate ranges of fit—there is no mention of the usual scientific criteria for probabilistic matching, such as combined systematic and statistical error, quantified normative models, or standard deviations. In terms of scientific method, there are often more marks in a given artifact that do not correspond to meaningful phase changes than marks that do and more inconsistencies than parallels. Statistically speaking, Marshack's hypotheses for objects such as the Placard bâton are a bad fit. Intermittently throughout *The Roots of Civilization* Marshack says that he tried many different correlations and picked the best one, but the reader is left with the impression that others might be as good. In general, he claims to have done a great deal of empirical testing, which he has consistently not reported in his texts. There can be no doubt that he does perform such tests. In one passage, for example, a colleague mentions "mathematical, statistical, graphic, and sequential analysis of the results of . . . microscopic studies" (Movius 1972). Still, in scientific terms these are serious, even crippling omissions. Marshack's principal attempt to redress that again omits the discarded hypotheses and accounts of methodology that are standard in laboratory research (Marshack 1991a). "Tests can be tried, within a range of possibilities, by anyone interested," he remarks at one point (Marshack 1972a:86). That is not entirely an interesting prospect, since if I were to try to rematch the lunar sequence I would be re-proving his hypothesis. Any rematch would be another instance of the same theory. There is no way to disprove the theory by adjusting the lunar sequence, and so the positioning comes to seem somewhat arbitrary. A reader of *The Roots of Civilization* will tend, I think, to find each sequence about as plausible as every other—a scientifically untenable situation.

But to all these questions Marshack could respond by asking what model, and what criteria, might be preferred in analyzing notations that are prearithmetical: the supposition is that they were made before there was such a thing as counting (see Marshack 1974b:265; 1985; 1988b). Wittgenstein thought of this problem, I think correctly, as an instance of the more general question of understanding when someone is following a rule. At one point in *Remarks on the Foundation of Mathematics*, he even imagines a "cave-man" who makes surprisingly ordered marks for no clear reason (1983:344):

There might be a cave-man who produced *regular* sequences of marks for himself. He amused himself, e.g., by drawing on the wall of the cave:

— — — — —

or

— — — — —

But he is not following the general expression of a rule. And when we say that he acts in a regular way that is not because we can form such and [such an] expression.

For the present purposes, it is safe to ignore the phrase “he amused himself,” since Wittgenstein probably meant it as a reminder that the cave-man’s intentions and language are inaccessible. Wittgenstein also imagines a thrush that “always repeats the same phrase several times in a song.” Are we to conclude that “perhaps it gives itself a rule each time, and then follows the rule?” (Wittgenstein 1983:345). The stories go to show that regular actions need not follow regular rules and that private “amusements” do not have rules in the ordinary sense. In the case of actual Paleolithic notations, there is no mathematical standard available to use as a criterion of adequacy, since marks give no evidence of the marker’s conception of lunar phases—or, to say it a little less exactly, the marks *are* the marker’s conception of notation. Should any notation be uniform, if notation itself was in its infancy? Should any fit be optimal, if the idea of representation itself was still inchoate?

In this way cogent objections about Marshack’s lack of statistical rigor get turned into questions about the universality of quantification, numeration, and rigor itself. There have also been objections to Marshack’s reliance on tiny marks, at the threshold of invisibility (Littauer 1974:328). But why assume that the notations were referred to later, or that they could be read? Perhaps these notations were kinaesthetic: their meaning, as we would put it, was in their making. In that case any size that the burin could scratch would count—and as a corollary, the engraver might be indifferent to the size of the marks within the limits of the register lines or the available surface. Our thoughts about clarity and legibility—concepts that loom large in current texts on graphic communication—depend on the notion of notation as something that can be efficiently read: but why assume it should be read at all?

Marshack is emphatic about the difference between his claim to have found notation and his hypothesis of the lunar calendar: “Of the hundreds of incised stones throughout Europe that I have studied in the last quarter of a century . . . not one has provided evidence for a linear, sequential ‘lunar’ notation” (Marshack 1989). If the claim and the hypothesis are kept separate, then the inadequacy of scientific objections to the lunar calendar becomes more obvious. The lunar calendar is susceptible to a number of critiques about methodology and falsification, but each of them has to appeal to a prior sense of notation. And what do we know about notation that

might serve as a basis to argue against Marshack? Under what circumstances, with what evidence and what rules of interpretation, can we critique his assumptions?

3. *The problem of decoration.* If “decoration” could be kept in quotation marks (as it often is in the recent literature) or defined negatively as nonutilitarian, non-symbolic, or nonnotational marking (Marshack 1989), then there would be no special problem. But Marshack’s work seems to provoke the question of decoration because he sees so little of it in comparison with what other researchers would see. An artifact such as the pebble from Barma Grande (fig. 8) might seem like a formal experiment: perhaps the engraver was matching and contrasting lines with the shape of the pebble and then quickly filling in blank areas with hatches and cross-hatches. It may look like what we expect of decoration; at any rate it does not correspond well to the structure of Marshack’s examples of Paleolithic notation, which are more one-dimensional even when the register lines snake back and forth. Yet in the first edition of *The Roots of Civilization* (1972a) he interprets it as notation, in which every mark has notational value. He surmises that the longer parallel lines were intended “to give the engraver an enclosed or separate notational area”—that is, a one-dimensional track—and therefore to serve as a visual and kinaesthetic aid” (Marshack 1972a:84). He starts reading with the large curving set on the obverse (fig. 8, top), “since this central form or figure seems to take up the prime space on this face.” It is also possible, however, that the “angle-marks at top and bottom, serving almost as edge-marks,” might have been made first. The two choices are based on two ways of approaching the surface: the person doing the marking began either by taking charge of the blank surface or by keeping to the edge. These are assumptions based partly on kinaesthetic hunches about ways of coming to terms with blank surfaces and partly on assumptions about the ways images might be structured. The analysis moves forward with the discovery that every right-facing hatch mark within these register lines was made before every left-facing hatch mark; and so Marshack continues and eventually counts every line on the pebble. In the revised edition of *The Roots of Civilization* (Marshack 1991b) the Barma Grande pebble is omitted, but the question of reading developed two-dimensional patterns as notations remains (Marshack 1991a:148, 158, 216, 248, 254, 434, and especially Marshack 1995a). Under what circumstances, and with what criteria, could a more or less uniformly marked surface be interpreted as a sequence of individual notational marks?

This more fully utilized surface shows what kinds of problems can be involved in determining sequences; Marshack generally says little about the trial-and-error process of finding the best order of reading for a surface thoroughly scored in two dimensions. Upper Magdalenian images exhibit “the ability to synthesize and compose” a “sum of images” into “storied” surfaces—in other words, to make meaningful narrative sequences—although the order of reading for such “non-linear se-

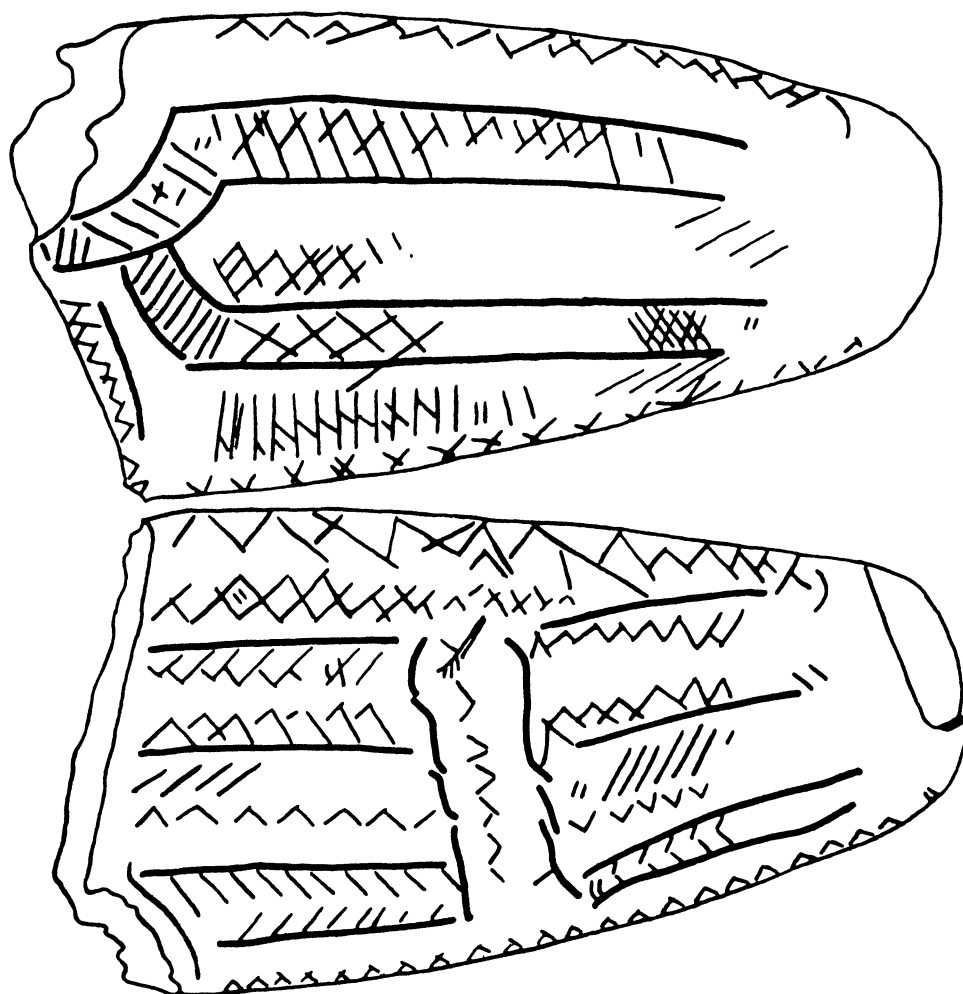


FIG. 8. Engraved pebble from Barma Grande, Grimaldi, schematic diagrams. Reprinted by permission from Marshack (1972a:83, fig. 16a, b), © Alexander Marshack.

ries" is generally indeterminate (Marshack 1972a:192, 211). What he calls, in this context, "decoration" is something distinct from a notation, but the criteria of its difference are derived from very different kinds of artifacts. On occasion Marshack reads notations by extending linear, one-dimensional configurations into two-dimensional multiples whose organizational laws cannot be derived from the one-dimensional cases. But what exactly are images in the general case, if not extensions of linear marks and linear accumulations of marks? And exactly how are images made, if not in this way?

It has seemed to some archaeologists that Marshack does not recognize decoration or allow for random, careless, and meaningless marking (e.g., d'Errico 1993a). In several papers he has described about two dozen "symbol systems" including meanders, "fishlike images" (which resemble lattices or nets), zigzags, and double lines (Marshack 1979a). But it can appear that he goes too far and enlists the majority of decorations in the ranks of notations. Considering a bâton from Mezherich

in Ukraine, which is engraved with several sets of parallel hatch marks, Marshack opts for notation over decoration even though the structure of the marking differs from the normative containing line and crossing hatch mark (fig. 9). It is relatively neutral to say the bâton has "an accumulation of sets of marks incised at different angles," with "unstructured, subsidiary marks at the far left." But the purpose of putting it that way is to emphasize the deliberation and internal structure of the marking: without actually counting the marks, he concludes that the bâton "represents a personal and idiosyncratic style of accumulating *sets* of marks." It may represent "a mnemonic accumulation for a ritual or myth, or a record of days and events," but either way, "the internal structure . . . suggests that this composition is not 'decorative'" (Marshack 1987:149, translation his;² 1988a:

2. "une accumulation de séries de marques gravées selon des angles différents . . . marques auxiliaires non structurées à l'extrême gauche . . . représente un style personnel et idiosyncratique d'accumulation de *séries* de marques. . . . une accumulation mnémonique

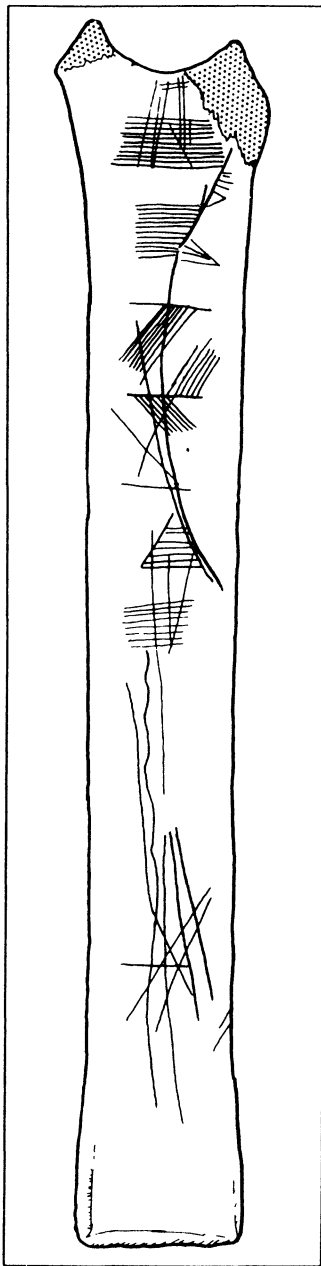


FIG. 9. *Bâton from Mezherich, Ukraine, mammoth ivory, 19.7 cm. Reprinted by permission from Marshack (1987:150, fig. 14), © Alexander Marshack.*

427; 1994:387). To others, the markings on the bâton might appear sufficiently loose and unstructured as to be classifiable as nonnotational, if not intentionally "decorative." "It seems that practically nothing is random," one observer complains, "and very little is decorative" (Lynch 1972). Others have objected that "more evolved ornamentation" can be "just as intentional, cu-

mulative, and sequential" as some artifacts Marshack presents as notation (Vencl 1972). But where, Marshack might ask, do we get our ideas about the nature and frequency of decoration? Why can't notation be the norm and decoration the uncommon counterexample?

Marshack thinks that a "complex sequence of events, microscopically determined, eliminates the possibility of decorative intent or 'art'" (Marshack 1972b:474). But why would he not see the Mezherich bâton as a less carefully made artifact? Like the bâton from Le Placard, it looks as if it might have been the work of a few minutes and therefore, by our standards, likely to be "un-thinking" nonsemiotic marking or loose decoration (Davis 1989). I can imagine decorating a strip between two register lines by making right-sloping marks after left-facing marks as on the Barma Grande pebble or, as on the Mezherich bâton, by making a few sets of hatches, adding a register line, and moving on to the next set. But these are sliding scales: it is difficult not to see decoration as a kind of careless or immediate activity, as opposed to notation, and it is even more difficult to conceive notation as a potentially careless, quick, or relatively unstructured act. Decoration seems to entail a "regular design" made "rapidly and simply," and notation is somehow its opposite (d'Errico 1994:10).

In the critical literature, the question of the difference between notation and decoration has focused on Marshack's claim to find frequent changes in marking tools within a single surface, making it more likely that the surfaces were marked in a deliberated, "time-factored" way. But that is a slippery point; the technical evidence for those claims is not clear, and the performative aspect of notation and decoration is too variable to make the division stick. A hand might change grip on a tool in the course of a few centimeters of marks; and a person recording a notation might put down a few marks quickly and carelessly and then make the next few marks slowly and with a different tool (Marshack 1972b:474). Francesco d'Errico and others have taken Marshack to task for his ways of distinguishing burins and his arguments about the intervals between sequences of marks. Since d'Errico uses an electron microscope whereas Marshack uses an optical one, d'Errico's readings are physically closer, and he has been extracting different kinds of empirical data. For a while it seemed as if the problem of Paleolithic notation would come down to the atomic scale in its search for dependable criteria. D'Errico claims, for example, to have identified the autographic marks of a single burin, so that he could tell definitively when a tool was reused or when, as Marshack claims, one tool was allegedly exchanged for another in the course of a protracted series of temporal notations (d'Errico 1989a). But I would propose that the problem lies ultimately on an entirely different level: the concepts of decoration and notation are in some disarray, and they need to be addressed before any technical inquiries can hope to make sense. D'Errico, for instance, uses a provisional tripartite classification of notations: those in which "the elements constituting the system differ from one another," those in which "the elements

pour un rituel ou un mythe, ou la consignation de jours et d'événements . . . la structure interne . . . suggère que la surface n'est pas 'décorative.'"

are identical but can be differentiated by their distribution on the surface," and those in which "the marks cannot be differentiated" because they are identical and equidistant (d'Errico 1994:10–11). But these criteria apply to any number of "decorative" objects, and in particular they apply, respectively, to the bâton from Le Placard, the Mezherich bâton, and the Barma Grande pebble. By d'Errico's working classification, each of them could be a notation. The question is not the analytical criteria for judging changes of tools or the passage of time or the slowness of the marking: it is the prior problem of the disheveled concepts that prompt those questions to begin with.

The Incoherence of Close Reading

These examples are enough to show the wonderful power that Marshack's readings have to turn confident criticism into rooted doubt, throwing our assumptions about images, marks, signs, notation, and decoration back at us instead of silently serving those assumptions. Excessively close readings can also begin to look lunatic or wrongheaded, and they may say more about the historian's notions of images than about the artifacts under study. (That at least is my sense of Marshack's work, since its oddity strikes me forcibly as a 20th-century quality. I find I have a firm sense of Marshack's use of these artifacts and virtually no idea about their original meanings.) A feeling of mild pathology hangs over all close readings, from Sherlock Holmes to Panofsky, from Morelli to Marshack—and along with it there is a double attraction and slight repulsion at the activity itself. It is too much, and yet it is never enough.

Since my initial strategy was to show how a reading could be made more closely than Marshack's, it may seem as if I would advocate the closest possible reading—that is, the closest reading that could still seem to make sense—in any given case. Certainly that move effectively uproots assumptions and shifts the conversation from disputes about facts or methodology to the underlying conceptual base. But there is something seriously wrong with advocating closer reading, or closest reading, even aside from the fact that it might not be meaningful or helpful in a given interpretive discourse. In the study of visual artifacts it is tacitly assumed that readings, both close and far, are matters of choice. Each serves particular interpretive ends, whether they are the ones the interpreter might have described or not. In each instance there is the moment when the anthropologist, archaeologist, or art historian declines to read a given mark, or to read it as a certain kind of mark, and then the argument can proceed only on the agreement that the semiotic system functions as it is imputed to do. The "closest" reading would depend on how the community of interpreters perceived the disposition of meaningless marks and meaningful signs and how it chose to understand "mark" and "sign." That seems reasonable, and often enough it is true in practice, but it also harbors another assumption: that there are levels

of reading that find meaning in successively smaller signifying units. One archaeologist might only care that the Placard bâton is a bâton and that it was found near another, similar one. Another might be interested in its size and shape in relation to comparable examples. A third might notice the "decoration" on the handle. A fourth might be drawn to the structure of the "decoration" (hatch marks between register lines). Marshack notices each mark, and I have suggested that there may even be more to see. The notion of levels of meaning is as clear here as it ever gets: oil painting is a morass by comparison, a single slurred continuous line from the name of the entire painting to the stray hairs of the paintbrush embedded in the paint. In both instances, the point of unsurpassable closeness is a distance where the lenses of the interpretive machine can no longer focus, where for the purposes of the reading there are no finer marks, no further signs to be read. It is the place, in a semiotic account, where the lexeme (the meaningful unit, such as brushmarks that form a tree) gives way to the morpheme (the structurally defined but meaningless unit, such as a brushmark), or the character gives way to the mark, or—in the general case—the semiotic meets the non- or subsemiotic. Whatever the medium or the terms of the analysis, the hypothesis of various levels of reading culminating and terminating in a closest possible approach is necessary to make sense of the idea of close reading.

All three of these ideas—levels, free choice among the levels, and a final level—can be questioned. And here I want to switch disciplines, because some of the most interesting work on close reading has been done not in anthropology or art history but in literary criticism. The critic Paul de Man's essays on poets, philosophers, and other critics are presented as exemplary close readings: those who follow his arguments carefully can be left with the impression that nothing could be closer. But in each case there remains a certain measurable distance between de Man's critical strategies and the texts he exposit. The distance is not an external one that might be measured by contrasting de Man's versions of texts with other accounts but an internal one that appears between his version of texts and alternative versions that can be read in those same accounts. In other words, there is at times a primary text irregularly represented in his writing that is more nuanced and articulate than the text as it is formally presented.

An excellent article by Henry Staten raises this question in regard to de Man's reading of Nietzsche's *Birth of Tragedy* (Staten 1990). Staten observes that a hierarchy of degrees of closeness is implicit in the terms of de Man's reading of Nietzsche. De Man, Staten says, views Nietzsche "from a middle distance," and Staten proposes to move in closer in order to discern "fissures . . . between unities of meaning," such as Nietzsche's apparently unified "Apollonian" or "Dionysian." Each distance has its own ideological force (it is a construction that can be shown to be utilized for a given purpose), although the meaning of the degree of closeness that de Man employs remains invisible to de Man himself for

the duration of his reading. Other readings are suppressed by the text in a way that could be recovered, but in Staten's reading de Man's particular "distance" is what de Man himself would call a moment of necessary blindness. It has to do, Staten thinks, with de Man's past and his "cold" refusal to listen to the "'ecstatic call' of the 'Dionysian bird'" (Staten 1990:37)—but however we may choose to explain it, Staten's reading demonstrates a way in which closer readings can be half-hidden in any account, and it locates a moment of blindness over the exact place that is understood as the closest approach to whatever text is under consideration. I think much the same could be said of accounts of images, and I would suggest that one of the reasons Marshack's readings have proved so difficult to critique is that blindness concentrates where the reading seems closest. In reading Marshack's interpretations, I have deliberately made use of his own photographs in order to suggest that closer readings lurk in the text itself ("text" here taken as the sum of photographs, diagrams, and writing in the book). The photographs are evidence of variant readings, still unperformed, and they combine with the interpretation he gives to create a tension between his own interpretation, presented as the closest meaningful response to the artifacts, and something closer that might yet be given words. Marshack's blindness is especially clear in a passage where he asserts that his work does not "read in" meaning (1974a:330):

Categorizing a sequence as a "tally" or, more descriptively, as a possible hunting or gaming tally or even as a mnemonic device is not a form of scientific analysis. It is a form of analogic gaming, based on the matching of certain gross visual similarities. Intensive methodological analysis of each sequence, on the other hand, provides a body of internal data not requiring ethnographic comparisons.

It may seem that the problem here has to do with placing the border between "internal" and external data, so that everything inside inheres in the object and is "scientific" and immune to analogy and everything outside depends on unstable analogies (Marshack 1992a:182). But that border in turn depends on the location of the closest reading. The significant phrase here is "gross visual similarities": the distinction between reading and reading in is determined in advance by assumptions regarding what is gross and what is fine, what is vague and what is intensive. In other words, proper "scientific" interpretation is set in motion by an invisible decision regarding the location and nature of the finest available data.

When close reading itself is at stake, as it is in different ways for Ginzburg and de Man, a writer's distance will necessarily be repressed rather than openly declared as it seems to be, even though—and, in de Manian terms, *because*—other positions remain legible in the text. This could be put—a little extravagantly, but in the spirit of de Man's paradoxes—as the impossibility of close reading, since it goes to show that close reading is the name of a method that posits a number of dis-

tances from a work but does so in order to be able to remain blind to the one distance that is finally adopted. Marshack never addresses the appropriateness of his chosen level of close reading, except by saying that he studied every significant mark; in de Man's terms, he *cannot* address the question without writing an account that would be incompatible with the one he presents.

This helps explain the occasionally incoherent and unhelpful literature that is generated in the wake of concerted close reading: their analytic disjunction from the work at hand may best be assigned to the way that unimprovably close looking seems to be located within a field of blindness. The author of the close reading relies on the notion of levels or distances of reading and suppresses both the existence of closer readings and also the assumptions about closeness that inform his chosen distance. Marshack is resourceful in describing his reading but less so in uncovering the assumptions that make it possible; and in a similar way, his critics have usually been quick to find alternative readings but hard-pressed to say why their readings should be preferred to his.

A close, or closest, reading cannot succeed in erasing signs of closer *meaningful* readings (that is, not just closer looks at meaningless marks), and it cannot fully understand or exposit its own position. It may seem that this is an acceptable aporia, because it is also the description of a very adventurous place, fraught with risk and potential reward: having brought the critique forward along a series of known readings, the writer inhabits a foggy place where everything seems "closest" and there is no more room to move. Eventually someone else may find a way to assess the account and push forward again. I put it as a dramatic scenario to bring out its suspicious rationality. It is a picture of interpretation that models itself on progress or exploration—both of them suspect ideals. A deeper critique, made by de Man himself in other texts, demonstrates that this last hope of close reading is also misguided.

At the beginning of *Allegories of Reading*, de Man sets out what amounts to a proof of the impossibility of unimpeded motion toward or away from the object (1979:4):

On the one hand, literature cannot merely be received as a definite unit of referential meaning that can be decoded without leaving a residue. The code is unusually conspicuous, complex, and enigmatic; it attracts an inordinate amount of attention to itself, and this attention has to acquire the rigor of a method. [Hence] literature breeds its own formalism

On the other hand—and this is the real mystery—no literary formalism, no matter how accurate and enriching in its analytic powers, is ever allowed to come into being without seeming reductive

A "naïve" reading of literature, which understands it as a mimetic project, is always at odds with the critical impulse, with its increasingly strict method. The dialogue between the two kinds of reading creates a state of affairs in which there are persistent attempts to recon-

cile the two into a general theory of reading. De Man resists that, and remains skeptical of "false models and metaphors" about the "inside and outside" of literary representation. Barbara Johnson calls this "rigorous unreliability": the more rigorous a close reading becomes, the less reliable it is revealed to be (Johnson 1984:285). There is no simple path from one side to the other: reading itself changes en route, and it passes through the "conspicuous, complex, and enigmatic" acts of decoding and forgetting that make "method" possible. In a characteristic move, de Man understands this injunction in terms of his own theory about reading: the more rigorous he becomes about the "reconciliation" of the two kinds of reading, the less reliable his text becomes. The two are linked in a single motion—hence Johnson's phrase "rigorous unreliability."

It is a commonplace in art history, archaeology, and other disciplines concerned with visual images that any reading can become as close as it wants, its only limit being the texture of the work itself: I can claim that any mark is significant if I can make a persuasive case that it is a mark at all. In this way the limits of an ultimately close reading seem effectively only a matter of optics (What can we see when we get that close?), propriety (Is it appropriate to look at Monet through a magnifying glass?), or analytic coherence (Can we assign meanings to every gestural mark?). These working notions share a common figure in which the interpreter moves in by easy, gradual steps, through a known landscape, to a limit imposed by what are taken as semiotic requirements or by what I have called "propriety" or "optics." Much of contemporary art history and archaeology still believes in the possibility of adjudicating, in a reliable and rigorous manner, between "internal, formal, private structures" and "external, referential, and public effects." De Man's critique takes away the last hope that a close reading has of controlling itself: it cannot succeed in repressing closer readings, it cannot understand its own position, it cannot know how to move between positions—and it cannot avoid appearing "unreliable" in relation to more relaxed, "naïve" or general reading. And de Man's interpretation also suggests why close reading has to seem strange, especially when it is most entrancing: and unsurpassed close reading is an absolute refusal to negotiate the terms of reading. It is "reductive," as de Man says, and therefore also stubborn, intractable, obstinate, and difficult: all qualities that Marshack's readers have felt when they have tried to come to terms with his work.

Conclusions, with a Speculation on the Pathology of Close Readings

There are many more things that could be said about close reading, and I might mention several of them before I bring these thoughts to their conclusion. It would be interesting, for example, to see what ideas other theories might bring to bear on habits of close reading and to compare protocols of reading in disciplines such as religious exegesis, psychoanalysis, and medicine (all of

which have highly developed lexica to monitor interpretive accuracy). Since my concern has been visual artifacts, I have not even opened the question of the particular meanings close reading has when its object is linguistic. I have said next to nothing about the history of close readings from Greek literary criticism to the present, even though it is often relevant to the suppositions of my own approach. Nor have I addressed the difficult correspondences between words such as "decoration," "notation," and "sign" and the terms of visual semiotics, including "sign," "mark," "trace," and their linguistic counterparts (Elkins 1995a). Given the almost unencompassable range of the ideal of close reading in activities as different as detective novels and surgery, it would also be important to explore how the problems I have described occur in disciplines other than archaeology, in order to see what properties might unite different senses of close reading. But I have omitted these questions, among others, in order to concentrate on what I take to be irreducible conditions of close reading in visual artifacts. To conclude, I want to bring together a few pragmatic comments on the question of Paleolithic artifacts and then end with some wider speculations on why close readings remain so rare and so strange.

In terms of the kinds of artifacts I have mentioned here, close readings are the best places to begin to ask what is meant by the most fundamental terms that are used to describe the images. Such an inquiry would have the virtue of turning our conversations inward long enough to see what we expect in any given case of the central terms in our discourse about images. In Marshack's work, a provisional list might include the following assumptions about notation, some of them still unexamined in the literature:

1. Notation is made up of marks disposed in a one-dimensional manner, either in a straight line, in rows, in curving lines, along an edge, or between register lines. If a surface is notational and is entirely covered with marks, it will break down into one-dimensional sequences.

2. Notation is "intentional," meaning that each mark is carefully and deliberately made. Sloppy or haphazard markings are more likely to be decorative.

3. Decoration and notation are mutually exclusive; a marked artifact may be either decoration or notation but not both. When a configuration of marks is "structural and sequential," the "laws of probability argue against . . . an 'artistic' intention" (Marshack 1991a:36). Decoration and "organized designs" do not usually show "unrhythmic crowding" of lines (Marshack 1991a:51).

4. Notations are made over time, with measured pauses between marks (they are "time-factored," in Marshack's phrase). Marks may be made all at once, but if they are notational they will represent an accumulation of time intervals that would stand for longer intervals. Conversely, marks made over time are likely to be notation (Marshack 1972a:169).

5. Notational sequences show "internal irregularities" or divide into sets (Marshack 1972a:86). Conversely, entirely uniform marks are likely to be decoration.

6. If a set of marks is a notation, every mark is signifi-

cant and must be counted. No rule of form or location or strength of mark can exempt a mark from being counted.

7. Notation does not make use of signs or symbols made of sets of marks. If a configuration is a notation, compound marks that appear to be signs or symbols need to be dissected into their individual marks (Marshack 1991b:39).

8. The shape of a mark is generally not significant; it can be any size. (The exceptions occur where smaller marks correspond to phase points in the lunar sequence.) Conversely, a shaped or representational mark will not normally be notational (Marshack 1972a:230–31).

9. The orientation of a mark is generally not significant; it can be horizontal or diagonal in a sequence that is primary vertical. Marks can be “feet” or complementary crosshatching added to other marks that have already been laid down (Marshack 1991b:149).

10. The position of a mark on a surface is generally not significant; a fully marked surface can sometimes be read by determining the order of marks, sometimes by trial and error (Marshack 1972a:212).

These are the kinds of assumptions that need to be addressed first, or along with, more practical or conventional interpretations if we are to increase our awareness of our own place in history and the expectations we have of artifacts (or, to say it the other way around, the meanings we “find” in them). That purpose is not always cogent, especially when interpretive communities are already posing and answering interesting questions; but when they become stalled with insoluble claims, as they have in Marshack’s case, these questions can dissolve some of the obstacles to further dialogue.

It may seem that this has been the overly close examination of an obsessively close reader, but the ideas involved are widely applicable. Marshack’s method is not an eccentric or marginal regimen but the vigilant application of a central methodology of any discipline that works with what it takes to be a structured, “systematic” artifact of any kind, from Paleolithic artifacts to Neoexpressionist canvases and from graphs to literary texts (Elkins 1995c). Marshack does what we all say we should do, but he does more of it, and in many respects he does it better: and for that reason the difficulty that people have experienced trying to argue with him needs to be taken seriously.

One of the joys of Marshack’s work is that it shows just how loosely we read the more commonplace images that occupy most of anthropology and art history. Anthropologists interpret cultural artifacts in accord with assumptions about decoration that allow them to get on with the business of writing without stopping to count every welt in a scarified body or every bead in a necklace. The best way to reveal those kinds of decisions is to contrast them with other interpretations based on different concepts of adequate or close reading. Scarifications and beadwork, after all, are “time-factored”: each bead takes a moment to string, and each piercing hurts. An interpretation that dissected a tattoo or a piece of jewelry into moments and gestures that fine might well appear as lunatic as Marshack’s lunar calendars

have sometimes seemed: but that response disguises a serious problem—a hollowness in the concept of interpretation itself. The same is true in my own discipline. Art historians and critics pay little attention to groupings, types, sequences, and series of marks and virtually none to individual marks. Our smallest units tend to be images in their own right (depicted figures, portraits, objects, symbols), and when it comes to individual marks, as it does for example in Pollock’s work, we generally prefer theorizing about their nature to studying them. All these are signs of what we overlook in order to preserve a certain working notion of close reading. By Marshack’s standards, our seeing is blurred and cursory, and our standards of looking are indefensibly lax. In literary criticism, where the concepts I have been exploring are most fully developed, the blindnesses of close readings affect the theory of close reading itself. Ginzburg (1980) is fully aware of his attraction to close reading in detective fiction, art history, and psychoanalysis but only partly aware of the reasons he is so attracted. His books replay that attraction by applying the method to various subjects, rather than delving into the critical question of the place of his writing within the tradition and the limits of self-reflexivity in any given instance. Like art historians, anthropologists, and archaeologists, Ginzburg prefers close readings over looser ones, and he sometimes becomes interested in the history of close reading itself—but that interest does not intrude on his chosen regimens of close reading, which are unimpeded by any potentially disruptive critical reflection.

Close reading is structurally pathological in relation to other modes of reading. Earlier I suggested that is a result of its position at the far end of a domain of ways of reading whose other extreme embraces “naïve” decoding without—as de Man says—interpretive “residue.” A less exact way of saying that would be to point out that most of our readings are more or less—mostly more—lazy. Most of us want to escape the tyranny of prolonged close engagement with the artifacts or with our writing. Taking more ordinary encounters with objects as the standard, readings like Marshack’s (or Morelli’s or Panofsky’s) are apt to seem a little strange. Yet stiflingly close reading is an imperative of humanist scholarship and of literacy more generally: it has to be possible in order for there to be such a thing as a text, or an image, to understand; but it also has to be impossible in order to let us get on with the vaguer meanings we all prefer.

Comments

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A preliminary awkwardness—which it would not be useful to pursue here as such but which I must declare

because it will color my comment—is that I am used to taking “close reading” in a different sense from that stipulated by Elkins. (The exemplary close reading, for me, is the literary criticism of William Empson in *Seven Types of Ambiguity*.) What I think of as close reading is not penetration to a collocation of smallest signifying units and could never entertain a reductive distinction between notation and ornament of the kind ascribed to Alexander Marshack, and it has nothing to do with interpretive rigour or a semiotic hierarchy of closeness of the kind described with reference to Paul de Man. So my first problem is that the close reader of Elkins’s pathology is certainly not a straw man but a construct designed to bring out some logical consequences entailed in a “constant” but “unanalyzed” “ideal” that I do not think I share.

This said, and turning to the sense of “close reading” he stipulates, it seems, first, that this is a scrutiny of the object in which the lowest-level signs possible are distinguished and assembled into signifying combinations and, second, that the purpose of such scrutiny is to check on the correctness of interpretation. The medium of analysis is semiotic and the project hermeneutic. My problem here is that, even leaving aside the intricate issue of whether hermeneutic and semiotic can couple productively at all, semiotics seems to me inadequate to the interest (or meaning) of the artifacts I must deal with as an art critic. I need not argue this here, since Elkins himself, in “Nonsemiotic Elements in Pictures” (1995a), has excellently described the problems raised by semiotic readings of visual images of the kind addressed by art historians—say, for purposes of common visualization, any painting by Rembrandt. As he says in that paper, I think, to accommodate the properties that make the pictures worth scrutiny—the way they are painted, on the “level” of the individual mark at one extreme and the total character of our experience of a picture at the other—either the notion of the sign must be expanded and diversified à la Charles Peirce to a point where I doubt whether the specific interpretive thrust of “sign” is any longer doing much work or an inelegant apparatus of sub- and suprasemiotic accessories has to be cobbled together to cover precisely what is *proper* to such painting. Either recourse puts the notion of sign under such pressure that if it had an authentic theoretical status it would have to yield place.

I suppose it is uncontroversial that a valid reading should be adequate to low-level orders. But actually, even though an appeal to low-level coherence may satisfy an important hermeneutic canon, I think that close reading typically serves a heuristic purpose rather than a hermeneutic one. The close reading is then local and selective rather than overall or complete—and not a random trial bore but the purposeful following of a vein. What it fixates is the pregnant detail in which larger orders are implicit or by which larger orders are controlled.

I realize that the “complete” reading Elkins is referring to is not a critical program. It is a potentiality entailed by a certain sense of the structure of the object.

But what this ideal complete reading would seem to me inadequate to is the nature of the visual act, which is determining in different ways for both the maker and the beholder of pictures. This is as much a sequential process as the making and marking of one of Alexander Marshack’s bâtons. But it too is intentionally shaped—aimed and framed, focusing and discriminate, opportunistic and elliptical, restlessly conjectural and diverted by the informatively different, dismissive of external redundancy but itself repetitious in its elected scanning path, only weakly retentive of pure visual data and quick to bank economically categorized objects instead—and so on. (Perhaps this is the “lazy” reading Elkins feels we will and should eventually revert to; I reckon it is proper reading of visual art.) What is the theoretical status of a procedure that vision is not even theoretically capable of? And what is the status of the signifying structure that entails it?

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To speak, as Elkins does, of “close reading” is to employ an odd trope, for what does it mean to be *close* enough to a visual image to *read* it properly? How, it is worth asking, are the concerns associated with vision and reading to be combined when dealing with visual art? Standing too far away from something, you cannot *see* its details properly. Getting too close, you will not be able to view the whole object at once. To view it adequately, then, an observer needs to find just the right distance—neither too close nor too far away. What, finally, have these ways of describing visual experience to do with *reading*? A close reader is someone who sees important details which others who are less observant are too far away to view. But a reader who gets too close, focusing on minor points, is unable to get what, speaking metaphorically, we call “the big picture.”

These ways of talking about “close reading” seem to draw on our everyday experience of understanding other people. When I am close to *a person*, then I know without much need for questions how she or he feels. But perhaps I am too close to intimate friends to judge them objectively. Certainly it is hard to be objective about myself, for I am too close to myself to view myself as others see me. Understanding someone, like doing close reading of a visual artwork, requires that there be some distance between us.

Because I have read the literature on Nicolas Poussin (1594–1665), I feel close enough to his works to have a sense of what kinds of interpretations are reasonable (Carrier 1993). His culture is distant, but through reading I bring it close, bridging the gap between his world and mine. Because I have known the abstract painter Sean Scully (1945–) for 16 years, I feel close enough to him to have good intuitions about how to describe his art (Carrier 1994:245–67). We are close enough for me to give reliable close readings of his work. The anthropo-

logical examples discussed by Elkins differ from these cases. When the interpreter has in hand only an object, then determining what marks or other properties of that object deserve analysis seems hard. Lacking any independent written evidence about how these objects were understood by their creators, understanding what counts as a close reading of objects from very distant cultures is very difficult. If no contemporary commentary on the artwork is available, what can the historian do except look as closely as possible at the object itself? But who is to say if that interpreter is too close to be objective? And if there is debate about what features of the object in question are relevant, how can we settle a conflict of interpretations?

The art historian who has explored these issues in the most far-reaching way, Richard Shiff, makes the contrast between iconic and indexical features of paintings which captures the essential logic of this situation: "At one moment, as we respond to the iconic function, we may feel that a painting signifies whatever it resembles and gives some account of it; this is to respond to its iconic function. At the next moment, as we respond to the indexical function, we may feel that a painting signifies the hand that made it, giving us information about the person who motivated that hand" (Shiff 1989:68). Indexically, he suggests, characterizes modernism. Perhaps, then, Marshack should be identified as a modernist art historian. This identification of his work may explain why his work has attracted attention, but it does not, of course, answer Elkins's questions about how to evaluate it.

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Two comments on Elkins's excellent article—the first frankly amateurish, the second a bit more firmly grounded:

A reader from the humanities inevitably falls to wondering how Marshack understands the relations between the notational activities he is trying to reconstruct and those we call "language." Having given oneself the regulation smack on the wrist for logocentrism, one still wants to know: In what possible world of Paleolithic human doings does Marshack set his calendrical mark making? Ultimately I think the answers he gives to this question are grist for Elkins's mill. First of all, he freely admits that he does *have* a speculative picture of Paleolithic cultures and capacities and that the picture in some sense drove his inquiry: "These concepts [of the pattern of skills and potentials into which notation fits] are important, for they were part of the intuitive insights that allowed me to proceed with the research" (Marshack 1972a:118). But do not the concepts themselves, as Marshack presents them, run counter to his effort to draw firm lines between notation, decoration, and "sign"? The striking feature of Marshack's speculative Paleolithic—the way it differs from previous wis-

dom—is surely its wish to add into the picture of human practice many more dimensions than language, tool making, and control of fire. Language "was an adjunct and a product of the increasingly complex, widening, time-factored, time-factoring capacity and potential of the hominine brain and its culture." Communication—for instance, the range of words and gestures that he believes came into being to mark pain, fear, frustration, desire, etc.—rests on "a common awareness that was essentially time-factored and non-verbal, referring to processes and relations with a large measure of emotional, kinesthetic, or mimetic understanding." I do not see how this general picture of Paleolithic development tallies with Marshack's effort to separate out notation from, say, "kinesthetic or mimetic" kinds of mark making. Or at least the general picture seems to me to suggest the sort of insecure boundaries between mark-as-metonym and mark-as-metaphor which I take Elkins to be arguing for. And in the end I do not feel too abashed at suggesting that the key question may remain that of verbal language. If we situate the markings Marshack is describing already within some kind of linguistic field, in which a shifting between registers of representation is increasingly made possible—a shifting from memory to prediction to inference to punctual insistence on the present state of things, between "emotional, kinesthetic, or mimetic understandings"—then surely our whole picture of the mark marker's sense of the *range* of things a pattern of marks might do (might do concurrently) is turned in Elkins's direction.

I much admire Elkins's presentation of de Man, but I am not quite sure whether he believes that de Man himself would have claimed to be doing "close reading." I doubt that he would. I think he saw "closeness" as one of a family of terms, which will never go away in epistemology, that wishes or imagines a final level of *material visibility* for the act of signification. Other such terms, which de Man confronts directly, always in a sceptical spirit, are "grammar," "inscription," "semiotic," and *signifiant/signifié*. As Elkins knows well, the main drift of de Man's later work is to argue against these forms of "scientific" materialism in favor of a truly "mechanistic" (and therefore figural) one. "Reading" in de Man's view is neither close nor distant, or always close *and* distant, as well as literal and figurative, semantic and rhetorical, summary and exhaustive, paratactic and periodic, rigorous and lazy, commonsensical and forced—and all the hundred other metaphors with which we try, as readers, to get hold of the nothing or everything (the unknowable, the unjustifiable) we do. In reading reading, says de Man, "we look for the delicate area where the thematic, semantic field and the rhetorical structures begin to interfere with each other" (de Man 1993:200). You could almost put "notational" for "semantic" here, and "decorative" for "rhetorical." Almost, but not quite, since it is time and again de Man's chief point that the distinction between the "decorative" and the "mimetic" in rhetoric is impossible to draw. This is the real scandal for close reading. The "decorative"—meaning the mere figure of speech, the

mere trading on the accidental possibilities of the medium—is also the moment, in signification, at which “notation” becomes “mimesis”—only apparently, of course, only delusively, but irresistibly. And in any case, the logical status of the “onlies” here is precisely what reading puts in doubt. The implications of all this for Marshack’s key distinctions are ominous. Elkins spells them out with a wonderful lack of postmodern gloating.

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This paper seems to have two parts that need not fit together: a useful analysis of some of Marshack’s work and an attempt to put that analysis into a theoretical framework of other art criticism. I have no knowledge of the internal debates in art criticism; my remarks may bear on the theoretical issue, but for me the important thing is the evaluation of Marshack’s work.

We are indebted to Elkins for his summary of Marshack’s assertions about notation, which exposes some of the difficulties in evaluating his descriptions and interpretations. These difficulties highlight the importance of some framework for distinguishing notation, decoration, “unintentional” marking, and marking that has some other function. As I understand it, Marshack would be comfortable with a claim that microscopic examination is essential for establishing (1) the sequence of mark making and (2) the use of one or more tools (although these are not truly independent observations). I agree. Without what Elkins is calling “close reading” there can be no useful observations. Such observations are fundamental for establishing intent, but they are not sufficient. The issue that Elkins and others have grappled with is what other observations establish intent. What is appropriate close reading, however, is more open to dispute. Marshack has occasionally heaped scorn on observations that do not derive from the use of a microscope, as though observations were somewhat better with magnification. Does this mean that an electron microscope yields better results than an optical one? I think not. How close is close enough seems to be an issue but not one that helps us understand anything more about the importance of Marshack’s work.

First and foremost, Marshack has made us all aware that close reading is necessary. What marks “mean” and whether they were made intentionally can in some instances be resolved by examining the forms of the marks and the surfaces themselves. Thus, I have argued that careful examination of marked bones from Bilzingsleben, Pech de l’Azé, and Haua Fteah shows that they have been gnawed or heavily eroded (perhaps by stomach acids) or both (Davidson 1990, 1991). This is visible with either no magnification or that of a hand lens, and therefore closer reading is unlikely to reveal anything about their meaning. These taphonomic circumstances seem to have priority in any interpretation of the speci-

mens, and, as with the pierced bones from La Quina (Chase 1990), there is no good case here for early symbolism. Closer reading will not get us closer to understanding.

Marshack has also made us aware that notation may be an important issue. I am far from thinking that he has established that it was going on. Nevertheless, should his interpretation be correct, it is part of the history of codification that begins with language origins and goes on to writing, printing, computer discs, and electronic mail. Before the message was carried in a cleft stick, the stick itself carried the message. Did notation happen as Marshack says, and did the messages have anything to do with the moon? We may never know, but the answer will surely come from better thinking rather than better looking, from closer argument rather than closer reading.

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Many archaeologists and art historians will be surprised to hear from Elkins that Marshack’s analytic methods—Marshack’s (e.g., 1984) usual term for his procedures—are “among the most careful . . . in all of archaeology as well as in art history and criticism, visual theory, connoisseurship, and conservation.” By “careful analysis,” Elkins seems to mean what he goes on to consider as “close reading,” though clearly there can be careful analysis that is neither close nor reading. But if we accept his equation and his sophisticated elaboration, then possibly he is right. To assess it, we need wider views of both Marshack’s and Paul de Man’s work than the ones Elkins frames.

Among archaeologists, Lewis R. Binford’s scrutiny of the archaeological landscape tries to disentangle what I have elsewhere called nonintentional, zero-degree intentional, and variable intentional traces (Davis 1992b) in a site that was partly traversed by human or hominid activity as well as by natural forces and animals. Sometimes Binford calls this scrutiny “reading,” as in decipherment (e.g., 1983:11, 14). Usually, however, he wants to “take a reading,” as if from a thermometer (e.g., 1984:9). In its most bravura performance, this reading (e.g., Freeman, Binford, and White 1983) is surely a more “careful” or “close” forensic analysis than Marshack’s. In art history, Sir John Beazley’s (e.g., 1942) discrimination of “analytic individuals” (I use the standard archaeological term [Hill and Gunn 1977]) in Greek vase painting—actual painters, their workshops, etc.—is an equally careful and “close” forensics. Beazley’s “reading” is an individuating technique, like discovering or identifying the voice one wants to hear in a crowded, noisy room—whether or not it can, in itself, be understood. The actual historical results of both analysts carry greater weight and have attracted greater agreement than Marshack’s.

Whatever he may say, Marshack adopts neither a rig-

orous evolutionism nor a rigorous reflexology. Indeed, he avoids them and in some cases directly violates them, such as when he introduces a teleological force—a human “symbol-making capacity”—into evolution by natural selection. For just this reason, he seems to “read”—that is, attempts to diagnose (as Binford would say) or attribute (as Beazley would say)—the *remaining* components of the graphism, to be parsed, we could put it, as either nonadaptive reflex or adaptive nonreflex or what we can call graphic formality. Depending on background theory, yet to be explicitly identified, either one of these elements of the graphism can be taken as an index of what has ordinarily, and rather variously, been called “intentional,” “meaningful,” “conventional,” or “semiotic” marking: if the graphism is not reflexive, it must, so the reasoning goes, be voluntary, calculated, conscious, or what-have-you; if the graphism is not adaptive, it must be reflective, symbolic, aesthetic, or what-have-you.

Most important, because any forensic determination requires criteria for parsing the whole morphological field through time (the holomorph) into Binfordian (adaptive-rational), Beazleyan (reflexive-individuated), Marshackian, and possibly other domains, the graphism is always an index of the negative or converse of each of these possibilities. Therein, as Elkins suggests in different terms, lies its complexity and its challenge—challenge, that is, for any particular discipline that takes “conventions” or “signs,” say, as supposedly irreducible objects of study (as in one contemporary version of a semiotician art history). The very identification of the graphism itself, let alone its Marshackian reading (or Binfordian diagnosis or Beazleyan attribution), is at stake, though we must not dramatize the interest of this fact as such: Binford is quite prepared to discover that the “graphism” is, in fact, a natural scar, blotch, or stain, and he operates a forensic analysis adequate to the possibility, and Beazley initially assumes that everything is “graphism” even if it is not (there are times, for example, when paintings are attributed on the basis of the pot breaks). At a certain point, in other words, we leave the “graphism” and return to the context that supports and enfolds it, the artifact or the site, which in itself cannot be “read” in Marshack’s sense. (By the same token, at a certain point Binford leaves the site and “reads” the graphism, which cannot really be diagnosed in his sense; Beazley leaves the graphism and “reads” the artifact, which cannot really be attributed in his sense.) Thus it might appear that Marshack’s work is an intensive interrogation of the very possibility of reading, no matter how (or especially when) “close.” But so too, and equally, is the work of Binford or Beazley—from different directions in the explication of the holomorph, the material and temporal continuum of the site-assemblage-artifact-mark. If close reading is “impossible,” by the same token so too are diagnosis and attribution—at whatever scale. I do not think the de Manian doctrine of the necessary blindness of close reading really takes us very far with this point. Among other things, de Man (e.g., 1986) explicitly limited him-

self to the hypogrammatic (potentially readable) context of the “inscription” rather than the holomorphic (completely nonreadable) context of the hypogram.

I believe Marshack’s “readings” are important not because they are (or are not) “close.” In their evaluation of alternative scenarios of production, documentation of the material remains of the hypothetical holomorph and its hypograms (if any), fine-tuned attention to traces of inscriptional activity itself, etc., they do not seem to be as powerful as Binford’s diagnosis of adaptation or Beazley’s attribution of individuation. But none of this really matters—for in the end it is not so much the structure or style as the supposedly substantive *object* of Marshack’s reading that counts. Of course, as Elkins demonstrates, any general problems of reading will apply to Marshack’s. But does this tell us much about the specific content and intelligibility of Marshack’s—as opposed to any and all other close readers’—unique readerly interest?

Here we should recall that even in the field of prehistoric art studies—or the evolutionary archaeology of graphic formality—Marshack’s “close reading,” even on its own terms, is not necessarily any “closer” than that, for example, pioneered in the unjustly forgotten work of G-B-M. Flamand, *Les pierres écrites* (1921). Flamand literally paid microscopic attention to the way in which North African prehistoric engravings were made, studying the depth, direction, cross-section, etc., of incisions, extracting information about the step-by-step procedures and tools of the engravers. Flamand did not necessarily think he was “reading.” In his interest in patination and superimposition, he was closer to “Binfordian” concerns for site formation and “Beazleyan” concerns for chronosocial attribution. But nonetheless at some points he clearly aimed to reconstruct the cognitive schemata and conventions of the engravers. In Flamand we can find at least a prototype of most of the technical procedures or “analytic methods” employed by Marshack. The question, then, is not really *how* Marshack reads—or how “closely”—but *what* he reads so closely.

Why Marshack has become a globally influential scholar in many disciplines and Flamand remains an obscure practitioner applauded only by historians of African rock art studies (see Davis 1990:272–74) is not an uninteresting question. Flamand’s markings were in a sense already readable because they were understood, at the time, to be pictures; although one might wonder what the pictures symbolized, their legibility as such was not in question. Hence Flamand could be credited not with a substantive cultural-historical discovery but only with a seemingly archaeological or technical method. Marshack’s markings, by contrast, were not deciphered or even understood to be readable. Although Marshack has consistently proffered his decipherments—his well-known interpretation of “lunar calendars”—as a possible but purely hypothetical explanation for the readability of something else that he *has* succeeded in reading, the excitement that accrues to any decipherment perhaps overshadowed this reading. Mar-

shack is reading readability—in the sense, for example, that one might successfully read that a sequence of marks is in English or in a binary algorithm and still remain utterly unable to read that English or that algorithm as such, except by making a “best guess” from internal structure (unlike Champollion, who deciphered Egyptian hieroglyphs by “translating” back to them from Greek). To simplify a brain-twistingly complex affair, Marshack is reading the presence of Paleolithic-ese without *necessarily* reading what it is writing about. This is reading writing as an index-gauge of readability; Marshack is “taking a reading” of writing. The catch is that although there is always a temperature there is *not* always a writing to be “taken” in this sense. Marshack’s method, then, is more like a weather balloon than a thermometer; the phenomenon to be measured may or may not be there depending not only on how high, or close, the balloon gets but also on the vicissitudes of the atmosphere itself.

Although Elkins is correct that Marshack makes many “assumptions about notation,” his useful list of some of them—many are familiar outside Marshack’s oeuvre—really goes to show, if we had forgotten, that Marshack “reads” just, only, or essentially what he usually calls the “time-factored” nature of graphic formality. I am tempted to say that graphic formality is distinguished from both graphic adaptation and graphic reflex—clearly both are temporal or in time—by being *time-factored*, that is, and strictly speaking, divided by time. Importantly, factored division must remain hypogrammatic—a function of the meaning of the supervening or interleaving inscriptions or, more likely, of a superposing metric or scheme—rather than holomorphic, a function of nonintentional developments. Under this strict, if in principle forensically determinable, condition, formality is potentially readable as a segment of a schematism if not as a passage of writing. But because Marshack is reading the time-factoring of inscription (knowable if unread), not the inscribed meanings as such (readable but unknown), he often confuses unwary seekers after actual decipherment, which properly remains hypothetical—a “best-fit” proposal—in his work unless he can deploy independent (e.g., ethnohistorical) evidence. We need further discussion of all this, of course. It is possible, for example, that time-factoring is the general condition of, and thus a synonym for, writing and to that extent does not explain its evolutionary emergence—a crucial problem for Binfordians.

Now, it is a contingent fact of many of Marshack’s analyses that he studies “time-factoring” by a painstaking, often microscopic (“close”?) scrutiny of regular intervals—more regular, that is, than chance repetition—introduced into the making of individual or discrete and often very tiny or barely visible marks. (Interval is both inscriptive—the regularity of tracing—and hypogrammatic—the regularity of spacing. Holomorphic interval is, from the vantage point of reading, accident.) This focus, I suspect, is largely a function of the origin and development of Marshack’s own ideas and interests. It is not a theoretical requirement, or limit, of Marshackian

forensics. As Elkins suggests, time-factored formality occurs equally in, or as, “whole signs” or mark assemblages, in, or as, whole artifacts in their manual handling, as I have tried to show (Davis 1992a), and in, or as, whole sites in the “site formation process,” as behavioral archaeology suggests (e.g., Schiffer 1987). Some of these analyses will not be especially “close” by ordinary literary- or art-critical standards. Indeed, they will tend to be what is often called (in supposed opposition to “close”) “structural.” In fact, “close” reading might be carried out not to decipher meaning at all but only to discover where notation or narrative significance, the time-factoring of graphic formality, has been *disrupted*—“disfigured,” as de Man put it (e.g., 1984a)—by counter-cutting temporalities, such as the temporality of natural selection considered by Binford or the temporality of social identity considered by Beazley (for such an application of close reading, see Davis 1992a: 205–16).

In all of this Marshack’s approach reconnects with Binford’s and Beazley’s. Like theirs, his forensics, and whatever history or “reading” he can extract from it, is grounded in a set of views—largely implicit and as yet unsubjected to anthropological, let alone ideological, criticism—about repetition, regularity, rhythm, periodicity, individuation, continuation, sharing, and community in the human lifeworld. These views generate, of course, substantive social-archaeological expectations (e.g., transindividual repetition of rhythms and regularities = “tradition”). But they also propound a complex politics, even an ethics. Marshack’s great work, after all, addresses the “roots of civilization”; like Gombrich (1960), he offers a liberal, enlightened story about the achievement of meaning in human history—highly attractive, to be sure, but highly charged. Marshack’s weather balloon is sent out in order to discover where the skies have cleared, where, for example, the changing face of the moon can be and has been reliably and regularly observed and recorded—in other words, where readability, the possibility of human community and culture through time and across space, has appeared. Throughout, the problem is not so much the “impossibility of close reading” as the impossibility of “tradition”—at least in Marshack’s sense; it is not so much that the balloon cannot get close enough as that the sky is, was, and never will be clear enough for all of us at the same time.

Elkins invokes the midcareer work of de Man (i.e., 1979) to show why Marshack’s “close readings” are, if not “impossible,” at least defined and limited by the supposed fact that reading, when it actually reads, cannot simultaneously “read”—identify and critically reflect on, let alone dispute or disavow—its own “distance” from the object. This condition will be especially acute in “close” reading because, as Elkins notes, it claims to reduce such distance to the absolute limit conceivable at that point in the history of reading. De Man’s general and by now familiar notion supposedly applies to reading in general; like competing and perhaps more compelling notions of reading (such as Iser’s [1978] anal-

ysis of the textual “blank” as it is encountered and surmounted by the “act of reading”) it applies, willy-nilly, to Marshack’s “readings.” Again, however, does this really help us grasp the “case of Alexander Marshack”?

More interesting, for me, is the possibility that Marshack’s readings are exemplary—fascinating and frustrating—because they engage an issue that de Man took up in his later work. Attempting to produce a history of consciousness as a *theory* of the supposed facts of reading he had earlier elaborated, this work, as Derrida (1986) has recognized, was a critical reflection on de Man’s own earlier critical theory of reading. Elsewhere (Davis 1994; 1996: chap. 8), I take de Man’s central issue to be the reciprocal, two-way passage between “materiality” and “meaning,” simultaneous if different dimensions of an artifact in the strict sense. (Matter is in three dimensions, and meaning occupies the “one dimension” of nomination and predication.) In difficult essays on Hegel and Kant, de Man (1983, 1984b) considered this passage to be a process of “formalization” in which the second dimension, what he called “figurality,” and the fourth, “temporality,” effect the translations of modality. It seems to me that Marshack—using his own language, to be sure—imaginatively explores just this territory, as witness his continued emphasis on narrative, figurality-in-time, or what he sometimes calls “storying”; for this reason, he really “reads” neither writing nor nonwriting but, as I put it earlier, graphic formality or, more exactly, “story” formalization. As Elkins notes, there is a place (more exactly, a time at a place) where lexeme gives way to morpheme, character to mark, semiotic to nonsemiotic (and vice versa); Marshack works over and along a cusp—de Man saw it as an “abyss” but hoped to bridge it—for which archaeology and art history, catching up with him, need a critical theory (Elkins himself [e.g., 1995a] has made notable contributions to it). Since most of Marshack’s readers read him, however, from one or the other side of the “abyss,” he is very difficult for them to read—a phenomenon Elkins observes effectively.

There is much to take away from Elkins’s elegant paper. It is well worth while, for example, to highlight the Wittgensteinian question and to wonder what counts as a criterion for following a graphic “rule.” (We should look, I think, not to the “ruledness,” which can always be bent or broken, but to the actual “following”—the temporally defined or “temporary” material organization of replicating marks with the apparent purpose of reproducing certain properties [Davis 1987, 1992c]. The forensics of “purposiveness,” as Binford would insist, is different from the decipherment of signs; I “read” the cigarette butt on the ground to discover that someone wanted to wait before committing the murder, not to receive his advertisement that the murderer smokes.) And, if “close reading” is one’s thing, it is well worth reading Elkins closely to determine what he thinks the *opposite* of “close reading” might be—sometimes it seems to be “lazy” or “naive” reading, sometimes reading outside a discipline, sometimes reading for structures, sometimes “self-reflexive” reading, sometimes

the sceptical disavowal of reading itself—and whether it too must be “impossible.” If, as the opposite of all of these, close reading is hardworking, disillusioned, discipline-bound, attentive to phenomena, nonself-scrutinizing, pragmatic, I’d rather see more than less of it—“impossibility” or not. Indeed, surely this “impossibility” is an artifact of a runaway “self-reflexiveness” to which Elkins himself is somewhat blind. But, as Elkins knows, that’s irony.

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Elkins’s concept of “close reading” is too nebulous to produce an insightful analysis of the various procedures that archaeology uses to study objects laden with meaning in a social context. As anthropologists we adhere to paradigms, conceive theories, and test those theories using the analytical criteria and methods that we are able to develop. “Close reading” appears to be a paradigm that Elkins does not share with the scientific community. Epistemologically, what matters to us is not the level of observation or how carefully objects are studied but what questions we ask and what interpretive principles and analytical means we possess to answer them. Elkins’s perspective falls outside this context: he wants to destroy the current paradigm of archaeological science without providing a new one. Since I operate in the context of normal science, Elkins’s stated aim, to demonstrate that “there is no such thing as a truly close reading,” naturally seems to me an unproductive one.

In trying to achieve this goal he chooses an easy target. The weaknesses of Marshack’s approach are well known, many of them probably to Marshack himself. I have often pointed out (1989c, 1991, 1992a, b, 1994; d’Errico and Cacho 1994) that Marshack has never proposed a definition of “system of notation” or the other concepts he uses. He claims that he has identified Upper Paleolithic systems of notation, but since he has never explained what a system of notation is we do not know what he has discovered. This does not mean, however, that a “close reading” of the archaeological record is impossible.

The questions raised by Marshack should be seen from a different perspective. Humans are the only beings capable of creating artificial memory systems means of recording, storing, and transmitting information outside of the physical body. Undertaking research on the origin of artificial memory systems in the Paleolithic requires, first, that we define and classify the various types of such systems, that we understand the various rules which govern their codes and the ways in which information is processed. This is what I am trying to do with a large number of ethnographically documented artificial memory systems (1995). Secondly, we should verify the applicability of our model by developing methods for interrogating the objects. The distinction between “notation” and “decoration” is a crucial issue: analysis

of modern artificial memory systems indicates that the type of code will determine the probability of secure identification of archaeological ones.

Criteria for identifying types of artificial memory systems such as those based on accumulation of information over time derive from the technological analysis of marks. When marks are created by lithic tools, a change of tool will probably take place before each new stage of marking if the periods separating stages are relatively long. If changing the tool produces morphological changes visible to the naked eye, these different morphologies can be interpreted as the result of the craftsman's will. In contrast, if changing the tool does not produce changes in the morphology of marks visible to the naked eye, then it is reasonable to consider such changes as an epiphenomenon and the result of the accumulation of marks over time. Isolating reliable criteria for identifying morphological variations produced by changing tools therefore requires equipment for going beyond the naked eye's view and constitutes an important step in identifying certain types of artificial memory systems. In contrast with Elkins's argument, this example makes evident the interest of a technological analysis of Paleolithic marks in investigating their possible use as artificial memory systems.

The first step in the analysis of marks is the identification of the technical procedures used (unidirectional movement of a point, to-and-fro movement of a cutting edge, rotation, pressure or indirect percussion of a point). Since different technical procedures result in different types of marks (see d'Errico 1995), the diagnostic criteria will be different for each. The second step is finding additional criteria for determining, within each technique, the specific causes of morphological differences between marks, among them modification in the orientation of the tool or of the object as well as change, breakage, or resharpening of the tool.

Elkins is right about the weakness of Marshack's analyses (White 1982; d'Errico 1989b, 1992a, 1993b, 1994). Marshack is always confident of his ability to identify human-made marks, the way in which they were produced, the direction of movement, possible changes of tool and changes of orientation of the object, traces of long manipulation of the objects, and the like, but he relies exclusively on the microscopic observation of archaeological material. The observation of a particular morphology does not, however, provide, in the absence of experimental data, proof that this morphology is the product of a particular tool or of a particular technical procedure. Marshack has never clearly described or validated by replicative experiments the diagnostic criteria he uses in his microscopic analysis, and therefore when he identifies the technique used in producing prehistoric marks we do not know the basis for his claim.

Passing me off as a partisan of the use of the scanning electron microscope against the use of optical microscope, Elkins, a critic of Marshack, uses the same arguments used against me several times by Marshack (1989, 1992b). In my research I use optical microscopes, scanning electron microscopes, environmental scanning

electron microscopes, image-analysis software, measuring stations for profile recording and evaluation, and noncontact three-dimensional measuring systems. The problem is not the equipment used for observation but whether an instrument or a combination of different instruments can help us in reconstructing past human behavior.

In conclusion, Marshack has provided no theory or analytical criteria, only a large amount of raw data and stimulating, if unverified, hypotheses. Elkins, for his part, denies the possibility of carrying out research in the field; he has a very reductive view of the role of the combined use of theory, observation, and experiment in science. My feeling is that his way will not take us very far. An accident in the history of the discipline should encourage us to work better, not to stop working.

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Asked to tell the story of early man, one seven-year-old wrote, "It was al durk. Anything cud 'appen to anythink." Though short on precision, the comment has a humility lacking in Marshack's reading of scratch patterns on Paleolithic fragments. I can only speculate on how much has dropped off the edge of the Paleolithic world—materials which might, presumably, have stood within analogic reach of the burin marks in question more appropriately than the phases of the moon. Elkins, however, is not primarily concerned to question Marshack's homology; his is a formalist's response to a formalist's analysis of texts which, because they predate writing, arithmetic, and agriculture, can have no traceable and semanticizing context of use. What results is the finest of arguments between proto-grammarians, turning on the system (or lack of it) among notations and concluding with an appeal for greater rigour concerning the "fundamental terms" through which we describe images. Nothing upsets a formalist (Marshack) as much as a more scrupulous formalist (Elkins).

I parody Elkins's case to mirror his method. Marshack's microscopically close reading provokes Elkins to go closer so that he may explore the hypothesis that techniques of close reading (in whatever field) generate neither accuracy nor truth but instead promote more of the same and consequently expose "a hollowness in the concept of interpretation itself." The claim is a bold one, founded on the shifty semantics of Paul de Man (or, more particularly, on a model of meaning derived from his *Allegories of Reading*). And it is here, on the generality of Elkins's assertion concerning interpretation and on its philosophical grounds, that I shall focus my reservations.

No small part of the force of Elkins's general case stems from the match he makes between bone fragment and philosopher. De Man's linguistic paradigm locates contradictory impulses at the structural core of the sign,

which he perceives as having double and incompatible functions—simultaneously figurative and referential. Split, language paradigmatically catches itself out being literal at the same time as it is metaphoric. For de Man, metaphor is *the* representative linguistic trope, to be defined as a “figure that disfigures,” conferring “the illusion of proper meaning to a suspended . . . semantic structure” (1979:195). Where a sign appears to refer to or to represent external reality (logos), it does so only because users and recipients have neglected one side of its metaphorically literal nature (lexis), with the result that they transform the sign’s “referential indetermination” into “a specific unit of meaning” (p. 154). De Man’s fondness for close reading (and hence his attraction for Elkins) stems from his elevation of the figurative over the literal, an elevation which extends the “interior” spaces of the text while shrinking what lies “beyond.” Any impulse to turn outwards (and refer) is, and should be, undercut by appeal to the rhetorically constructed (and marked) nature of the referent—or so the story goes. For de Man, there is no “outside,” only a linguistically driven and linguistically corrected impulse towards it.

The idea that the “outside” is no more than something which language is about or has about it must gladden the heart of those who seek to read scratches on Paleolithic bones in the manifest absence of the Paleolithic world, but it is not a notion to be necessarily and uncritically generalized. Yet Elkins does just that, at least tacitly: having increased the available and possibly significant marks that make up the “interior” of his bone texts and consequently having called into question and further dematerialized Marshack’s already tenuous lunar referent, Elkins claims that Marshack, with his eye for notational nicety, does “what we all [as close readers] say we should do” in fields as diverse as archaeology, art history, and literary criticism. I would object that many say and do nothing of the sort. Only if de Man’s account of the problematics of reading is in some sense both pervasive and right can such a claim be made. “Closeness” for de Man derives from an epistemological crisis which weakens to breaking point the relationship between the sign and the referent. However, readers who are of neither a semiotic nor a poststructuralist persuasion might posit an indexical connection where de Man finds unbridgeable distance. For the psychoanalytic critic, signs, however distorted, remain indexically tied to a founding trauma whose force is made present through the signs’ semantic displacements. For the historical materialist, trauma is necessarily public, but it too is “outside” the text, existing for the reader in the vehemence with which its conflicts are thrown forward (or disguised) by the language from which the text forms. My examples merely indicate that there are classes of reader unprepared to take de Man’s overt, and Elkins’s implied, linguistic turn away from the referent. For such readers Paleolithic marks, lacking any but the most speculative “outside” to which to refer, may be matter best left to semiotic illumination. It is, however, special pleading to transform a species of close reading *sans* ref-

erent, attractive to a linguistic philosopher semi-*sans* referent, into the very type of all close readings.

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Elkins’s careful account of the notion of “close reading” relied upon in “anthropology, art history, and archaeology” indicates structural contradictions which render close reading “impossible” before concluding that this impossibility stems from a question of comfort, the everyday desire for interpretive distance. While “close reading,” Elkins states finally, “has to be possible” if there is “to be such a thing as a text, or an image, to understand,” it also “has to be impossible, in order to let us get on with the vague meanings we all prefer.” For the first time—fittingly in the last line of his essay—Elkins puts the cart before the horse, implying that close reading is untenable on the basis of just those less precise readings it puts into question. This conclusion is as incongruous with each of the different strands of the scrupulous analysis that precedes it as it is revelatory of the general predicament engendered by assimilating the notion of “reading” (whether “close” or summary) directly to “visual artifacts.” Having made that assimilation (Elkins speaks of “a text, or an image”) without (closely) investigating *its* bases to begin with, one may also declare it practically unworkable and (summarily) pull the plug. The surprising and improbable outcome of Elkins’s analysis—Paul de Man might have called it ironic—may offer yet another indication of the problem of reading it raises and strives to address.

In choosing to concentrate on the work of Alexander Marshack, with the assistance of Paul de Man (and Henry Staten on de Man), Elkins indeed goes to the heart of this problem, in ways that are tangential to the analysis he presents. First, he demonstrates that Marshack may inaccurately characterize the markings he so minutely reports; “close reading” here seems no guarantee against misrepresentation toward a specific end. Next Elkins raises the more formidable question of the status of the markings themselves: at what “size” and in what “position” do microscopic marks cross the “threshold” separating notational marks from signs, and by what criteria can we distinguish notation from decoration? The problem with such clear-sighted queries is obvious to him in turn: since Marshack is analyzing markings for which no other markings can serve as historical control, his lunar theory may well appear “lunatic,” but it cannot be disproved, for “each [of a number of critiques about methodology and falsification] has to appeal to a prior sense of notation.”

Marshack’s critics, then, would be defeated by that rarest of synthesis: the combination of historical knowledge (or ignorance) with formal logic, an empirical, rather than merely definitional, *petitio principii*. Nonetheless, as Elkins elaborates in a cogent list of such instances, his “close readings” can lead to a salutary inves-

tigation of "the most fundamental terms that are used to describe images" once these are confronted with the very specific "assumptions" Marshack makes about the defining qualities of notation. Before discussing Marshack's work as an exemplary test case for the conventions of descriptive discourse, Elkins then turns to the work of another exemplary close reader, de Man.

Following Statten, Elkins wishes to make the point that close reading must always be positioned at some "one distance" from the work it reads and that any position is necessarily "blind" to itself, that is, to the other readings its particular closeness veils. De Man, Elkins recognizes, was already well aware of this and as skeptical of the faith that methodological "progress" would relieve the pressure on reading as of a "'naive' reading of literature, which understands it as a mimetic project," that is, one requiring no significant formal reading at all. Now, the essay by de Man which Elkins cites (but does not name) is entitled "Semiology and Rhetoric," and the merest reading of that title brings into view the general predicament I have alluded to above. Semiology—which, in the course of that essay and the corpus of de Man's writings, is increasingly identified with "grammar" (see de Man 1979:6–7)—offers a way of understanding language according to "the consistent link between sign and meaning that operates within grammatical patterns" (p. 8), while rhetoric is precisely the way in which language renders such a link *inconsistent*. The problem is that grammatical patterns and rhetorical ones cannot be kept apart. Indeed, they might be identical down to the letter, as de Man proceeds to demonstrate through examples of rhetorical questions raised equally by the famously mundane (Archie Bunker who, like us, "might like to get on with vaguer meanings") and the famously mystical (Yeats's "leaf" and "blossom," "dancer" and "dance") whose impeccable grammatical form leaves their meaning no less in doubt (pp. 9–12). And this particular manifestation (not the only one but, in the present context, an especially conspicuous one) of the problem of the fundamentally double status of language to which de Man devoted his work—the problem, as de Man frequently put it, of not being able to know or to read "what language might be up to" (p. 19)—is not a function of the "distance" from which one reads, how "close" or how far (or how fast or how slow). Spatial metaphors that disguise the difficulties inherent in all reading, these terms function nonrhetorically only at the optometrist's office or at the wheel—when what one reads is by definition purely or univocally conventional or symbolic, such as disconnected letters of descending size or the number of an approaching exit—or when one "reads" as Marshack does. In deciding that the marks on Paleolithic artifacts worth considering are notations and in linking these notations to a specific motive (the lunar theory), Marshack has certainly made these artifacts legible. Yet, as Elkins demonstrates indirectly, it is not "close reading" (or "the impossibility of close reading") that is at issue in Marshack's method: decades of painstaking microscopic

research can and will produce selective results. What is at issue is what is alone deemed to be meaningful and what that meaning is, and in this Marshack is everything but a "close reader" in the sense Elkins associates (albeit "impossibly") with de Man. Elkins states that he has not considered close readings whose "object is linguistic"; nor, in discussing closeness, is he concerned with "just closer looks at meaningless marks." Yet by assimilating Marshack's literally microscopic work to "close reading," by identifying closeness with the finding of "meaning in successively smaller signifying units"—that is, by implicitly conflating "image" and "text"—he both presumes a linguistic paradigm and denies it, as rightly he should. Marshack's rigorous method for "reading" a countable sequence omits what it must, "signs," "symbols," and "decoration"—the "rhetoric," one could say, of historically illegible engravings—as well as their permanently illegible, yet rigorously indistinguishable double, "meaningless marks." In "reading" the marks of "visual artifacts" whose identity, meaning, and configuration must all be up for grabs, Marshack is by no means reading language, whose duplicity or ambiguity of meaning arises instead from the fact that its elements and relations are indeed already determined but always in one too many ways.

Marshack is "reading" images, and if he transcodes them into the most single-minded of meanings—"notations recording the phases of the moon"—and does so without refutation, it is not only because prehistoric artifacts preclude all comparative *points de repère* but because, viewed rigorously, such bases for comparison are precluded by all images and not only prehistoric artifacts. Just as images, unlike language, range mimetically from iconic representations to "meaningless marks," Marshack's "reading" of images demonstrates the impossibility of reading images in linguistic terms. However "long" we may turn "our conversations inward" so as to interrogate "the central terms in our discourse about images," those terms will not be the terms of language, and Marshack's transcoding of image into single-minded notations will not be refuted. It is not that Marshack reads too closely for comfort or not closely enough but rather that, for the same reasons that reading with certainty is indeed impossible on de Man's view, it is impossible that Marshack would indeed be reading. To do so he would first have to *invent conventions* concerning *all* the marks—whether repeated or not—that he perceives *and*, if that were not enough, invent those conventions doubly and at cross-purposes—as grammatical forms, for example, and rhetorical deformations, as semiosis and as trope. Compared with the incomparable origin of any and every language, decades spent before a microscope recording markings lacking distinct identity and precedent may seem as straightforward as noting, say, the phases of the moon.

By confusing what Marshack does with reading, Elkins ends up here with what he elsewhere attributes to Nelson Goodman—"a precisely articulated model of the confused density of thought we bring to images: a clear

reflection of something that is originally unclear"—and for that same reason, as he clearly recognizes, a "misreading" (Elkins 1993a: 362).

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Despite the title to Elkins's essay and his careful logical and philosophical "close reading" of my "close readings" of a numerically small but important class of Upper Paleolithic engravings,¹ it is *not* the philosophical "impossibility" of close reading that is a proper subject for discussion but the *historical* nature of close reading and the nature of the diachronic changes and developments that occur in close readings within different realms of inquiry.

Since the invention of the microscope and telescope and their multitudinous adjunct technologies, it has been an increase in the range and scale of close readings and the analytical data derived from these readings that have, in large measure, defined and described progress in science. What Elkins describes, cumulatively, as the "impossibilities" of close reading are, actually, the questions, uncertainties, assays, inner discourses and arguments, and tests that accompany the development of any innovative inquiry. These define or describe not "impossibility" but, rather, a self-correcting process.

This is exemplified by Elkins's discussion of the Barma Grande pebble. That pebble was published in 1972 but had been examined in the first days of my research, around 1968. The pebble was omitted from the second edition of my book (Marshack 1991b) because subsequent research had indicated that it did *not* conform to any tradition or mode of Upper Paleolithic notation. I explained this to Elkins and sent him the 1991 edition, but he decided to keep the Barma Grande pebble as part of the structure of his argument. The change in my assessment of the Barma Grande pebble was part of the normal process of a developing, self-correcting inquiry. In the second edition of *Roots of Civilization*, instead of the Barma Grande pebble I included, for Italy, the analysis of an engraved composition containing an intertwined accumulation of serpentine, "macaroni" motifs (1991b:102–5). My ongoing research had found that accumulations of this motif were present in every region of Upper Paleolithic Europe, including the Franco-Cantabrian caves. Though these serpentine motifs were *not* "notations," they were ubiquitously and apparently periodically accumulated. They may, therefore, have been part of Upper Paleolithic cultural, symbolic tapestries that included notation (Marshack 1975, 1977, 1979a, 1991b, 1992, 1995c). In place of the discussion of the Barma Grande pebble I inserted my analysis

of the Grotte du Taï notation (Elkins's figs. 5 and 7), an analysis in no sense based on a "close reading" of the type Elkins describes.

The Taï composition, incised on both faces of a fragment of bone, had an internal complexity and, therefore, analytical problems that were greater than any I had yet seen—complexities, in fact, that could not be assessed by microscopy. This complexity was due not to the cross-sectional data of particular marks or to any uncertainty in counting sets and subsets but to the great number and variety of the problem-solving strategies that seemed to have been involved in structuring and accumulating the composition. I published a note stating that the composition seemed to be "notational" but that proper publication would have to await further study (Marshack 1973). That study took 20 years and involved direct, firsthand study of the problem-solving strategies and traditions found in the notational systems and notational artifacts of preliterate peoples in every part of the world (see Marshack 1974b, 1985a, 1988b, 1991a). These were not close readings of incised cross sections or of static images but cognitive studies of the diverse modes of visual problem solving devised by different cultures for encoding, structuring, and sequencing a notation. In one instance a calendar board had been kept by a Maya shaman not by incising but with charcoal, and so the analysis was conducted by infrared photography. My analysis of the sequence of marking, which I determined involved the marking of short subsets on certain days in order to build, cumulatively, the larger sets of a structured "month" and to accumulate, sequentially, the months of a cultural "year," was confirmed by the ethnographer who had collected the board (Marshack 1974b). My analysis of a calendar stick kept by the Yakut of Siberia revealed the same type of boustrophedon sequencing that I had found on the Taï plaque (Marshack 1991a). The studies also involved consultation with neuroscientists and cognitive scientists, who supported both the mode of analysis and the findings (Marshack 1985b). The Taï analysis was not, therefore, a "close reading" of the type that Elkins imagines.

I had found in my long inquiry that most of the notations in the ethnographic record contained, in a single composition, different forms of symbol, sign, and reference and involved sequences of different problem-solving strategies. Consequently, when I returned to the Taï plaque I saw not a static image but a sequence of problem-solving strategies at different points or positions in the accumulation, strategies that I had encountered and studied before. I saw the results of that quarter-century-long inquiry. When Elkins looks at the Taï plaque, however, he sees a *static* image, with unrelated groupings at the left that seem to occur as visual *intrusions*. We have, then, two attempts at "close reading" involving different levels of experience and expertise.

I describe one set of the strategies found on the left side of the Taï composition (Elkins's fig. 7). Sets of marks are engraved above and below the containing lines (C–G) (Marshack 1991a:46, fig. 17), a process that

1. See my report in this issue (Marshack 1996b) for other classes and my close reading of cave paintings.

occurs nowhere else in the Taï composition (fig. 5). At the far right (fig. 5), when space for marking was needed and one of the incised horizontal containing lines (*E*) ran into the broken edge of the bone, that containing line was extended downward, vertically, and continued to be marked; it then ascended to meet the horizontal line (*F*), with the ascending vertical also being marked; the marking then continued on row *F*. At the far left, however, the problem of inadequate space took a different form (fig. 7). The small marks incised above the containing lines (*C–G*) had been marked first, while the marks *below* the containing lines had apparently been incised five to six months *later* in response to the lack of space created by the vertical zigzag (fig. 5) (see Marshack 1991a). In this area the upper and lower sets were less than a millimeter apart, yet they had apparently been incised approximately half a year apart. For the engraver this was easy to “read”; for me, however, it had posed a problem for two decades. For a novice viewer such as Elkins and one unfamiliar with notation as a problem-solving mode, figures 5 and 7 are, of necessity, perplexing static images. Significantly, it was *not* microscopy that led to the above processual determination, though microscopy was used to study the cross sections and to determine where a lower mark crossed a horizontal or crossed a unit mark on the upper register. The determination was, essentially, “cognitive,” a result of long years of inquiry into modes of problem solving in notation. To recapitulate: close reading by a novice tends to see the static printed image, studded with inexplicable intrusions; the “expert” viewer, however, sees a sequence and process of problem solving that can only belong to notation and not to “decoration”—a process that apparently occurred over a significant period of time. It is, of course, possible that a “closer” reading by scanning electron microscopy will alter certain aspects of the above readings, but I doubt that it will change a determination of the essential process.

Since the problem in close reading is not merely that of “closer seeing” but of intelligent, expert seeing, it is necessary to address a different aspect of the problem that Elkins raises, one that concerns not the “impossibility” of close reading but rather the limitations and errors that can occur in the research of those who claim that they have instituted far “closer” and therefore more “scientific” forms of reading Upper Paleolithic engraving. Improved technology and “criteria,” it is claimed, can by themselves bring better understanding and, therefore, validate or invalidate the notational hypothesis. This may seem the converse of the problem raised by Elkins, but it goes to the essence of the same problem: how one “sees” what one “sees” and does not see what one has not been properly prepared to see.

Francesco d’Errico, seeking to test and invalidate the Upper Paleolithic notational hypothesis, used scanning electron microscopy and experimental tests to determine the nature of incised cross sections and the changes that occur to an engraving point and engraved cross section under conditions of use. He then applied this technology to the study of a set of 168 incised pebbles

from the French Azilian (post–Upper Paleolithic). He found that each of these pebbles had been incised at one time and with a single point and concluded from this that notation did not exist in the Upper Paleolithic (d’Errico 1989b, 1994). He had, however, elaborately and exhaustively studied a set of pebbles “decorated” with a motif. To anyone familiar with notation it was clear that these Azilian pebbles had none of the characteristics or criteria of the Upper Paleolithic notations I had published. Their primary characteristic for analysis and a test of the notational hypothesis was that they were available, they were engraved, and they contained sets of marks. As a result, the “closest reading” that had yet been devised for studying engraving had led to a false conclusion. D’Errico had erred in his use of a good technology by not investigating the variability and complexity of the corpus and had therefore applied his “close reading” to an inappropriate sample (see Marshack 1995c). Significantly, the one composition in his sample that was clearly notational by his own analysis and criteria (Ro48) (d’Errico 1995), in that it had an accumulation of sets that had been made by different points and, as he wrote, was probably incised “over a period of time,” left him puzzled, and so he ignored it in all later publications, persistently maintaining that “all” his Azilian pebbles had been incised at one time and with one point.

I advised d’Errico in this journal (Marshack 1989) to use his technology to study the engraved accumulations found in the Upper Paleolithic, usually on fragmented pieces of bone. He did so and found that notation did, after all, exist (d’Errico and Cacho 1994) but claimed that it could not, despite that, be “lunar.” He has, in fact, tried to maintain that though there was notation and it was incised by different points, it could still have been incised at “one time” (d’Errico 1995, but see Marshack 1996a). What type of notation, however, could conceivably have been incised at “one time” during the Upper Paleolithic that, like that Taï notation, consisted of a linear, sequential, accumulation of over a thousand marks broken down into sets, subsets, and superordinate sets? D’Errico has not attempted to address this problem. Instead, he now claims that he has discovered “the origins of writing,” but there is nothing in the Taï composition or in any of the notational compositions that he has recently studied that in any way meets the criteria for “writing.”

I earlier noted that the microscope and telescope have helped to define and describe progress in science. But *technology* is not science, and microscopy, no matter how “close” or precise, does not alone and merely by its use provide either understanding or knowledge. Any “close reading” requires some level of conceptual preparation for proper interpretation of the data obtained.

I close with a discussion of some of the errors made by Elkins:

1. Marshack claims that “notation is ‘intentional’ . . . Sloppy or haphazard markings are more likely to be decorative. . . a marked artifact may be either decorative or notation but not both.” Not so. It is one of the primary

findings of my research that there are different types and classes of marking, including unstructured intentional forms of "sloppy" ritual marking that seem to create a random *mélange* of marks and the "unstructured" accumulation of "structured" motifs that fill a surface. These are not "decorative," since they were usually not intended for display. It was the act of marking, not the "design," that was relevant. I addressed contrast between "notation" and "decoration" in the terms noted by Elkins because traditional Upper Paleolithic archaeology had for more than a century categorized almost all marked artifacts as "decorated," that is, as "art" or as "decoration" (cf. d'Errico 1992, Marshack 1992).

In considering the Mezherich composition, for instance, it was not the static image that Elkins provides (fig. 9) that was involved in my "reading" and interpretation of the composition as notational but, rather, once again, analysis of the corpus of incised materials and compositions from the Upper Paleolithic of the Russian Plain. There was a tradition among these cultures of motif accumulation (see Marshack 1979a) as well as exquisite instances of "decorated" personal adornments. The Mezherich composition did not fit either of these categories. It was incised on a broken bâton, and broken artifacts represent one of the primary classes of materials upon which notation but not decoration was often incised. The composition was idiosyncratic. All "decorations" of this period were made in the "style" of the period and the region (cf. the Mezin bracelets and the decorations on the Mezin figurines). Every notation in the Upper Paleolithic, however, differs from every other and never occurs in the "style" of extant regional decoration. It was not the static image that Elkins has depicted that I was "reading" in the Mezherich analysis but also the context and the tradition within which it was found and the specialized body of notation to which it seemed to refer. "Logic," in this instance, is blind. The static image presented by Elkins is perplexing because it has no context.

2. "Notations are made over time, with measured pauses between marks. . . . marks made over time are likely to be notation." Not so. It is a major finding of my research that accumulations of *motifs* also occurred over time, both on the mobiliary materials and in the Franco-Cantabrian caves. Such motifs could be either single marks, sets of marks, or "signs" such as handprints, macaronis, and "tectiforms." Accumulations "in time" were, therefore, not limited to notations, which were a comparatively rare and extremely specialized form of accumulation.

3. "If a set of marks is a notation, every mark is significant and must be counted." Not so. A primary finding of my research is that, while every mark may be significant, not every mark must be "counted." "Cueing marks" placed upon a mark may indicate the symbolic, referential importance of that mark or the importance of the position of that mark in an accumulation. The mark would be "read," but it would not be "counted."

4. "The supposition is that they [the notations] were made before there was such a thing as counting." Not

so. There was clearly an ability to count small sets of, say, one to ten, but there is no evidence of a summing of sets, that is, of an arithmetical system. Small sets of days could therefore be counted and marked at one moment, and longer periods of observation could be marked each day without counting. This is the type of observation and counting common in many nonarithmetical calendars. There could be a count of the number of days from a crescent to a ritual and a count of the number of days for a ritual and yet no count of the days in a month or year. Recognition of six months or "moons" was merely a count of six, not a count of the days in six months.

5. "Marshack's central claim is that many Paleolithic markings . . . are notations recording the phases of the moon." Not so. The primary finding of my research was, as Elkins recognizes, that the Upper Paleolithic notations did *not* record the "phases" of the moon but, instead, there was a tendency to begin or end accumulating sets or groups of sets around observational points in the lunar cycle. It was such accumulations that were tested and that can be tested in any long sequence of sets. As any astronomer can tell you, this is not a recording of "the phases of the moon," but it is the way that a nonarithmetical lunar record would be kept. If an Upper Paleolithic notation is run against an astronomically accurate lunar model, moving it forward one day at a time, there is usually only one "setting" of that full sequence of notation that will both begin around a phase (a crescent, invisibility, or full moon) and then continue for the full sequence to maintain an *observational* but not an arithmetical lunar "fit" of the type described. Rare artifacts are ostensibly "lunar" on first observation (e.g., the Gontsy ivory, the Blanchard plaque [Marshack 1972a, 1991]).

6. "The 'lunar model' . . . shows the moon's phases along with a possible count of the days between them." Not so. The lunar model is an accurate astronomical structuring of lunar periodicity; it includes a "zero" day that allows the 29½-day lunar month to function within a two-month 59-day period of observation. The model is adjustable within that frame. It is intended to provide not a "possible" count of the days between observed phases but an astronomically correct model against which one can position a possible sequence of naked-eye observations of phases and periods. The "possibilities" that were tested were in the notations, not in the lunar model. This mode of testing was repeatedly checked with practicing astronomers, and the resulting data were repeatedly presented for evaluation and criticism at meetings of astronomers and for peer review of my papers in volumes edited by astronomers. It was never faulted on the grounds raised by Elkins.

Significantly, in the notation from the terminal Upper Paleolithic site of Tai there was a break or division of long five-to-six-month sequences at apparent *solstitial* positions (Marshack 1991a). The internal "lunar" approximations within each five-to-six-month period were, however, of the order noted above. The Grotte du Tai is a cave shelter overlooking a junction of rivers

and a wide plain upon which herds grazed and moved seasonally, with foothills behind the cave to which animals would move in the summer. The end of the Upper Paleolithic was a period of increasing long-term sedentism, and Tai was a stationary long-term habitation from which to follow the round of the seasons, the movement of animals, the seasonal movement of the sun, and the monthly waxing and waning of the moon. That order of sedentism would occur later in the first farming villages of the Middle East and eventually result in the institution of formal, temple-anchored astronomical record keeping. When "reading" the Tai plaque the original maker was *also* reading the referential context. I was, initially, "reading" only the plaque.

7. "The shape of a mark is generally not significant. . . . the orientation of a mark is generally not significant. . . . the position of a mark on a surface is generally not significant." Not so. The shape, orientation, and position of a mark may, in fact, provide the crucial discrimination for a proper reading. This was indicated in my early reading of the Gontsy and Blanchard notations and my more recent reading of the Tai notation.

Elkins has raised and argued many of the central problems and questions associated with the notational debate. He has done so in a manner never before attempted, and he has done so intelligently and perceptively in terms of his own preparation and in terms of the "text" and images he had in hand. But he has done so by a use of logic and philosophical reference and by reference to the static image on the printed page. He has indicated no knowledge of the Upper Paleolithic materials or their variability. He has not understood the nature of "close reading" as an aspect of ongoing historical development rather than as philosophical and logical constraint. Nor has he addressed the general problem of the nature of notation as a variable form of information encoding or of notational analysis as a specialized form of inquiry occurring at different levels of "close reading." Nevertheless, it will henceforth be impossible to discuss the problem of notation or notational analysis without reference to the perplexity of and the arguments and problems raised by Elkins.²

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X 95

It is not often that a literary critic is asked to respond to an article in *CURRENT ANTHROPOLOGY* written by an art historian. Credit (or blame) for this anomalous situation must of course go to Elkins's remarkable breadth of scholarship and this journal's capacious understanding of intellectual inquiry, but it is also clear that certain theoretical questions are continuing to exert pressure on disciplines in ways that cut across disciplinary boundaries. The problem of close reading can be understood

as fundamental to any interpretive discourse, as Elkins persuasively shows. Though much could be said about the ways in which the various disciplines of anthropology, art history, and literary criticism are made to speak and interact in Elkins's article, I shall repeat this article's interdisciplinary gambit and direct my comments as much as possible toward the paradox of close reading *per se*.

Two theses intertwine in Elkins's text. The first is that close readings have the power to transform "confident criticism" into "rooted doubt" because they cast our skepticism back at us, forcing us to confront the uninsurability of our own presuppositions. The second is that any close reading can be undone by a "closer" reading; "a close, or closest, reading cannot succeed in erasing signs of closer *meaningful* readings (that is, not just closer looks at meaningless marks), and it cannot fully understand or exposit its own position." These two theses both trace the emergence of "reading" as a transcendental problem out of the specific violence and stubbornness of a "close" reading, but they do so from opposite vantages: from the one perspective, the close reading destabilizes our own act of reading; from the other, our own act of reading destabilizes the close reading. Elkins understandably blurs the difference between these two vantage points, since they add up to the "impossibility of close reading"; however, in doing so he risks understating the very difficulty he is setting out to analyze.

To sharpen this point, let me appeal to a constellation of texts which Elkins invokes but, no doubt for pragmatic reasons, reads about as "distantly" as the conventions of critical writing allow. When Henry Staten reads Paul de Man reading Nietzsche's *Birth of Tragedy*, Staten does not begin in "confident criticism" only to end in "rooted doubt." We could probably find moments of hesitation or slippage in his essay if we looked hard at it, but its tone, generally speaking, is confident all the way. If in his turn Staten is distorting Nietzsche's text, he is "blind" to this fact, and, as Elkins points out, this blindness *enables* his reading. To read one must forget the very uncertainty which reading exacerbates. An abyss lies between Elkins's two theses: the skeptical knowledge of reading's uncertainty is precisely what reading obliterates even as it produces this knowledge.

We witness this paradox in action as Elkins sets out to "make a reading of one of Marshack's readings that is closer than his own reading." This project is more complicated than it looks. One's first reaction, indeed, might be to object that Elkins does not really "read" at all: he demonstrates that Marshack's formalized notation differs from the photographic image it represents, but he does not himself propose an interpretation of these marks or signs except in order to draw attention to inconsistencies in Marshack's approach. Elkins is of course seeking to "read Marshack reading" rather than read a Paleolithic text—his object, one could say, is the skeptical knowledge of reading's impossibility rather than the positivity of a "closer" reading. Yet the one can only be obtained through the other, even though the two pull in opposite directions; and if we look more closely (so to speak) at Elkins's argument we observe that a cer-

2. © 1996 Alexander Marshack.

tain reading of the Paleolithic marks or signs is in fact being performed. The marks or signs are being "read" as self-identical entities, available as such to the eye, thanks to the mimetic activity of Elkins's hand and pen. If Marshack assumes that marks mean what he says they mean, Elkins draws attention to the violence of Marshack's reading by implying the transparency of his own act of representation. Reading "more closely" than Marshack, he effaces "reading" itself by transforming reading into seeing. Yet only through this blindly confident claim to see is he able to arrive at the insight that reading requires blindness.

The difference between "marks" and "signs" is in one sense moot, since for Marshack notational marks compose a sign of the human as temporal self-awareness: "time is here *symbolized*, abstractly and cumulatively" (Marshack 1972:90). This symbol of humanity is also the sign of reading's possibility, since throughout *Roots of Civilization* Marshack lays claim to a certain intuition, a "feeling about man" which spurs and ratifies his interpretive efforts (Marshack 1972:11). His close readings, in other words, occur as a claim to see. Repeating Marshack's necessary error, Elkins demonstrates the irreducibility of reading to seeing only by contradicting his own demonstration. It should go without saying that he can hardly be blamed for enacting the very predicament he has diagnosed.

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I am convinced that there is no such thing as a close reading, if that means (as Elkins would seem to imply) the exhaustive scrutiny of every minute detail of a phenomenon. I am also convinced, thanks to Elkins, that there is something utterly wrong with Marshack's interpretation of prehistoric artifacts. But it seems to me that the second fact, which concerns one particular case, can have no bearing on the first, which should be the result of epistemological considerations. Although he claims otherwise, Elkins has written an essay which could be read simply as a critique of Marshack's work.

I could make a long list (but there is no space for that) of the points on which I agree with Elkins (although I cannot claim to possess the thorough knowledge of Marshack's work which he apparently has), but I would put my emphases elsewhere.

Like Elkins, I have my quarrel with Ginzburg. My critique of the latter would be much more severe, however, because I think that is the key to the whole issue. When Ginzburg presented those "far-flung practices" of interpretation which Elkins does not want to call a method (characteristic of Morelli, Freud, and, it should be remembered, Sherlock Holmes), he took pains to distinguish them from linguistics, which has managed "to avoid the qualitative, that prime hazard of the humanities" (1983:93). As I have shown elsewhere, this claim is absurd, for, if anything, what structuralism (which is

clearly the kind of linguistics he is thinking about) brought to linguistics, and later to semiotics, is precisely a qualitative approach. Indeed, phonology, the first of the semiotic sciences to be invented, is distinct from phonetics precisely in that it is qualitative where the latter is quantitative. Phonetics is able to measure the minutest details of the composition of a sound and thus capture its individuality well beyond the capacities of the human ear. Phonology introduces discontinuities, that is, qualities or categories, in the places where they are perceived by humans, which are often different in different cultures (Sonesson 1989:27, 347).

Ginzburg's claim that linguistics must be qualitative because it retains only "the reproducible features of a text" shows a strange conception of quality: it stands to reason that each of the repeated instances must be quantitatively different but qualitatively identical. Contrary to what Ginzburg supposes, then, quantity goes with individuality and quality with the general case. Something which cannot be repeated cannot mean anything, for there is no way in which it could be conveyed from the creator's mind to that of the interpreter. Even Sherlock Holmes, whom Ginzburg enlists in his cause, actually reasoned from one singular fact to another by means of general rules taken for granted, that is, he made what Peirce calls abductions. His knowledge, which Watson found so unsystematic, was of a general kind and thus capable of serving as the middle term of the abductions he was interested in—all of which goes to show that even Holmes (and his role model, the physician) relied on general categories to make sense of minute details.

Having devoted a whole book (Sonesson 1989) to demonstrating the dangers of the linguistic model in semiotics, I am not to be suspected of trying to reduce the latter to the former. Yet I think there is one respect in which the linguistic analogy remains valid which has not been given its due: in suggesting what kind of science semiotics is and thus demonstrating its originality as an approach to the humanities. In one of the few passages which the French structuralists were not in the habit of quoting, Saussure pointed out the difficulty of linguistics and, as he said, "the other semiological sciences": they are concerned not with material objects but with the point of view taken on those objects (see Sonesson 1989:26). Sound waves as such are of no avail in phonology, but their meaningful divisions are. Analogously, pictures, as I have shown (1989, 1994b), cannot be purely "dense" systems, in Goodman's sense—that is, they cannot be continuous, in which case they would not be pictures of anything.

An analysis of Marshack's interpretations cannot confirm this conception, but it may be used to illustrate it. When Marshack makes his "close readings" he gets lost in details, even taking to measurements of which he cannot himself make sense later. This fact, demonstrated by Elkins throughout his article, seems to me to contradict his second claim that in close readings (alleged ones because we have agreed that there are no real ones) artifact and interpretation are the most difficult to separate. On the contrary, the continuities of the ob-

served details stand out against the discontinuities of the system of interpretation. There are actually two problems here: one is that Marshack claims to observe something without the aid of any scheme of interpretation, and the other is that the scheme he eventually introduces does not account for his putative observations (or, of course, for the additional ones made by Elkins).

In the natural sciences, numbers become meaningful because they are correlated with a theoretical grid. Fortunately, this grid may take any form as long as it is coherent. In the humanities, however, the grid cannot be arbitrary, because it must mimic the "point of view" taken by the users themselves. It is possible that Marshack's lunar calendar is identical to the principle of pertinence used by prehistoric humans, however implausible that may seem from his observations. From the point of view of pictorial semiotics, von Däniken's claim that certain pretechnological images show wrist-watches is much better substantiated than Anati's interpretation of a stick man with raised arms as a "prayer" (Sonesson 1994a). But, of course, once we go beyond the pictorial interpretation system to the culture as a whole, von Däniken's assumptions are shown to be absurd and those of Anati a little less so. Similarly, it is not from the markings themselves but from a wider context that Marshack's calendar must be justified.

It is not because he observes them more closely but because he compares the artifacts with the lunar scheme that Elkins discovers the deviations of the former from the latter, and it is in this itself "deviant" sense that he may justly claim to make an even "closer" reading than Marshack. This shows that schemes of interpretation do not necessarily blind us to the details but may actually be the very instruments which permit us to see them (see Sonesson 1995). I have used this method myself in criticising the classical analyses of pictorial semiotics from Barthes to Floch (Sonesson 1989). What I regret is that Elkins does not try to find a better fit for the deviations which he discovers. At least in one of the possible interpretations of his phrase, I would thus also contest the second part of Elkins's first claim: it is possible to choose a distance closer to the artifact at will, and Elkins himself does so in noting the deviations from Marshack's scheme. And it is precisely in this way that we make progress in interpretation: by criticising the analyses made by others (and by ourselves).

I also have my misgivings about Elkins's third claim: it is to be suspected that putative close readings will use basic terms in a chaotic and contradictory way, which is exactly what Elkins shows Marshack to do. In taking a theoretical stance, it is possible, as I have shown elsewhere, to make perfectly good sense of the difference between signs and nonsemiotic elements, although the distinction is certainly different, for instance, in the cases of linguistic and pictorial signs. And if we want to find out the difference between decoration and notation, it is much better to start from a theory about the nature of surfaces, for instance, that of Gibson, which is exactly what I have tried to do in my work (see Sonesson 1989).

I thus find myself in complete disagreement with Elkins's theses and in complete agreement with his demonstrations.

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I find Elkins's essay tremendously stimulating for the range of interpretive activities from different fields that he brings into one sparkling discussion, for the way he articulates the problematic character of the attempt to fix any notion of a "closest" reading, giving a wake-up call to the reader who might have too quickly and unreflectingly settled on one such notion, and for the "closeness" of his interrogation of Marshack's marks, method, and critics and his ability to conceive new ways of thinking about things that seem already settled ("Why can't notation be the norm and decoration be the uncommon counterexample?"). Above all, I admire the way he evokes, through the focus on Marshack's work, a vivid feeling of the meaningful mark at the limit of vision (not only personal but world-historical), the mark hovering undecidably between meaninglessness and sense that has already (but *just*) been born. Since I am not an anthropologist, I can relish precisely the fact that Marshack's marks might be construed as being significant signs and then again as not. What I relish is not the "undecidability" (an unfortunately fetishized quality in some literary theory of the recent past, thanks to de Man) but the specific quality of this undecidability that stimulates the feeling of the meaningful mark in its chrysalis state, struggling like Michelangelo's "slaves" to emerge from the stone.

At the same time, I feel some reservations about the way Elkins has put all this together. The question about Marshack's work concerns most fundamentally not the particular interpretation of his purported notations but whether they are notations at all. "Close reading" as it is defined in literary criticism concerns the reading of what are uncontroversially defined as meaningful, interpretable texts. I fear that the use of the same term to speak of the inaugural recognition of marks as meaningful notations might be something of a metaphorical extension, one that leaves over more disanalogy than illumination. That Elkins is not taking sufficient account of this difference seems to me implied by his use of Wittgenstein to defend the absence of evidence for a notational system in the Paleolithic artifacts. Wittgenstein's "stories" about the cave-men, Elkins says, "show that regular actions need not follow any regular rules," but what Wittgenstein actually addresses in the passage cited is not the question of a rule but of what he calls the "expression" of a rule—which appears to leave open the possibility that the cave-man is following an "implicit" rule. Wittgenstein then also says that we can see the regularity without ourselves being able to formulate the "expression" of the rule—an obscure observation since, in the examples given, *insofar as they*

possess recognizable regularity, it is easy to formulate the rule (thus a simple "add one dot" rule generates the second sequence). Wittgenstein might mean that it is *not necessary* to be able to formulate the rule in order to recognize the regularity, but that doesn't mean that it is structurally impossible to formulate the rule. It is true, as Elkins writes, that "private 'amusements' do not have rules in the ordinary sense"; but if they really are *private*, if they are not structured by a formulatable "code," then they are, as Wittgenstein consistently holds in the *Philosophical Investigations*, incapable of being interpreted or shared.

And yet I am thrown back into indecision by the suggestion for which Elkins makes room by the (I think, inaccurate) reading of Wittgenstein: "Why assume that the notations were referred to later, or that they could be read? Perhaps these notations were kinaesthetic: their meaning . . . was in their making." All sorts of thoughts rush in here: I think of "action painting," of braille; most of all, I feel the excitement of the possibility of an incredibly intimate encounter through these private, kinaesthetic marks—precisely because they were private—with a human being from Paleolithic times, his or her physical registration (seleno-seismographic) of the observation of the night sky. But is it really possible to get into the head of an individual cave dweller in this way? On the one hand we have the completely aleatory nature of the inscription, on the other hand the vast shared reality of the phases of the moon. The objectivity of the latter seems to render intelligible the possibility of feeling our way into the former. In the end, I don't find this possibility intelligible after all; yet Elkins opens a space for doubt.

But it is just his ability to read in such a resourceful way—a way that opens out instead of closing off possibilities—that I find at odds with the way he shapes his discussion with reference to de Man. The distinctive character of de Man's readings is not their *closeness* but their *programmatically* character. What de Man calls "rigor" is really the inflexibility of his program, which, as numerous critics have noted, always finds what it is looking for in every text it reads. The comparable thing in anthropology would be, for example, the resourceful Freudian who always manages (by "closer" reading) to rediscover the "form" of the Oedipal triangle beneath the "surface" differences in social organization in different cultures. In this respect, there is indeed an analogy between Marshack's and de Man's readings, but it shows nothing about the closeness of close reading; it is only that we have here two rigidly programmatic readers. Thus I have to reject the conclusion at which Elkins arrives, that "an unsurpassed close reading is an absolute refusal to negotiate the terms of reading," which seems to me to arise from the conflation of the notion of "close reading" with those of "program" or "method." I see no reason to accept de Man's claim, cited by Elkins, that attention to the "code" of literature "has to acquire the rigor of a method"—at least not in the sense intended by de Man. The development of narratology, poetics, and semiotics as analytical methods in literary

study have indeed been essential developments, but insofar as they have conferred greater "rigor" on the study of literature they have done so in a *neutral* fashion; these methods do not dictate or predict the *content* of any specific reading of a specific text as de Man's "formalism" does. As everyone today knows, no "reading" of a "text"—whether of a poem, of a culture, or of the "book of nature"—is possible without a "paradigm," "theory," or "preunderstanding" of what is to be read. But this necessity is of a *very* general sort and does not license arbitrary refusals to negotiate the terms of reading.

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Elkins has used two definitions of "close reading." The fullest is "the vigilant application of a central methodology of any discipline that works with what it takes to be a structured 'systematic' artifact of any kind." The second, stated in the summary and implicit throughout, is something like "responsible description preceding interpretation." The second is more general than the first, and both, he concludes, are incoherent and at least structurally "pathological." I cannot examine the several debatable statements about the semiotics of visual images that are made here. In general, however, I feel that the adoption of the model—ultimately Derridean—of literary language as representative of all signification is unjustified and that this model cannot be applied with the absoluteness Elkins assumes. It must be explained why Marshack's fundamentally indexical inferences are to be reduced to such specialized linguistic terms.

As I understood the issue, "close reading" was the ideal of the New Criticism, which prevailed in the academic study of literature before deconstruction. However "close reading" might have been done by one critic or another, it was based on the assumption that the work of art was autonomous, an imaginative unity of aesthetic interest. "Close reading" was related to modernist formalism in art and to "formal analysis" in art history and criticism and was regarded as the appropriate critical response to a work of art *as a work of art*. As the modernist conviction that art *is* in fact autonomous took hold, specialists—critics—appeared to explicate works of art *in themselves* (and by related means to trace an "internal" history of art). Paul de Man (the authority in this argument) offered another definition of "literature," which is not formal expressive but rather semiotic. By separating "grammar" and "rhetoric," de Man departed from earlier semiotic critics and made the move from structuralism to poststructuralism and deconstruction. "Grammar" describes comprehensible (but by no means unambiguous) statement; "rhetoric" is inherently open, and its figures and tropes are essential, de Man suggests, to the code of literature. The semiotic analysis of literature in these terms permits a kind of "close reading," which, however, inevitably leads to

aporia, never to the "unity of form and content." In the face of an interpretation of literature that claimed to be exhaustive (structuralism), de Man might seem to be reaffirming the old consumer-oriented "aesthetic" view of art, Kant's preconceptual heightening of our imaginative faculties. In de Man's terms, however, the work is never a "symbol" in a Kantian sense and the reading of literature as literature leads not to reference but to an "anxiety (or bliss) of ignorance." This "ignorance" is positive, and it makes us aware that any number of readings are possible and that any reading that pretends to be definitive is reductive and wrong-headed. Literature is language about language that makes us conscious of the nonreferentiality of language.

It is not easy in my view to transfer the semiotic principles of this episode in the history of literary criticism to all description, explanation, and interpretation in all fields, unless one is willing (as some are) to say that description, explanation, and interpretation are indistinguishable and equivalent to the interpretation of linguistic signs. This requires demonstration. In the meantime, it is centrally important that de Man is talking not only about texts he can *read* but about *canonical literature* when he argues for the impossibility of "close reading." Marshack is doing something very different. The implicit definition of art is much broader and simpler. It is based on indexical inference. Here, among artifacts agreed to be Palaeolithic, made over a period of 25,000 years, there are more or less similar configurations of serial markings. The surfaces and markings with which he is concerned are irregular, and it must be supposed that tabulation and signification and their precondition, planarity, were being worked out at the same time. Marshack seems to me to have the deepest respect for the presemiotic primordially of the objects he is trying to unriddle. We cannot, he says, assume that we are dealing with number systems, but *if* marks are being made separately and in series—not all at once—then it might be supposed that time is being kept in some way and that this time keeping is related to other "time-factored" activities (hunting, storytelling, ritual, migrations). In general, the numbers of grouped marks fall around the numbers of lunar cycles, if not exactly. Using this framework other evidence may be provisionally organized. In this way Marshack is able to offer what we later *Homo sapiens* also find congenial, an aid to pattern and process by story.

If the categories used in description are pointed up in the practice of description, this means to me that we can learn from the past and provides the opportunity to do history on two levels, one involving our own position, a classic turn of the hermeneutic circle. I agree that categories such as "sign" and "decoration" should be carefully reexamined, although it seems clear to me why they assume the secondary role they do in Marshack's attempt to provide an account of time notation. He in fact provides much material for such reexamination. In short, while "close reading" may have passed from currency in literary criticism, I cannot see that careful description is therefore rendered hopeless, impossible, or pathological in other fields.

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Elkins provides us with a provocative and interesting case study of what he describes as "close reading." As in Marshack's work itself, the greatest interest of Elkins's article and its most serious flaw is that its perspective is drawn from outside the bounds of anthropological archaeology. Two dimensions of an anthropological perspective are absent from Marshack's work and from Elkins's critique: (1) a profound appreciation for cross-cultural differences in representation (ways of seeing and ways of representing) and (2) the notion of different but interdependent levels of organization in cultural production, inextricably linking so-called internal analysis (close reading) to observations at other levels.

The very idea that a distinction between notation and decoration (or between decoration and representation, or between realism and abstraction) has any validity outside our own Western tradition has been recognized as problematical at least since Boas's (1955 [1927]) *Primitive Art*. Many later works in symbolic anthropology have problematized the notion of "decoration," while traditional art history "ghetto-ized" "the decorative arts," viewing them as less meaning-laden and more "purely esthetic." Of course, the notion of the purely esthetic, like Marshack's frequent and dismissive use of "mere decoration" (in contradistinction to "notation" or counting), is generally absent from the conceptions of anthropologists studying non-Western systems of representation. An excellent example is Munn's (1973) analysis of Walbiri iconography, the real-world referents of which are invisible to Western eyes. Far from being "merely decorative," Walbiri signs constitute a complex, dynamic, and flexible "language of forms" (Leroi-Gourhan 1993). The point is that the construction of meaning is very much a cultural matter. The melding of conventional tools and habitually memorized gestures (White 1994) into techniques (Leroi-Gourhan 1993) for the creation of meaningful forms yields enormous redundancy within any context of cultural production. In small-scale societies such as those of the Upper Paleolithic, learning contexts are primarily ones of observation and emulation and carry the heavy weight of tradition. The result (for example, in Magdalenian engraved animals) is a strict grammar for the construction of "visual realism."

If the construction of meaningful forms is largely a cultural matter, we are confronted with the second dimension of an anthropological analysis missing from Marshack's and Elkins's work: the contextualization of a given set of forms with respect to existing ecology, technology, social dynamics, broader representational repertoires, and belief systems. An anthropological close reading, then, is not merely a different but equal level of analysis but a set of observations at one level of human action and organization that must be integrated into and understood in terms of other levels. When Marshack distinguishes natural marks from human-produced marks or identifies marks made by different tool points, more

than pure observations *must be* involved. An external understanding of conventional tools, techniques, and gestures, raw materials, tool breakage patterns, stability of working surfaces, and spatial strategies for engraving is required. There can be no such thing as "internal analysis" without a thorough understanding of such "higher-level" variables, and, so far as I know, knowledge of such domains cannot be gained without experimental research (O'Farrell and White 1995) designed to create bridging theory between observations and interpretations (fig. 1).

An experimental baseline is replaced in Marshack's work by a faith in the purity of observation. For him the processes by which marks were created are self-evident. As an alternative research design to emulate, I encourage CA readers to examine the corpus of "close readings" produced over a 20-year period by the late Léon Pales (1969, 1976, 1981, 1989). Pales's work is characterized by (1) openness to alternative readings, (2) experimental ingenuity, (3) contextualization of observations with respect to variables such as stone technology, archaeological chronology, animal anatomy, and ethology, (4) methodological innovation (e.g., positive clay impressions of engraved surfaces), and (5) representational rigor (composite renderings combining differentially lit views of engraved surfaces). The fact that Pales's extraordinary research is presented with extreme modesty and published in French should not prevent Anglophone scholars from recognizing its merits.

The presumption of observational purity is buttressed in Marshack's work by his conveying of the impression that a photomicrograph is *representationally pure*. We are led to believe that Marshack's photos are indisputable depictions of reality when in fact they are *visual arguments* which can be countered by other visual arguments. Objects are lit, magnified, photographed, cropped, photographically enlarged, captioned, and accompanied by text in support of the position being advo-

cated. This is not in any way to suggest purposeful sleight-of-hand on Marshack's part but rather to point out that his interpretive agenda leads him to see one of numerous possible patterns in a kind of optical and cognitive selectivity. Preferential lighting is a revealing example. There are several instances in Marshack's work (O'Farrell and White 1995) in which unidirectional skim-lighting has been employed, the shadowed detail then serving as a basis for description and comparison. Several engraved marks on the famous Blanchard plaque and other similar pieces from the same site appear to be crescent-shaped in Marshack's photographs and are presented as such in accompanying descriptions and drawings. However, on close microscopic inspection and under alternative lighting (fig. 2) these often turn out to be circular marks one wall of which casts a crescent-shaped shadow.

In what I take to be Marshack's mistaken quest for *internal analysis*, archaeological context often becomes irrelevant. For example, the Blanchard plaque, which O'Farrell and I (1995) have studied from the perspective of experimental technology, is but one of about 15 punctated bone, antler, and ivory objects from the Aurignacian 1 of southwestern France. Six of these are facsimiles of seashells in ivory on which linear arrangements of punctations, executed in precisely the same way as on the Blanchard plaque, appear to represent the natural patterned surface of the shell. Moreover, most Aurignacian "engravings" on limestone, few of which are identifiable as to subject, are in fact linear sequences of punctations, not sharply engraved lines. What, then, makes one linear sequence of marks perceptible as a line or pattern having representational potential and another such sequence perceptible as calendric notation? Such questions are posed by a critical examination of Marshack's work but are almost never explicitly raised by Marshack himself.

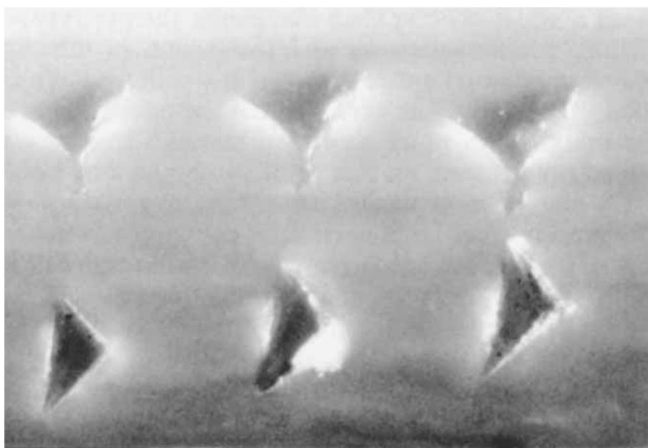


FIG. 1. Highly uniform marks that imitate almost precisely the shape of the tool point ($\times 16$) created experimentally by indirect percussion. This type of punctation replicates those on some pieces from the Magdalenian, such as the heavily engraved bone from La Marche (photo R. White).

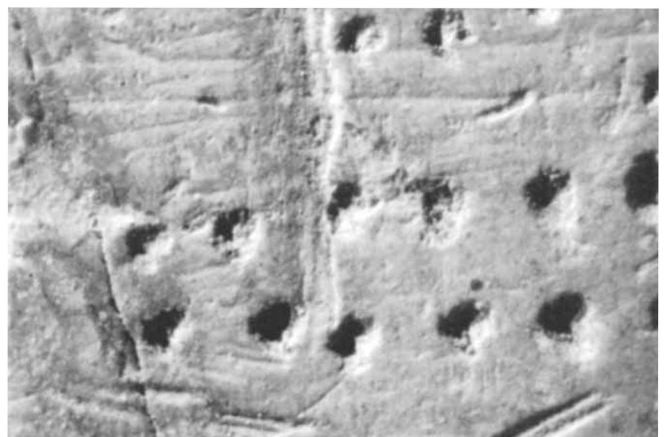


FIG. 2. Differences in punctation morphology can be created by changing the direction, angle, and intensity of the light source. Circular punctations are made to appear crescent-shaped in this photomicrograph of a segment of the Blanchard plaque (cf. Marshack's [1991:46–48] photomicrograph and accompanying drawings) (photo R. White).

In the end, who can argue with the fact that Marshack has forced us to examine the record of Paleolithic art at a very different scale? Moreover, who can argue with Marshack's journalistic and photographic skill in textually and visually representing his position? I must admit that, as a sophomore some 24 years ago, I was genuinely excited by and attracted to Paleolithic symbolism by Marshack's skill as a science writer. But in my opinion his anthropological contributions have been much overstated, especially by those quoted in the preamble to the present paper, and I think that Elkins is overly generous here in confusing the advances that have resulted from various probing critiques of Marshack's work (d'Errico 1994, Chollot-Varagnac 1980, O'Farrell and White 1995) with the relatively modest theoretical and methodological contributions of the work itself.

Reply

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In the months since I wrote the essay, I have had the benefit of several conversations about Carlo Ginzburg's essay on clues and historical method (Ginzburg 1980), which continues to be the *locus classicus* for discussions of close reading. It now seems to me even more fragile than it did then, and I doubt that it can serve as a stable starting-point for discussions about evidence, method, clues, or close reading. In one typical but telling example, Ginzburg enlists Mesopotamian divination using sheep livers as an example of "lower" or "empirical" methods, but such divination was a highly systematic, maplike practice complete with diagrams, clay models, and strongly abstracted geometric schemata. In short, it had all the characteristics of the opposing "higher," scientific method. Whatever the "lower method" is, it does not seem to be a method, and I have begun to doubt that it is a single practice in any useful sense.

I say this at the outset by way of an answer to Baxandall's beautifully phrased reply, in which he points out that close reading in the sense I employ it is truly pathological and fails to correspond to any actual looking. I could not agree more: looking—and even looking that is determinedly detailed, vigilant, or otherwise "close"—is much less predictable and uniform than what I have described. It would be interesting to try to give a phenomenological account of an extended and successful act of close reading, in order to say what really happens before art historians or archaeologists issue their apparently systematic and self-consistent results. Certainly it would look nothing like Marshack's microscopic forays. I also share Baxandall's interest in William Empson, whose work I think is a model of close reading in this more realistic sense (even if he is often preternaturally energetic in his attempts to get at every last shred of ambiguity). But it is worth saying that the phrase "close

reading" in my essay is primarily intended not as a plausible model of ordinary, or even exemplary, looking but as a "case" whose extremity illuminates a common unexamined ideal in many encounters with visual artifacts. It's not so much a *kind* of reading (though it sometimes serves as a heuristic beginning for reading, or a check on reading) as an extreme state at the borders of reading. That is also why I did not emphasize that de Man's readings are "rigidly programmatic" (as Staten says) and therefore inapplicable to more normal reading. It's the ideal that is at issue. We all wish, I think, to be able to see and describe as fully as Marshack and with the same piercing attention and untiring concentration—without knowing quite what that entails, and without wanting it single-mindedly enough actually to achieve it.

Ginzburg's model is frail, because the time he has to invest in his argument in order to make it appear that there is a "lower method" makes me wonder if the method exists, just as Baxandall's observations remind me that the truth is more elusive (and, ultimately, more seductive) than any systematically close reading. I take up this question at length in a forthcoming book (Elkins n.d.) These are essential issues, but they do not affect my sense that the words "close" and "reading" are not liabilities to the argument I was proposing.

In Baxandall's and Staten's comments, "close" is a synonym for "rigid" or "programmatic." To other commentators it seems like a synonym for "narrow" or "restricted." Carrier points out there are closenesses that are entirely different from the ones I entertain, such as being close to a person. (It's interesting that a "close reading" of a person is a semiotic act: in fact it is the original semiotic act, in Hippocrates' sense.) In a similar vein of skepticism, Godden says that what I mean is a text-based hermeneutics that tends to minimize references to the world. Most reading, he might say, posits a healthy dose of semantics to complement its syntactics. Marshack, Sonesson, and White make a related observation when they say that the lunar calendar is determined from a wider context than the marks themselves. For White, contextualization and integration of different kinds of material are essential in any fuller interpretation. "The point," he says, "is that the construction of meaning is very much a cultural matter." Marshack is especially insistent on this point, almost as if he would deny any value in reading an artifact in isolation—in performing what he himself calls an "internal" assessment. The thrust of his reply is that no image can be understood in isolation and that "historical" and "diachronic" changes and contexts must be brought into play.

I don't want to argue too strongly against these kinds of objections: though it may not seem so from my essay, as an art historian I am continuously working with contexts. Meaning is seldom well made when it is made in isolation—or, to put it more exactly, there is no such thing as isolation because both the artifact and the reading are always tied to *some* context. But at the risk of being misidentified as a formalist, I will register my

doubt that the breadth of a reading can somehow strengthen the baselessness of its most rudimentary assumptions about how marks make meaning. When the richness of cultures and contexts overwhelms an interpretation, dry foundational assumptions about marks or signs can start to seem irrelevant. But what reading can be immune from problems that infect its very terminology? Readings of society or culture can shore up underdeveloped notions of notation, decoration, and so forth—but they only defer the moment when we need to ask how our own sense of those terms drives our inquiries. In Marshack's terms, the "experts" have to convince the "novices" that historical parallels build stable defining contexts for assumptions about signs, notations, and so forth—assumptions that are implicit throughout the analyses. For example, I would want to know exactly which artifacts provide relevant examples of the parallel containing lines Marshack notes in the Taï plaque and how he decides which are close enough to what happens on the plaque to count as supporting material for his analysis (after all, in a sense "every notation in the Upper Paleolithic . . . is idiosyncratic"). And once I knew, I would probably want to say that the comparative examples only put off the moment when it is necessary to decide what, in a given context, qualifies as "sets of [notational] marks . . . engraved above and below . . . containing lines"—not to mention the even more difficult moment when it becomes necessary to ask *why* such distinctions seem natural.

In general, then, I resist objections that would shrink the argument on account of the word "close" until it applies only to a certain formalism or to a small segment of prehistoric archaeology. It's more the other way around: prehistoric artifacts are interesting in large part because the context is wispy and unreliable enough to force attention back on the rudimentary issues that inform any interpretation. I entirely agree with Marshack when he says that "the 'impossibilities' of close reading are, actually, the questions, uncertainties, assays, inner discourses and arguments, and tests that accompany the development of any innovative inquiry," but we disagree on the nature of those questions and uncertainties: to me, they circle back on terms such as marks, notations, and decoration. (In this sense I would concede each of his numbered objections, and I refer readers to them for clarifications of my informal list of notational properties.) True, the historical contexts define them and give them whatever significance they have in any particular instance, but that does not mean that historical contexts vitiate the obligation, or the possibility, of looking into the substructure of argument.

Other respondents locate crucial problems in the word "reading." Several wonder whether "close reading" is the right phrase for something that is, after all, "visual." Is there such a thing as "close looking" as opposed to "close reading"? It is a monstrously complex question, and it assumes a tremendous amount: that there is a nameable difference between reading and seeing, that seeing is known in some manner that is comparable to the way reading is known, and that there are only two

principal modes. Right from the beginning any division between "close reading" and "close seeing" runs into difficulties with the fact that Marshack's objects are entirely disjunct from the traditions of writing, "protowriting," and "prewriting." There is, of course, no reading in Marshack, but that does not mean it is any easier to say if there is "seeing." If there is a vernacular sense to "close looking," it would have to do with naturalistic images—something that is not at issue here. So without writing to read or images to see, I am not sure what gain there is in switching from "reading" to "seeing." If "notation" had a gerund, perhaps it would be closer to what I mean, but it would still be largely empty of practical meaning. In addition, many of the objects Marshack studies may not have been meant to be "read," traced, counted, or even recounted after they were made. For reasons like these it's necessary *not* to be too careful about the word "reading." Like "text," "image," and "artifact," "reading" functions in my essay as a place holder: it stabilizes the reference without tying it to a specific semiotic practice. I mean "reading" as an open-ended word, more like "encounter" than like "decipherment."

It's a slightly different thing to ask if Marshack or I manage not to rely on linguistic models. In a general sense I think we both do, even if I do not think anything specific in the argument relies on "the terms of language" (Lacour), on the historical specifics of de Man's poststructuralism (as Summers and Godden suggest), or on anything significantly or debilitatingly literary or linguistic. The apparent disconnection between reading and seeing, which Lacour stresses, is sometimes useful, though it is not sharp enough so that Marshack would have to "invent . . . grammatical forms . . . and rhetorical deformations, as semiosis and as trope," in order to fully "read" his artifacts (Lacour). What's puzzling about visual artifacts is that they are partly given under the sign of writing, but they never appear wholly as writing. I have been developing some ideas about the differences and similarities of written and "graphic" signs and "sub-semiotic" and "nonsemiotic" marks (Elkins 1995a); in general, the field is at an interesting stage, but it is well beyond the simple disjunction of "reading" and "seeing" that might appear to compromise an essay that seems to misapply "close reading" to "close seeing." As Summers points out, some of the things Marshack does are best referred to the "model . . . of literary language as representative of all signification," and, as Clark says, "the key question may remain that of verbal language." Yet I would resist subsuming sign systems under "reading" or "language," because that slights very specific nonlinguistic practices such as reading charts, numbers, equations, maps, and graphs. Ginzburg may be too holistic about "reading," especially—as Sonesson rightly notes—when he enlists philology as the principal humanistic example of the "higher" method. Reading is many specific, distinct practices, and none of them are clearly opposed to seeing. In the end the best reason that Ginzburg's essay should be treated more as a document of its author's predilections than as a viable

account of methods is that it glosses too many different approaches.

At the same time, "reading" in the general sense I use it is not entirely empty, and I would not want to give up its echoes of the ordinary meaning. As Clark points out, interpretations do take place within "some kind of linguistic field," but it is not at all a simple domain of literacy in some notational procedure: it shifts, as Clark says, "between registers of representation . . . from memory to prediction to inference to punctual insistence on the present state of things." It is something we cannot quite name, but it would be premature to call it "seeing" when seeing is even less well understood. To take the discussion of close reading further—beyond Ginzburg's models and beyond the informal sense I gave it in the essay—it is necessary to think more exactly about what "reading" in Marshack's case might be.

If Marshack isn't reading, what is he doing? Davis puts it most sensitively when he says that Marshack is "reading readability" or "taking the temperature" of a reading. In more Kantian terms, Marshack studies the conditions under which reading might be possible; or, to say it as Wittgenstein might, he thinks about how many different kinds of responses could count as reading. It's an acute observation to say that Marshack "is reading writing as an index-gauge of readability" and that an artifact might possess an index of readability or betray the "presence of Paleolithic-ese" without actually being read, being readable, or being writing in any sense.

It is not at all easy to go on and try to say what this "reading readability" is. On the one hand it could be argued that Marshack does nothing of the kind, since he does not interrogate what the signs of writing might be: he is not concerned to say what might distinguish the various species of "curious signs" and marks that I mention in the essay, and he is not interested in asking about his own assumptions about what reading or notation might be. In that sense he does anything but read readability. On the other hand, he wants to stay at a certain distance from the final act of reading, when the reader actually deciphers, translates, or otherwise understands the sequence of signs themselves. Often it appears that he keeps his distance for sober archaeological reasons, since the intentions of the makers, and much of the context of the making, are lost. But he has other, less easily described reasons for wanting to leave the act of reading open. One reason the "lunar calendars" are so difficult for commentators to describe is that they aspire to occupy the twilight between approaching an object with the intent to read and actually reading. It is not difficult to inquire into the assumptions a reader might hold at any given time regarding what is to count as mark, sign, legibility, and so forth; I did as much in the essay and elsewhere in more general terms (Elkins 1996). But Marshack's readings are something other than that.

And when, exactly, is reading not also "reading readability"? As Redfield points out, my reading also seems not to be a reading (since I don't propose new interpretations of the artifacts), but it is reading since I

take signs as "self-identical entities, available as such to the eye." (White also makes the excellent observation that Marshack's photographs are not evidence of any kind—I was doubting their veracity, not their status as evidence—but rather "visual arguments.") This is not the same as pointing out that I was searching for Marshack's assumptions but remaining unaware of my own: it is the claim that "reading readability" is always also reading. Redfield sees this as a move from "reading" to "seeing": he says I am "blindly confident" about my "claim to see" and that blindness is what enables my reading. I would probably rather think of this as a blind confidence in another kind of reading—a reading that is propelled by thoughts about reading.

These may seem to be overly subtle points, but to a large degree I think that they are the direction that needs to be pushed in order to carry this discussion further. Even though this response is the end of this particular forum, six notions might form the starting point of a more careful discussion of "reading" prehistoric artifacts—or visual artifacts in general:

1. *Contemplating reading.* Thinking of the possibility of reading, I might scan a surface for marks that appear to be meaningful. Some might present themselves as notations or even elements of calendars and others as iconic signs. It wouldn't occur to me to wonder about how I know such things, because I would be concentrating on the surface itself. From this point of view, the act of reading and the theory of reading are equally distant. This is one of the senses in which Marshack "reads readability," for example, when he surveys an artifact with notations in mind but with none in immediate evidence.

2. *Testing the waters.* Then it might occur to me to test the possibility of reading by trying out a reading on some part of the artifact. The experiment would not entail any awareness of principles: it would merely be a sample reading, a willful act of reading intended to discover whether reading (in some as yet undefined sense) is a good match for the marks. In Davis's metaphor, here reading is like sending up a weather balloon and seeing what happens; like the previous mode and the next one, it is also a way of "reading readability." Marshack's tests of the lunar calendar are this kind of activity, since they don't issue in the conclusion that the marks *are* lunar calendars.

3. *"Taking the temperature" of a reading.* Another mode, which mingles with the first two, is thinking about the plausible moments of reading. If an artifact seems partly well-behaved as a notation and partly wayward or incomprehensible, I might be drawn to think about probable and improbable acts of reading. Can the straight rows of vertical lines in the Placard bâton be understood in the same way as the "curious sign"? What can possibly count as reading? When Sonesson speaks about testing readings against the "instruments" that produce them, I would enlist sets of assumptions about particular acts of reading as instruments. When I use a barometer, I don't often wonder about how it works—but the match or mismatch between the barometer and

the weather changes my sense of the barometer and vice versa.

4. *Interrogating my sense of "reading."* A next step might be to turn my attention inward and begin to question my own criteria for legibility. That is what I did in the essay and what Marshack does when he argues with d'Errico and others. It is clear that this kind of thinking is not the same as actually reading, but it is often not as clear that it is also different from the musings and confusions of the first three modes. The apparent rise in self-awareness that goes with this kind of introspection was one of the points of my essay: blindnesses get shifted around, and suddenly many things are visible that weren't when I was reading readability or otherwise thinking of reading.

5. *Interrogating the interrogation.* Anything that presents itself as systematic can be systematically interrogated, and it might then occur to me to think about how I've been thinking about the criteria of reading. The infinite regression that opens here is nothing more than the commonplace questioning of motives and possibilities that goes along with any self-reflective method. It is the mode of each of the comments and of this reply. As Summers observes, a reading that proceeds by alternately finding meaning and questioning its own meaning is the precondition both of Marshack's "time-factored" notations and of historical writing in general.

6. *Simply reading.* Finally, I might just settle down and read an artifact. Though it often seems as if that is what Marshack does and though several comments imply as much about my essay, I don't think either of us actually does in the cases under consideration. In other contexts, both of us read—with unsupportable confidence and with complete oblivion about the other ways of thinking around and about reading. But the kind of reading I do in the essay (which Redfield identifies) is not reading in this ordinary sense: it is an inevitable unseen accompaniment to the other modes, and especially those such as the fourth and fifth, in which I would normally strenuously deny that I am reading.

I name these six modes to suggest that it is possible to think in a reasonably orderly way about the limbo between reading and "reading readability." Reading itself—the sixth response—is full of other kinds of problems: whether to read closely or cursorily, fast or slow, near or far, consistently or randomly. As Davidson points out, there are occasions for not reading closely, but in the unremitting logic of reading they have to be adjudicated by stepping outside reading and back toward the five other modes. Conversely, even the most abstruse meditations on reading lead back into acts of reading. Staten is right about my misinterpretation of the Wittgenstein passage: it's true that we can see regularity in marking without being able to state the generative rule or describe its "expression." But with the kinds of objects Marshack studies, the regularity of the marking is under suspicion just as much as the putative rule, or the lack of a rule, that generated it. Thinking about reading depends on taking time off actually to read.

I can think of at least two morals for the exercise: it

shows that reading is always also thinking about reading in various more or less muddled ways, and it suggests that disagreements about what Marshack does might often turn on exactly what a given reader thinks he's doing.

The deepest question here, the one with the widest application to the study of visual artifacts in archaeology, anthropology, and art history, is the problem of self-reflexivity. Whatever Marshack does, it is done while remaining blind to certain aspects of reading. Any reading—even the informal shifts of attention and focus that Baxandall mentions—depends on not seeing itself or not seeing itself equally at all moments. D'Errico's work, for example, is in every particular as dependent on assumptions about notation, mark, and sign as Marshack's: no amount of methodological care or technical innovation can avoid that. (I have no quarrel with d'Errico's new criteria for determining motions and sequences of points, but when it comes to drawing conclusions about notation and meaning his work raises questions exactly analogous to those I raised about Marshack's.) For his part Marshack is largely oblivious about his assumptions about marks, and I take that as a sign of the strength of his reading.

In the end there is little to be said for the single-minded pursuit of self-reflexivity. Some of my respondents are exemplary readers of visual artifacts, and they are well aware of the peculiar limitations imposed by perpetual self-interrogation. Self-reflexivity, which reaches a peak in some of the responses to my essay (for example, Redfield's or Staten's), has no necessary correlation with the performance of a convincing interpretation. I want to close by emphasizing this point, especially because it is so routinely lost in anthropology, archaeology, and art history, where writing is often taken to be scientific or driven by empirica. An account of a visual artifact does not endure only because it is found to be true or because it presents itself as unsurpassably reflexive, vigilant, or rational. Closeness, care, thoroughness, and even correctness are necessary but not sufficient for writing that aspires to last, and self-reflexivity is often a recipe for weak reading.

I realize that in talking this way I will lose some of the readers who participated in this exchange, since it is anathema in many fields to put the expressive value of writing above its truth value. But I want to insist on this point: if close reading operates as I have suggested, there is no *epistemological* reason to prefer one level or degree of closeness over another. What initially drew me to Marshack's analyses was his writing, and what provoked my "overly generous" assessment of his work was the power of his writing. Marshack can write with astonishing force, and his photomicrographs can be coercive "visual arguments." As far as I am concerned, there is still no book in the history of art that arrays its visual material with such compelling success as *The Roots of Civilization*. Ultimately it is that power, which scholars (excepting de Manians, among others) still denigrate with the word "rhetoric," that makes his work so intriguing. Part of its interest certainly comes from the

nearly incomprehensible position it occupies between reading and "reading readability" and from its various methodological lacunae and unanswered questions. Part also comes from its sometimes dubious claims about cognition, time-factoring, and the lunar calendar. Marshack's work is deeply flawed in these and many other respects, but it continues to be fascinating even for those who reject many of his conclusions: as several respondents point out, there are other writers on prehistory whose works combine Marshack's myopic precision with a higher degree of methodological consistency, a greater openness to rival hypotheses, and a more systematic interpretive agenda (Flamand, Pales), but few writers can bring so much of their encounter with the object into their prose. Any of us—in archaeology, anthropology, literary criticism, philosophy, or art history—would be lucky if we could face objects with such sustained force. In that respect, my essay was really an attempt to begin to understand how a truly successful account of visual objects might work.

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