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Citation

Bar-Yosef, Ofer and Philip Van Peer. 2009. The Chaine Operatoire Approach in Middle Paleolithic archaeology. *Current Anthropology* 50(1): 103-131.

Published Version

<http://dx.doi.org/10.1086/592234>

Permanent link

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The *Chaîne Opératoire* Approach in Middle Paleolithic Archaeology

by Ofer Bar-Yosef and Philip Van Peer

Since the pioneering days of Paleolithic archaeology in western Europe, the making of stone tools has received special attention. Numerous studies were aimed at creating systematic typologies of artifacts based on descriptions of their technical features and morphological attributes. Recently, the concept of *chaîne opératoire*, or “operational sequence” (sometimes called “core reduction sequence”), borrowed from French social anthropologists, has been introduced into the study of Old World prehistory. Its conceptual framework is focused on the recognition of the overall technology and the practical skills of the prehistoric knapper in employing a particular technique responsible for the transformation of raw material to tools. Although the stone objects of all periods received attention, those of the Middle Paleolithic—due to issues such as the significance of lithic variability in retouched tools, the demise of the Neanderthals, or the emergence of “modern behavior”—have been at the forefront. This paper discusses the definition of *chaîne opératoire* and its practice and demonstrates that as a system of classification, it is overformalized and provides but an illusion of reading the minds of prehistoric knappers. The need to pay more attention to the recognition of patterning in the technological information is essential if we wish to go beyond a formal type list of knapping products. We argue that an elaborate, complex typology of core reduction products and discrete *chaînes opératoires* is an approach that impedes informed behavioral interpretations by forcing a rigid framework of “technical” definitions on the prehistoric lithic technologies.

Since 1799, when John Frere announced the finding of human stone artifacts together with extinct fauna, Paleolithic archaeology has seen the study of stone tools as one of its major goals. In over two centuries of research, the study of the production and use of artifacts went through several phases (e.g., Sackett 1981). As the investigation of the nature of prehistoric sites and assemblages advanced and the interest in a theory-informed archaeological praxis arose, formal frameworks for standardized descriptions of archaeological observations were developed, culminating in D. L. Clarke’s monumental *Analytical Archaeology* (1968). While Clarke based his theoretical constructs on the study of later prehistoric periods and ethnographic records, one of the most influential approaches for the Paleolithic period was pioneered some years earlier by F. Bordes (1950), who created a type list for

the Acheulean and the Middle Paleolithic along with the basic definition of core reduction strategies. His endeavors were followed by those of other scholars in western Europe and the Mediterranean basin (e.g., Sonnevile-Bordes and Perrot 1953; Bohmers and Wouters 1956; Tixier 1963; Hours 1974; Goring-Morris et al. 1998). The quantitative and qualitative reporting of the typological paradigm marked a departure from the older “guide fossil” method in which a particular stone tool served as a cultural marker. Moreover, Bordes type list and the quantitative typological analyses substantiated the recognition of “cultural” variability among the Mousterian assemblages across western Eurasia (Bordes 1980).

Quite soon, the growing interest in the anthropological reality hidden in the archaeological record brought about serious doubt concerning the aptitude of the traditional morphological typologies to convey cultural interpretations. Ironically, it seemed that the gain in scientific analytical rigor had drawn the discipline away from its original goal. Rather than leading to an understanding of the simple facts of prehistoric life, the formal study of material-cultural remains appeared to have become isolated from its behavioral context. The systematic classification of artifacts and the construction of cultural taxonomies had only served the purpose of building descriptive culture history (e.g., Bordes 1953, 1961, 1980).

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This awareness triggered the development and ubiquitous establishment of lithic technological studies in both Old and New World archaeologies (e.g., Golson 1977; Tixier, Inizan, and Roche 1980; Honea 1983; Boëda 1988, 1994, 1995; Boëda, Geneste, and Meignen 1990; Pelegrin 1990, 1995; Schlanger 1990, 1991; Pigeot 1991; Sellet 1993; Dibble and Bar-Yosef 1995 and references therein; Andrefsky 1998; Bleed 2001; Shott 2003a; Soressi and Dibble 2003; Gamble and Porr 2005; Schurmans and De Bie 2007). Researchers felt that the nature of variability in tool morphology frequencies could not be properly understood when separated from the underlying dynamic processes of lithic production. This criticism motivated the practical and theoretical shift to the study of lithic technological variability in a different way, one more in line with the anthropology of technology (e.g., Lemonnier 1992; Pfaffenberger 1992; Dobres 2000).

Historically, the anthropological interest in technology emerged almost a century ago. It was studied as a component of human society and its way of living (e.g., Sollas 1915; MacCurdy 1926; Oakley 1961). Its archaeological development owes an intellectual debt to A. Leroi-Gourhan (Cresswell 1990; Schlanger 1990). Basically, technological studies in prehistoric research aimed at an understanding of how material items were produced. They have been concerned with both conceptual knowledge (technology *sensu lato*) and the practical skill (technique) involved in the transformation of raw material into tools (Pelegrin 1990; but see Ingold 1990; Lemonnier 2002). The process of recording and understanding the choice and selection of raw materials, the various methods of knapping hard rocks, the specific shape modification designed to obtain a set of products, and the spatial organization of lithic economy at a regional scale was considered a much more advantageous line of research for bringing us closer to understanding prehistoric artisans.

Some argued that the variability patterns laid out by this approach might reveal a “deeper” level of cultural choices apart from the functional constraints operating on typological patterning (e.g., Crew 1975). Also, in comparison with formal typological categories, technological data seemed to be less prone of being burdened with alleged emic meaning. To refit a flake onto a core or to determine the origin of a particular raw material is to provide concrete factual evidence about the past for anyone to observe and evaluate. With technological data, it became possible to reconstruct actual sequences of gestures and events. The acquisition of such data necessitated the analysis of all the components of a lithic assemblage in a much more systematic way than had been the case with the Bordesian system (Sackett 1986). Furthermore, individual sites or assemblages could not remain isolated from their regional context. They were to be considered as components within a dynamic spatial system through which raw materials and/or products arrived on site and eventually were left behind and/or removed from it.

While in recent years bone and antler raw materials are treated similarly (e.g., Camps-Fabrer 1988), it is the study of

lithic technologies that has become standard procedure in contemporary Paleolithic archaeology. Most parsimoniously, lithic technology as a system finds its place at the beginning of an overall economic *chaîne opératoire* of a Paleolithic group, destined to play a role in subsistence activities or in the production of nonlithic items. However, it cannot be overlooked that stone items were endowed with symbolic meanings and played a part in ritual activities. This is a poorly studied topic that is often raised when discussing modern humans of the Upper Paleolithic and later ages (e.g., Caneva et al. 2001). A considerable amount of methodological and epistemological impetus has come from the research of Middle Paleolithic sites and industries. Clearly, this was motivated by the problems of classification and of the attribution of meaning to typological variability encountered here. Also, the presence of two human morphotypes in Mousterian contexts—namely, Neanderthals and early modern humans (such as the Skhul-Qafzeh group)—raised issues of the relationship between biology and toolmaking. Furthermore, it seems that by comparison with the earlier Acheulean (or “core-and-flake”) industries, the technological paradigm provoked the most profound changes in both the nature of lithic analysis and behavioral interpretations. Accordingly, the Middle Paleolithic will be the main topic of our paper.

In the following pages, we will discuss a few problematic issues regarding the application of technological studies and suggest some potential fields of inquiry that can be tackled with technological data. For this purpose, we first describe the nature of the technological paradigm *sensu stricto*, alternatively known as the *chaîne opératoire* approach. We then evaluate its theoretical rationale and its analytical methodology to show that it is essentially typological in nature and that technological categories are in danger of becoming the icons that tool types once were.

Chaîne Opératoire

A Definition

Broadly defined as the study of how lithic items were produced, lithic technology can encompass a range of topics. For example, it can be taken to mean a technical study of percussion instruments, the analysis of “waste” elements and other nonretouched items in an assemblage, or the identification of stone-tool functions. Of that range, the description of the spatial and procedural dynamics in the lithic production process became equated with the *chaîne opératoire* approach, and as the term indicates, it has been often perceived as specifically a French school contribution to archaeology.

The *chaîne opératoire* concept derives from the work of A. Leroi-Gourhan (Audouze 1999, 2002) and studies by R. Cresswell (1983, 1993) and others (e.g., Lemonnier 1992, 2002), and it was adopted by French prehistorians for the purpose of lithic analysis (e.g., Geneste 1985; Boëda 1988, 1995; Boëda, Geneste, and Meignen 1990; Pelegrin 1990; Pigeot 1990, 1991;

Schlanger 1996; Inizan et al. 1999). However, similar analytical methods, although termed differently, were adopted around the same time by others elsewhere in Europe, the Near East, and in the United States (e.g., Crew 1975; Schiffer 1975; Munday 1976, 1979; Fish 1979; Jelinek 1991; Bar-Yosef and Meignen 1992; Meignen and Bar-Yosef 1992; Van Peer 1992, 1995; Sellet 1993, 1995; Marks and Monigal 1995; Meignen 1995; Hovers 1997; Meignen et al. 1998; Kerry and Henry 2000). Some claim (Shott 2003a) that it was introduced into the American archaeological methodology as early as the end of the nineteenth century (Holmes 1894, 1897).

The term *chaîne opératoire* is often used untranslated, although the terms *work chain* (Cresswell 1990) or *operational sequence* (Perlès 1992; Dibble and Bar-Yosef 1995; Chazan 2003) have been proposed. Here we will use the latter term, abbreviated OS, and speak of the “OS approach.” The basic principle on which the method relies was defined by Tixier, Inizan, and Roche (1980, 8):

Technology is not typology. It takes into account the entire lithic material without preferentially isolating what we choose arbitrarily to call “tools.” It places each item in the sequence of technical actions beginning (after its conception and prior contemplation) with the raw material and ending with the abandonment, the “death” of the tool assemblage. Even when fragmented into thousands of microliths and “debris,” a lithic assemblage always forms a coherent whole bound together by a methodical scheme. [Our translation from French]

This definition touches on two basic points regarding technology. First, technology is different in scope from typology, and second, an assemblage of lithics is not a random but a methodically interconnected association of artifacts. It is instructive to recall this original definition as it seems to have been interpreted in different ways in subsequent descriptions of the OS approach. Thus, at present there seems to be some confusion concerning the epistemological meaning of the term *chaîne opératoire*. For some, it is mostly a theoretical concept reverting to a basic underlying idea about technology that “the real existence of the tool is when it is in action, when it is animated by gestures” (Schlanger 1990, 20). Others conceive it as a concrete methodological procedure. For Geneste (1989, 76–77), the *chaîne opératoire* is a technological classification system according to a general model elaborated

with support of technological observations and refitting of the lithic material, both confronted with experimental reconstructions. . . . The notion of *chaîne opératoire* is therefore the means to chronologically organize the process of the transformation of raw material obtained from the natural environment and introduced into the technological cycle of production activities. [Our translation from French]

The other part of the original definition has sometimes been inflated to imply that an assemblage is essentially char-

acterized by one overall strategic or methodical template of lithic reduction (Delagnes 1995, 202, citing Pelegrin 1986):

The principle behind such a [*chaîne opératoire*] analysis is to study the entire assemblage, placing each piece in the [our emphasis] reduction sequence. It is based on core refitting or, when this is not possible, on mental reconstructing of the [our emphasis] reduction sequence.

Below, we will discuss this assumption.

Within the general OS approach, a distinction has been made between a technoeconomic and a technopsychological study (Boëda, Geneste, and Meignen 1990, 43). The former deals with the spatiotemporal dynamics of lithic reductions at the regional geographic level (Geneste 1985, 1989; Jaubert 1993; Soressi and Hays 2003; Pope and Roberts 2005). The latter is concerned with the identification of lithic production systems as concepts of blank or tool production and the description of their variability (Boëda, Geneste, and Meignen 1990; Turq 1992; Boëda 1994, 1995; Jaubert and Farizy 1995; Bourguignon 1998; Peresani 2003; Loch 2004; Delagnes, Jaubert, and Meignen 2007). Obviously, the technoeconomic approach works from a broader perspective than technopsychology, where only the transformation of a volume of raw material into end products, regardless of where it took place, is concerned.

It is felt by many today that technopsychology in particular provides a pathway to the cognitive minds of prehistoric knappers and that it is one of the most informative and meaningful ways of conducting lithic analysis. However, the social environments of these knappers and the way they may have influenced their choices and decisions has generally not attracted the attention of the practitioners of the OS approach.

The Analytical Method

The originality of the methodological development that lies at the basis of the OS approach consists of the integration of a number of existing approaches to lithic analysis into a general framework (Inizan et al. 1999). The components of the methodological procedure are replication of core reduction sequences, refitting of the products of core reductions, analysis of scar patterns and superpositions, and technological classification. Each of these procedures is discussed below.

Replication

Experimental stone knapping has been an essential asset to the study of prehistoric technology in various ways (e.g., Amick and Mauldin 1989; Pelegrin 1990). Among the best-known flint knappers, one can enumerate F. Bordes, J. Tixier, D. Crabtree, J. Whitaker, B. Bradley, and M. Newcomer as well as many others (e.g., Crabtree and Butler 1964; Bordes 1967; Newcomer 1971; Newcomer and Sieveking 1980). Indeed, experimental flint knapping has been practiced almost since the inception of the prehistory discipline. It enabled

more than one generation of archaeologists to learn about the mechanics of obtaining a series of detached pieces from a nodule, the role of various types of hammerstones, and the properties of different raw materials.

Experimental work assisted by ethnoarchaeological observations (Roux 2007) is instrumental in the construction of the middle-range theory required to bridge the gap between anthropological questions and archaeological data. An experimentally derived standard distribution of technological categories, for instance, has been used to infer the spatial dynamics in the formation of archaeological lithic assemblages (Geneste 1985, 1989). It can also provide knowledge about how particular shapes of blanks and fully finished objects were achieved, although it must be acknowledged that this more often appears to rely on descriptive analogies than on any theoretical understanding of the physical principles involved in conchoidal fractures (see Bertouille 1989).

Replication served to demonstrate the variability of the production process of morphologically similar Levallois end products and accordingly helped to devise a technological classification system (Boëda 1986, 16). There is, however, room for much more experimental research. For example, the time length of apprenticeship required for the production of Levallois methods was not a subject dealt with by experimental knappers. Current estimates indicate that a novice practicing every day will need 6–12 months to acquire the skill of a flint knapper who can produce thin, symmetrical Levallois points similar to those uncovered in the Mousterian layers at Kebara Cave (M. Eren, personal communication).

Refitting

Refitting is obviously a superior analytical tool because it portrays the different and successive phases of the reduction sequence (Schurmans 2007). Thus, the process of reduction itself can now be observed and not merely (some of) its material by-products. Unfortunately, the potential of many Paleolithic assemblages for reconstruction through refitting is very limited. The further we go back in time, the worse the situation gets. In Middle Paleolithic sites, such as caves and most rockshelters, the palimpsests of occupations generally hamper the possibility of carrying out refitting that will not be too costly (in terms of funding) or too lengthy in time to perform (but see Bordes 2003).

Successful refitting efforts are known from a few Middle Paleolithic sites and many more Upper Paleolithic contexts. In certain cases, refitting was performed on Middle Paleolithic open-air sites where the assemblages were buried within a short time after abandonment, such as Beauvais (Locht 2004), Wallertheim A (Adler, Prindiville, and Conard 2003), Tor Faraj and Tor Sabiha (Henry 2003), and Fara II (Gilead 1988).

Assemblages from Upper Paleolithic open-air sites such as Stranska Skala (Skrdla 2003), Pincevent (Ploux and Karlin 1993), Etiolles (Pigeot 1987; Olive 1988), Rekem (De Bie and Caspar 2000), Boker Tachtit (Volkman 1983), and epi-Paleo-

lithic sites in the western Negev (Goring-Morris et al. 1998; Davidzon and Goring-Morris 2003) provided a wealth of information concerning reconstructed production systems.

Under every circumstance, refitting is a very laborious process, and often the percentage of the refitted elements within the assemblage is low (Cziesla et al. 1990). For example, the Magdalenian assemblages of U5 at Etiolles provided a mean refitting of 18.6%, which is considered as a successful case (Pigeot 1987). At the Middle Paleolithic site of Rheindalen B1, the extraordinary figure of 45.6% was obtained, but this involved a large number of broken elements that were put together (Thieme 1990).

Certainly one reason for low success rates is that the increasingly time-consuming effort is not justified by the amount of new information gained (Pigeot 1987; Bodu 2007). Also, the context of an assemblage at hand has a direct effect on the results. For example, short-term knapping spots where raw material volumes were locally reduced offer the best chance to obtain high refit ratios. This was, for instance, the case at the knapping scatter Q1/A at Boxgrove, where 65% of the products were refitted into two major groups (Pope and Roberts 2005). Another example comes from the site of Taramsa 1 in upper Egypt (Vermeersch et al. 1997), and we will treat this in more detail below.

The Analysis of Scar Patterns and Superpositions

The “stratigraphic” analysis of dorsal scar patterns on lithic artifacts, or *lecture des schémas diacritiques* (Boëda 1986, 16), is sometimes considered a new methodological development. However, its principle was already laid out by Crew (1975) and Dauvois (1976), and an analogous procedure was used by F. A. Hassan (1988) in a study of hand-ax symmetry. The temporal sequence of the technical actions is read from the scar patterns on both cores and blanks. Careful observation usually allows us to infer the relative sequence of removals. Hence, each product bears the physical evidence of a part of the overall reduction sequence. Given the fortunate feature of lithic production that each percussion act normally results in a flake, it is hypothetically possible to reconstruct a general model of the sequence of actions (Pigeot 1990) involved in the reduction of raw material volumes without actually refitting them. However, whether this approach can achieve an adequate representation of the sequences that were actually carried out is questionable (Volkman 1983; Dibble and Bar-Yosef 1995), as we will try to show with an example below. In any case, a sufficiently large sample of products must be analyzed in order to find a systematic recurrence of discrete scar-pattern groups (e.g., Meignen 1995; Meignen et al. 1998). When metrical attributes and statistical testing are added to this analysis, additional conclusions can be made (e.g., Tostevin 2003a, 2003b).

Technological Classification

The technological event made visible by the physical or mental reconstructions discussed above is usually supplemented with the typological identification of its physical remnants using technologically relevant attributes. In turn, the combination of these data serves a typology of reduction processes (Delagnes, Jaubert, and Meignen 2007).

The most widely used classification has been established for the Levallois system. E. Boëda (1994, 25) has stressed the paramount importance of core analysis, but usually Levallois blanks are also classified according to the disposition and order of predetermining and predetermined negatives on their dorsal faces. The type frequencies are commonly used to determine the relative importance of the various production methods in a given assemblage. Similar technological classifications have been devised for other reduction strategies, such as the discoidal and Quina systems (Geneste 1990; Turq 1992; Boëda 1994; Bourguignon 1998; Loch 2004).

Technological typologies (Dibble and Bar-Yosef 1995, xii; Chazan 1997) were also adopted by American archaeologists aiming at reaching a better understanding of the “life histories” of tools, a task that requires the detailed study of core reduction sequences and the definition of debitage categories (Andrefsky 2001 and papers therein).

In the techno-economic method (Geneste 1985, 1988; also employed by Jelinek 1991), there are 26 original classification categories organized in five reduction classes forming a temporal sequence. For instance, cortical flakes will be classified as stage I products, representing an early stage in the reduction sequence, and so forth. On the basis of the proportional representation of the stages, the spatial organization of lithic reduction in an assemblage can be characterized by comparison with an experimentally derived standard distribution, as mentioned before. A similar system of technological classification had already been used for the analysis of intersite variability in the Negev desert of the Near East, when proportions of core and debitage categories and morphometric data were employed to distinguish between functionally different sites and to describe the nature of lithic transfers between them (Munday 1976, 1979).

Clearly, the archaeological study of technology along the methodological lines above has significantly expanded and improved our understanding of Middle Paleolithic human behavior. However, in order to realize its full analytical potential, it seems to us that some problematic issues regarding the epistemology of contemporary technological studies need to be explicitly addressed. As a matter of fact, these problems are similar in nature to those that were the reason for the development of the technological paradigm in order to improve on the traditional typological approach. Moreover, the shortcomings may be worse, being disguised by this seemingly superior methodological framework.

Methodological Problems

While the concept of *chaîne opératoire* has the merit of soliciting a contextual and process-oriented perspective on lithic technology, its analytical method must rely in an important measure on the classification of individual items according to prescribed theoretical categories (Shott 2003b). This is based on the justified assumption that the morphologies of artifacts can betray both their technical function within a reduction sequence and the reduction method itself. For instance, a core with intersecting negatives of predetermined flakes struck from different sections on a Levallois core’s perimeter will be considered as evidence for a centripetal-recurrent method of exploitation. A technological typology is as valid as any other, but it is imperative that the characteristic products resulting from the application of the reduction methods we choose to define can be clearly identified. The validity of any typological system relies entirely on the clarity and objectivity of its classification criteria.

It can be seriously questioned whether this condition is fulfilled in the OS approach. First, a significant share of morphological equifinality must be expected in the range of morphologies shown by production debris. For instance, it is hard to believe that there could be predetermining products that are characteristic of the centripetal-recurrent Levallois method alone. Second, it is difficult to see how discrete criteria could be established to identify dorsal scars as particular types, for example, as negatives of predetermined Levallois blanks. This is a fortiori problematic when the concept of predetermination is broadened to include any shape that seems to be brought about by the conscious use of some pattern of pre-existing ridges (Boëda 1994; see Dibble and Bar-Yosef 1995). How could a scar, lacking attributes such as a striking platform and a bulb of percussion, be reliably identified as a negative of a Levallois blank? Yet such identifications are crucial in order to infer the reduction method applied. For example, the distal right scar in figure 1A would most certainly not be classified as a negative of a predetermined blank. However, when refitted to the blank in figure 1B, it appears to be part of the central distal scar that is indeed a Levallois blank negative.

In the absence of explicit classification criteria, it is not surprising to find considerable discrepancies between different analysts. This is most clearly exemplified in the distinction between centripetal-recurrent Levallois and discoidal cores (e.g., Pasty 1999). If this basic problem is not resolved, technological classification systems will not live up to their task to provide formal descriptions of the technical procedures represented by lithic products based on their observable features.

It is clear that the technological classification of cores and blanks will have an effect on the form that mental reconstructions of the *chaîne opératoire* represented in an assemblage will take. In itself, this virtual reconstruction poses another methodological problem. The important role played by

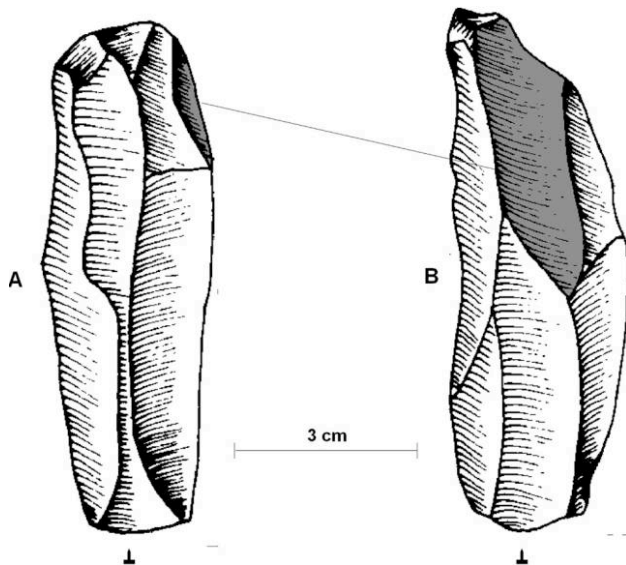


Figure 1. Blades from Taramsa 1, sector 89/03. Line indicates the fusion of two negatives after refitting of blade A to blade B.

the so-called Frison effect is generally well acknowledged. Like retouched tools, cores may pass through a number of different stages in the course of their reduction. Using again the example of a centripetal-recurrent Levallois core, its present technological morphology is only an indication of the application of that method during the last stage of the core's reduction sequence. Refitting is therefore essential to document strategic changes within reduction sequences and thus, eventually, to describe one or a few standard reduction types in an assemblage.

However, as indicated earlier, the same result can seemingly be achieved by the analysis of scar-type superpositions. The underlying idea is that when scars on the dorsal face of an artifact have been classified, it becomes a record of a partial reduction sequence. As an analogy, we can refer to dendrochronological sequencing whereby the individual artifact represents a tree section with an idiosyncratic sequence of rings. The recurrence of items with the same scar-type superpositions will be taken as evidence for the overall *chaîne opératoire* represented in an assemblage, eventually combining different reduction methods throughout the sequence. Next, the individual "sections" are assembled into this "master" reduction sequence that therefore is derived in reverse order from the latest generation of negatives on exploited cores.

In practice, the value of this reconstruction is severely hampered by the lack of classification criteria as discussed above. Furthermore, how exactly a master sequence should be assembled is not very clear. Arguments such as an appreciation of the core's state of reduction, size measurements (Boëda 1986), the preservation of the cortex on dorsal faces of early reduction-stage flakes are invoked, but it seems that often, groups of floating sequences are arranged into a temporal scheme just on intuition (Delagnes 1995). Clearly, the "model

operational sequence" risks being a construction in the mind of the analyst, never having been applied by the prehistoric knappers.

Testing for Analytical Coherence

Taramsa 1 is a chert exploitation site in the lower Nile Valley located on a remnant on a Lower Pleistocene gravel terrace. From the end of the Middle Pleistocene onward, chert nodules have been exploited here in pits and trenches in and around which debitage of the exploited volumes took place (Van Peer, Vermeersch, and Paulissen 2009). Our analysis is concerned with excavation sector 91/04, where a very dense cluster of late Middle Stone Age artifacts identified as Concentration 28 occurred. 1406 artifacts more than 2 cm in length were scattered over a small area of about 1 m². Twenty-six reduction sequences were almost completely refitted next to a few partially refitted flake sequences. The actual refit for this assemblage is 65%; when the artifacts that belong with certainty to one of the sequences but that were not actually refitted are taken into account, the refit rises to about 75%. This indicates that even in knapping locales, isolated artifacts have been discarded or lost or that knapping debris may be admixed with background accumulations (Conard 2001), the "veil-of-stones" effect (Roebroeks et al. 1992).

In order to confront the information provided by the refits with the evidence obtained from a technological classification combined with an analysis of scar-pattern superpositions, two Levallois sequences, Cc28/14 and Cc28/27, were deconstructed. The cores and blanks were classified according to the system mentioned above, and next, a "model" sequence was mentally reconstructed on the basis of the observed frequencies of scar patterns.¹

The Technological Classification

The first analytical step consists of the classification of the cores. Both (figs. 4, 5) are recurrent unipolar Levallois cores, even if core B (fig. 5) is not very straightforward. A large scar of a preferential flake is clearly visible, but the overlying negative scar indicates a failed attempt to produce a subsequent blank. According to their considerable volumes, both cores would seem to have been abandoned at a relatively early reduction stage. Next, the debitage products are classified into *predetermining* and *predetermined* blanks. Among the latter, various types are identified: preferential (fig. 2, 1–3), unipolar recurrent (fig. 2, 4–6, 8–10), bipolar recurrent (fig. 2, 7, 11–12), and centripetal recurrent (fig. 2, 13–15). Thus, according to the classification of blanks, it seems as if most of the Levallois methods have been used. The predetermining negatives on both the cores and the blanks suggest that the

1. Ideally, both types of technological analysis should have been independently performed by two or more analysts, whereas they have now been executed by the same person (P. Van Peer) and in the specific context of this comparative exercise.

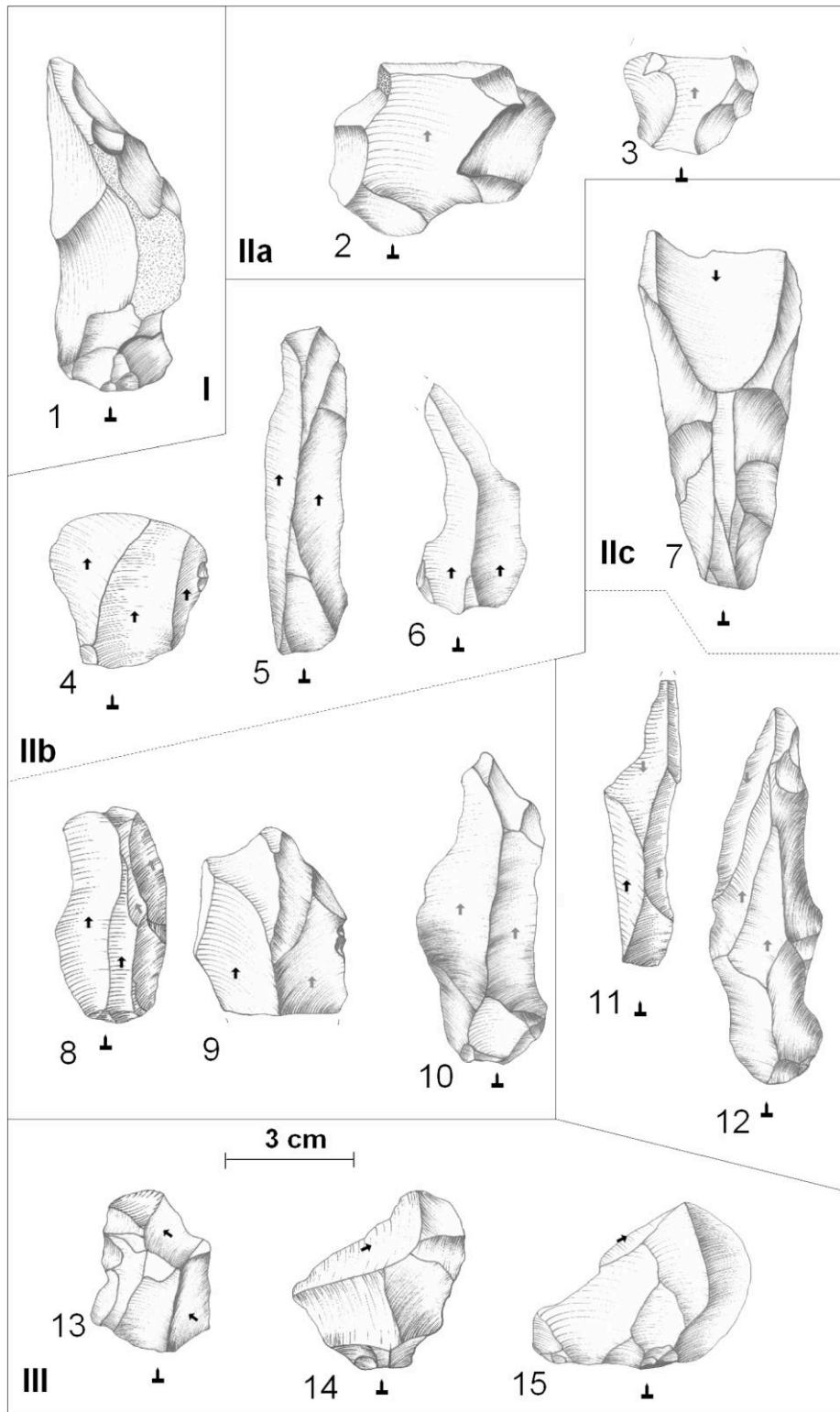


Figure 2. Selected technical products from deconstructed reduction sequences 28/14 and 28/27 from Sector 91/04, Concentration 28, at Taramsa 1 arranged according to inferred position in the overall *chaîne opératoire* stages (Roman numerals).

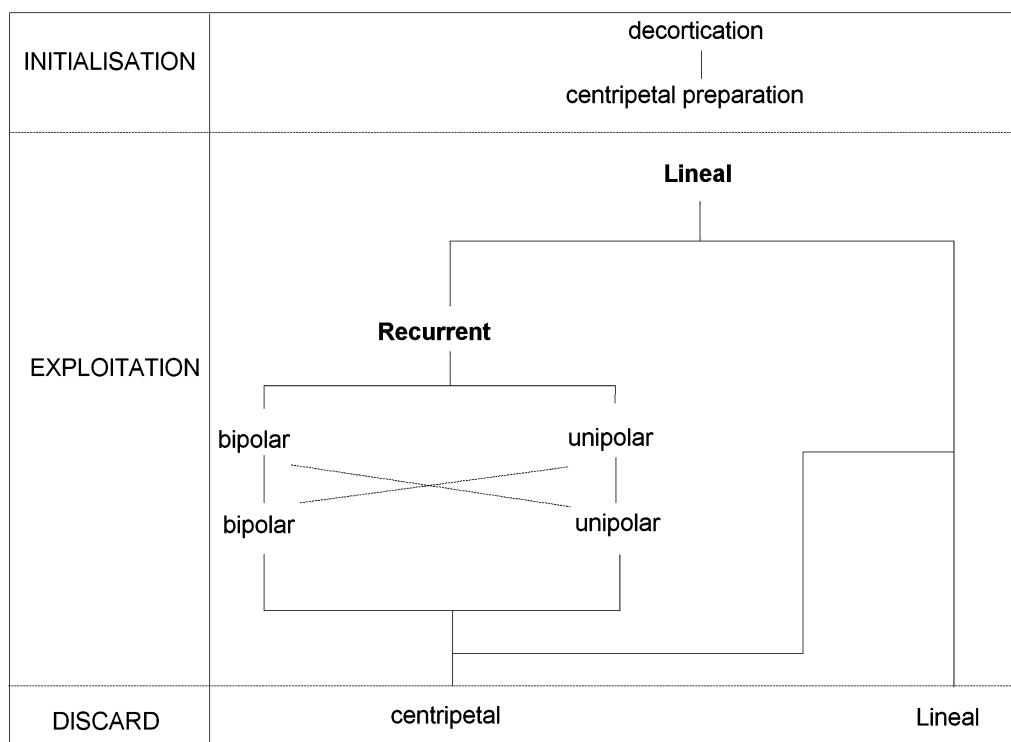


Figure 3. Schematic representation of the overall *chaîne opératoire* of the assemblage constituted by deconstructed sequences 28/14 and 28/27 from Taramsa 1 as inferred from their operational sequence classification.

initialization phase of the cores consisted of a centripetal preparation.

Now, these types need to be supplemented with chronological information in order to be assembled into a master reduction sequence. There is one large preferential flake in the assemblage with a patch of dorsal cortex (fig. 2, 1). This can be taken as evidence of the use of a lineal method of exploitation right after the initialization phase of the core. Two other preferential flakes (fig. 2, 2–3) show the negatives of a previous preferential removal overprinted by centripetal predetermining scars. This indicates that the lineal method was sometimes maintained throughout the reduction. But it was not the only option; a number of laminar blanks show two or more unipolar negatives superimposed on the centripetal preparation (fig. 2, 5). They testify to the existence of a unipolar-recurrent method of exploitation. Also, some dorsal surfaces of similar laminar blanks exhibit earlier unipolar series separated from the present exploitation phase by centripetal predetermining scars (e.g., fig. 2, 8–10). Other laminar blanks show evidence of bipolar exploitation both in the present reduction state (fig. 2, 7) and in an earlier reduction phase (fig. 2, 11, 12). Finally, there are a number of centripetal-recurrent flakes with large invasive negatives on their dorsal faces resulting perhaps from previous unipolar exploitations. Their small size can be taken to indicate a late reduction stage.

Based on this analysis, an overall model could be inferred as shown in figure 3. The reduction of individual Levallois cores, after a similar initialization phase, may have followed a specific trajectory through this general model, idiosyncratically combining its particular modules. However, alternative “models” might be conceived as well. For instance, the fact that there are no centripetal-recurrent cores may be taken to indicate that the recurrent phase of exploitation happened earlier in the sequence.

The Refitting

How accurate does this model turn out to be when it is compared with the actual reduction processes as shown in the refitted sequences?² First of all, it appears that some of the typological identifications are wrong. Thus, the proximal fragment of a preferential Levallois flake (fig. 2, 3) is refitted onto the distal fragment of a blank supposedly showing a unipolar-recurrent method (fig. 2, 9). The blank in figure 2, 5, presents an interesting case. It was identified as being produced in a unipolar-recurrent method. This is indeed the case, but that unipolar series is struck from the opposed striking

2. For detailed descriptions, the reader is referred to Van Peer, Vermeersch, and Paulissen (2009).

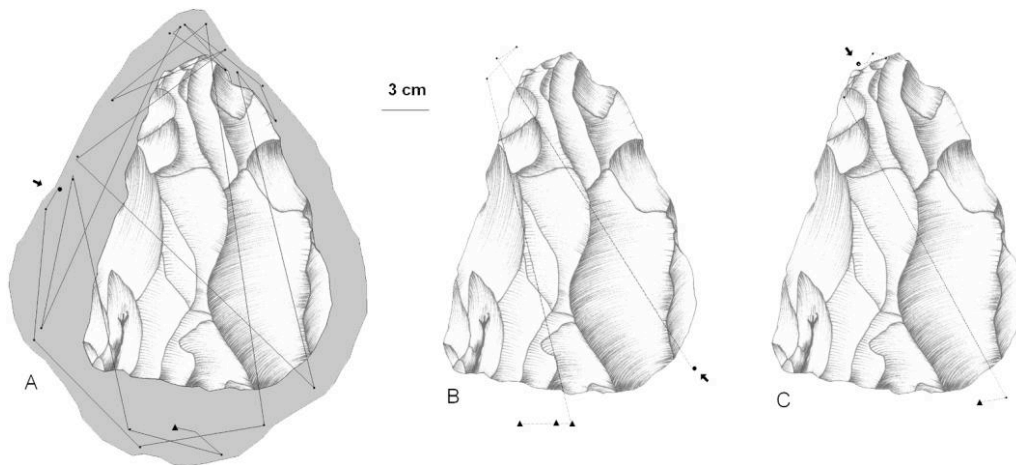


Figure 4. Reduction sequence 28/14 from Taramsa 1 separated according to three subsequent exploitation surfaces (A–C). The gray area represents the plan projection of the original nodule; lines represent the progression of the reduction sequence; dots indicate the position of the preparation removals; triangles represent preferential flakes struck from the subsequent exploitation surfaces.

platform on core B. As a whole, this series fits in an alternating bipolar exploitation of the core. The three flakes evidencing a centripetal-recurrent method of exploitation, in contrast to all the other predetermined blanks in these two reductions, are detached in a plane that is tangential to the intersection plane of the core. This strongly suggests that they are not Levallois flakes, because these are normally produced in a parallel plane. They are simply predetermining flakes meant to reshape the core convexities, and therefore, they cannot serve as evidence of a centripetal-recurrent method. In sum, it appears that morphological criteria to unequivocally identify and distinguish the various Levallois methods are often inadequate.

With regard to the accuracy of the reconstructed master sequence based on the pattern superpositions, notice how several products show direct superpositions of scars that appear to be significantly distant from each other once the complete order of the entire sequence is established. Sometimes a blank that was produced at the very end of the sequence may show dorsal remnants from the very beginning, but of all the reduction stages that the core passed through since, no trace is left. Thus, important temporal hiatus may be present in these supposedly partial sequences. Obviously, if these are not accounted for, the reliability of the assembled “type sequence” is in jeopardy. For example, the preferential blank of figure 2, 2, is considered to represent an early reduction stage. The refit, however, shows that it was the twenty-fifth flake in a total series of 37. The large negatives on the proximal end and on the right side are from flakes struck early in the sequence. Of the intensive preparation in another core sector performed since, nothing remains. The blank of figure 2, 10, is a particularly instructive case. It was the last blank in the

sequence of core A, but the intersecting negatives on its dorsal face span almost the entire duration of the reduction, with important gaps in between them. It was classified as a unipolar blank with evidence of an earlier unipolar series represented by two consecutive scars. The refitting shows that in between the production of the latter, an important reshaping of the core convexity occurred. The two blank negatives, therefore, belong to separate reduction phases and are not evidence of a recurrent exploitation of the same Levallois surface. These refitting examples indicate that when long and complex reduction sequences are involved, it is virtually impossible to determine a general type sequence from the isolated products.

This finally brings us to the comparison of the model sequence with the actual reductions. How well does this model describe the general tendency, if any, in this assemblage? The exploitation sequences of the upper surfaces of the cores are schematically represented in figures 4 and 5. It appears that both reductions have passed through several initialization/exploitation cycles: the upper surface of core A has been reshaped three times and that of core B four times. In both cases, the first cycle ended with the production of a single preferential Levallois blank, and in the next phase, both cores passed into a recurrent mode of exploitation. This is most explicit in core B, where 10 predetermined blanks were produced in a pattern of alternating, i.e., bipolar, exploitation of two opposed striking platforms. In core A, the exploitation at this stage is unipolar. Exploitation phases three and four of core B delivered short unipolar series, each comprising two predetermined blanks. Both times, the core convexities were restored by partial centripetal reparations. The third exploitation phase of core A ended with the detachment of only one predetermined blank, but given its lateral position on the

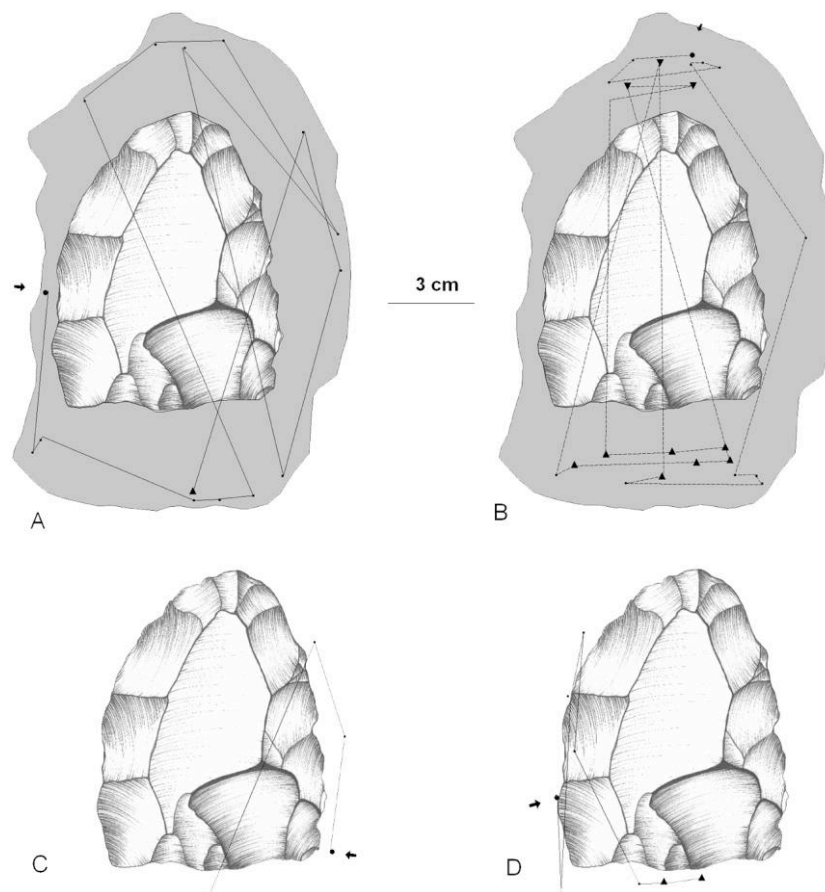


Figure 5. Reduction sequence 28/27 from Taramsa 1 separated according to four subsequent exploitation surfaces (A–D). The gray areas represent the plan projection of the original nodule; lines represent the progression of the reduction sequence; dots indicate the position of the preparation removals; triangles represent preferential flakes struck from the subsequent exploitation surfaces.

newly prepared core surface, this can be considered to be the first flake in a unipolar series.

Generally speaking, these features of the two reductions do emerge in the model, in particular, the fact that a first lineal exploitation phase is followed by either bipolar or unipolar exploitations during more than one reduction cycle. On the other hand, the model suggests a degree of variability in terms of method combinations that is simply not attested to in the actual sequences. For instance, in the refits, there is no evidence at all of the centripetal-recurrent method of exploitation. Nor does the lineal method exist as a separate trajectory. As indicated, this might be inferred from the fact that secondary preferential flakes seem to be present in the assemblage and from the apparent lineal exploitation of core B. The refitting, however, shows that this exploitation phase is only the last in a complex sequence of which hardly any trace is left in the present core configuration. When the bipolar exploitation phase of this core (B) is considered in more detail, it

appears that the productivity of the two platforms is very unequal: only three blanks were detached from the distal platform. Clearly, there was only one preferential striking platform throughout the entire duration of this reduction sequence. Thus, according to the refit evidence, the generally used reduction method in this assemblage is a unipolar one with variable production efficiencies in the various reduction stages.

Based only on the technological classification of the artifacts in this assemblage, there is no doubt that the Levallois production system is the only one represented. When refitted, the reduction of core A does indeed exhibit the typical association of all the Levallois concept criteria (Van Peer 1992; Boëda 1994). The refitted sequence for core B, however, shows a number of different organizational features. It must be concluded from this comparative exercise that the mentally assembled reduction model provides a rather imprecise flow-chart for the general approach to lithic reduction in this

assemblage, both in terms of volumetric organizations and of methods of surface exploitation.

Theoretical Problems

The main thrust of our methodological argument so far has been that in many of its aspects, the OS approach is a typological procedure facing the same problems as other lithic typological systems. However, being optimistic, we believe that the amplitude of these methodological problems can be significantly constrained and, therefore, that technological analysis continues to be of paramount importance. Now, we must turn to the more difficult matter of problematic theoretical cornerstones of this paradigm that affect the epistemology of the OS approach. They are related to the attribution of meaning to technological categories.

Archaeological Classification

Every prehistorian knows that mentioning the term *Mousterian* carries the connotation of a certain set of artifacts, such as side scrapers and points. Similar sets, created by generations of archaeological studies and numerous publications, emerge and are defined as Szeletian, Bohunician, Aurignacian, Natufian, and many other industries. The social and historical meanings of these classifications (variously described as “industries,” “paleocultures,” or “social entities”) remain to be discussed by the interested parties (Clarke 1968). Such basic classifications, incorporating stone-tool categories, often with the addition of detailed attributes—even if not all objects are strictly defined or their definitions are not accepted by all—are imperative in order to bring a “grammar” to the archaeological record (Whittle 1996) and to facilitate reporting the studied prehistoric assemblages (Clarke 1968; Dunnell 1971). Original formal typologies created by the pioneers of prehistoric research were derived from direct analogies between the stone artifacts and ethnographic examples (e.g., Sollas 1915; MacCurdy 1926). In the second half of the twentieth century, the theoretical interest in the role and nature of classification in archaeology as an empirical science expanded exponentially and gave way to different methods. For example, from the 1960s onward, an analysis based on attributes at various scales of measurement was favored (Bisson 2000). This approach aimed at finding objective, natural groups (Doran and Hodson 1975) and at explaining the meaning of variables and structures they brought about. It is not our intention to review here the immense literature on archaeological classification of the last 50 years. We feel, however, that a few remarks are in place in the context of our discussion of the OS epistemology.

When individual facts or events are classified according to a prescribed set of criteria, they turn into formal data. We choose the criteria to establish categories based on prior knowledge, experience, and also according to the expectations in search of order and clarity (Shott 2003*b*). They are not

independent features of the external world because researchers are actively involved in their construction and, as a result, infuse them with meaning: classes are units of meaning (Dunnell 1971, 45). In order to serve anthropological inquiry, such formal data are put through a process of analysis, an outcome of which is that they may turn out to be behaviorally meaningful. However, this does not imply that we have come anywhere near emic categories of meaning or any other emic framework of meanings in which the facts had their place. Not surprisingly, it is difficult to continuously maintain the distinction between the formal and the emic. In the act of formal classification, only a heuristic tool, we are almost bound to think that we are restoring emic meaning to the facts. However, what is really being done in such a case is to impose an a priori interpretation, and subsequent interpretations will only be accommodative statements.

Another aspect of the formalization process involved in the construction of data is the decision on the unit of analysis. This involves, on one hand, the choice of a unit of observation and, on the other, the choice of a level of generalization at which such observations will be grouped. The latter is particularly important because it imposes a threshold on variation. Below the threshold, it is assumed that there is only random variation not worthy of explanatory efforts (Isaac 1977). At the same time, it is hoped that this random variation is small enough not to prevent the emergence of patterns at the chosen level of generalization.

The decision concerning the level of generalization pertains first to the process of assemblage formation. In principle, the most objective level would seem to be the material production of one individual, at least until it can be demonstrated that there is no patterned variation between two or more individuals. Usually, however, a lithic assemblage means a set of artifacts from the well-defined spatial context of a stratigraphic unit. Given the coarse resolution of the Paleolithic record, we cannot ascertain that the assemblage we study corresponds to a set of activities carried out in a short time and constitutes an individual behavioral unit. It is implicitly assumed, therefore, that individuals contributing to the buildup of an assemblage conformed more or less to general standards of lithic production that characterize the assemblage.

This brief consideration is meant to indicate that data are always constructed according to a number of decisions that depend on assumptions about past behavior. In structuring the data, we also set limits to their interpretation in the future, and therefore, this is a crucial stage in an inferential procedure. Let us consider the OS approach from this perspective.

Reduction Methods and Objectives as Mental Templates

As we have argued above, one of the reasons for the development of technological studies was to reduce the impact of a priori assumptions about emic meanings in the interpretation of archaeological classifications. It seems, however, that

the problem is present again and even in a stronger form. In many OS studies, it is explicitly stated or at least implicitly apparent that their technological descriptions reveal both the intentions of and the technical choices made by the prehistoric stone knappers. For example, having defined 13 types of predetermining Levallois products, Boëda (1994, 39) states

If we admit that the prehistoric artisan disposes of the entire range of thirteen predetermining removals, he has the possibility of *choice* [our emphasis]. . . . Choice can be constrained by the future use of particular blanks that he knows beforehand to be suited for a particular task. [Our translation from French]

The typical OS jargon, using terms such as *intent*, *choice*, *preference*, and so on, is so commonplace nowadays that such interpretative terms are considered to belong to the realm of neutral description. One gets the impression that the technological approach paves the way right into the minds of the prehistoric artisans (Inizan et al. 1999, 103):

Analyzing the *chaînes opératoires* in terms of psycho-motoric processes allows one to go beyond the mere identification of technical gestures and to bring to light, for every step of the *chaîne opératoire*, the choices, the limitations, the preferences, and the reasons for success or failure; to see by which procedure any project has been realized. [Our translation from French]

Technological classification is not considered anymore as simply a heuristic device but as a system that reflects emic cognitive standards. A common opinion among researchers of Middle Paleolithic assemblages is that the prehistoric artisans had comprehensive knowledge of available methods and techniques, incorporating the various Levallois and non-Levallois methods. The underlying idea seems to be that an artisan is in total control of and totally conscious about the outcome of any act in the course of a reduction sequence.

The definition of Middle Paleolithic stone technology as an assembly of discrete concepts and technical scenarios is often supplemented with the idea that each reduction method has a particular morphological finality: "The variability has a technical logic of his own, being that the different options are related to the objectives of production" (Delagnes 1995, 210). These are the desired blanks, revealing prehistoric intentions for the production of which a particular method was chosen. This is again an assumption, and it is flawed by circular reasoning. The definition of reduction methods, as discussed above, relies on the morphological analysis of "desired products." Hence, almost by definition, each particular shape involves a particular method without any independent evidence for its existence. Simple random variation is likely to be confused with a meaningful pattern of discrete variability.

In this context of epistemological confusion, the use of a term such as *desired products* is unfortunate. It bears an emic connotation whereas it should only translate our perception of the intended finality of the reduction methods, which

themselves are our own design as well. Using both experimental and refitting data, as well as our (limited) understanding of the physical principles involved in lithic reduction, it is reasonable to argue that, for example, Levallois blanks are the predictable conclusion of a preceding sequence of technical actions. In that sense, they can be called end products. However, that does not automatically make them desired products. In contrast to the former, the latter designation contains a statement about how these items were perceived in the past. It is within *our* framework of reference that the predetermined product looks as if it must have been the desired product, not within *theirs*. It is only possible for us to identify artifacts that were somehow desired based on independent evidence. Microwear or residue analysis, for instance, can show that pieces were selected for use. Edges with secondary modification may indicate that the blank was chosen for its particular morphological characteristics. These blanks were selected according to decisions as to which forms were appropriate for particular ends.

In sum, we believe that the OS approach makes a number of unwarranted assumptions about lithic production in the Middle Paleolithic, either by Neanderthals or early anatomically modern *Homo sapiens*. It is also strange that assertions about the context of meanings surrounding the exertion of technical skills are made when the middle-range theory to support them is underdeveloped. Experimental replication alone does not seem a sufficient basis for such a process of theory building. A question that actualistic studies might address, for example, is, which physical mechanisms are engaged in purposeful shape predetermination? Furthermore, an epistemological contradiction appears to follow from such an assumption, as we will discuss below.

Discussion

Technological Choices

Certainly, Middle Paleolithic stone knappers made choices about how to execute reduction sequences, if only in response to raw material variation as well as to the availability and reliability of these sources. These choices or decisions by the prehistoric tool makers probably also took into account the time spent on a site or on the move that would predict what S. L. Kuhn (1992, 1995) referred to as *technological provisioning*. The concept of provisioning replaces the terms of *curation* and *expediency* introduced by Binford (1979). The term *provisioning* refers to the planning depth in artifact production, transport, and maintenance and the strategies by which the needs of foragers are met (Kuhn 1995, 22). Modern foragers, serving as the basis for the analogy, deal with anticipated demands for tools in different ways. Kuhn identified two principal modes of provisioning that ensure the availability of tools in advance. *Provisioning individuals* with their personal gear (Binford 1979) means that people always have at least a few needed tools at hand. Implements are manu-

factured and then transported and maintained in anticipation of variable needs. They could be in the form of special tools, if specific needs have been anticipated, or as more general tools (“Swiss Army knives,” as F. Bordes used to call them). In some cases, even raw materials in the form of shaped cores were carried for later use in the anticipation that in a new area, the raw material quality is of a lesser degree. The strategy of *provisioning places* is to provide the raw material or shaped cores and selected blanks at locales where activities will be conducted. This strategy requires some prior knowledge of both the timing and the probable locations of future needs. Its utility depends on residential stability and on the duration of use and frequent stays in chosen places or habitation sites (Kuhn 1995). The relative importance of each strategy (provisioning of places vs. individuals) should vary with residential mobility. Short-duration occupations yield relatively large numbers of tools carried by individuals, while places occupied for longer periods are more likely to be provisioned mainly with raw materials. As the duration of site use increases, the large quantities of debris from manufacturing tools on site will rapidly swamp the transported tools, which are always less numerous (Kuhn 1995). Because mobility patterns among modern hunter-gatherers vary over the course of a year and spatially within their territory (Bamforth 1991), foragers often practice a mixture of technological strategies (Kuhn 1992, 1995; Henry 1995, 1998), creating an archaeological record that will be very difficult to decipher.

Scales of Analysis

The active role of individuals in structuring Paleolithic archaeological records has recently received renewed attention (Gamble and Porr 2005) and is supported by current theoretical perspectives offered by behavioral ecology (Shennan 2002). As indicated earlier, behaviorally relevant generalizations of primary data should logically start at the level of the individuals active in lithic assemblage formation. This is a fortiori the case under the theoretical tenets above, where the morphological variability of artifacts is necessarily of discrete nature and is considered as the consequence of rational choices made by conscious individuals. Any generalization beyond that basic unit of analysis should be based on a clear demonstration of interindividual uniformity as a consequence of, for example, the existence of geographic networks for sharing technical knowledge.

We believe that the desire to trace the role of human agency is indeed legitimate (e.g., Dobres 2000), yet the task is always difficult, even within a “Pompeii premise” situation (Binford 1981) in sites in which living floors were covered soon after abandonment. Given the limitations to the resolution of the archaeological record, the assemblage is usually the unit of generalization without further consideration, implying that the individual artisan is not a source of patterned variability. This is only possible under the assumption that the individuals who contributed to the assemblage made on average the same

choices, because supposedly they represent the cultural standards and the technical tradition of the group. As indicated above, this should be demonstrated rather than assumed. How different, for instance, would the results of a comparative analysis be if we were able to compare the artifact sets produced by the individuals contributing to two distinct lithic assemblages rather than comparing the products of the two “average” individuals portraying each assemblage?

A detailed analysis of all the reconstructed sequences from Taramsa 1, sector 91/04, Concentration 28, two examples of which were discussed earlier, revealed the existence of four discrete “technical” groups to which most of the sequences can be assigned.³ This pattern was established based on location of the preparation flakes on the upper surfaces of these cores and on the order in which they were removed until the first end product was struck. In figure 6, a graphic representation of these spatiotemporal patterns of preparation is shown for the two reductions 28/14 and 28/27.

These reductions appear to belong to the same group, meaning that they are very similar in terms of the way that the preparation of the first exploitation surface was achieved. This similarity is indeed clearly apparent in the close corre-

3. Only a few sequences showed very idiosyncratic patterns that could not be assigned to any of the four groups. For full details on the method and the results of the analysis, see Van Peer (2007) and Van Peer, Vermeersch, and Paulissen (2009).

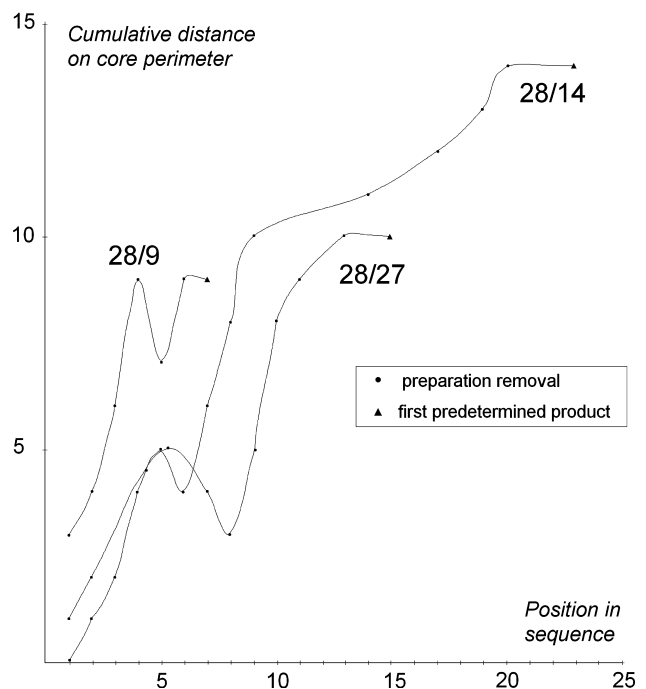


Figure 6. Quantitative expression of the reduction sequences of the first preparation phase for three refits from Taramsa 1, Concentration 28, based on mapping of the preparation removals on a plan projection of the core (for details on the calculation procedures, see Van Peer 2007).

spondence of the lines in figure 6, which also includes the line of a third reduction that belongs to this group. The group structure has been interpreted as representing the personal knapping habit of four individuals who discarded their production debris in Concentration 28. Of course, this is only an interpretation, but the exercise demonstrates at least that under certain circumstances, empirical procedures can be set up to derive high-resolution patterns from objective technological data and to reflect on their significance.

Prudently assuming that reductions 28/14 and 28/27 were indeed carried out by the same individual, it is instructive to reconsider the technological evidence from the formal typological perspective as it was presented earlier. The refitted sequences in particular have told us that this individual used a unipolar-recurrent method of exploitation proceeding from one preferential striking platform. Moreover, all the other reductions in the three other groups show essentially the same pattern of exploitation: recurrent series of elongated blanks struck from the same platform that was usually reshaped before the production of a new series. Only occasionally is the opposed platform used to produce blanks, where to it also received specific preparation. We might be inclined to interpret this as an indication of a shared standard template in order to produce generally similar, useful blanks. This evidence might also be taken to lend support to the validity of the concept of a “model reduction sequence,” however difficult it may be to mentally reconstruct it, as we have tried to show.

There is, however, some other evidence to consider. It was already mentioned that in their volumetric conception, a number of these reduction sequences show quite divergent features from the Levallois concept. They relate, among others, to the degree of convexity of the exploitation volume or, in other words, the proportions of the functionally different parts of the core and to the production of blanks in planes tangential to the main core intersection. If we are consistent with the principles of formal classification explicated above, these reductions cannot be classified as Levallois, and in fact, the name of Taramsa reduction strategy was proposed for them (Van Peer, Vermeersch, and Paulissen 2009). Reasoning in formal terms again, this leaves us with a situation where the same unipolar-recurrent method of exploitation is maintained across different, though related, production systems.

Interestingly, when the distribution of these reduction types across the four individuals is looked at, it appears that each of them produced reductions according to the entire range of formal strategies: from normal Levallois concept reductions to Taramsa strategy sequences in which all of the transformed concept features are attested together. From a behavioral point of view, where we see reductions actually being carried out in the context of an overall technoeconomic *chaîne opératoire*, it seems that our formal categories have little significance. At least, that is what this pattern of concordance might be taken to suggest. A similar array of technological attributes is represented in the assemblage from late Middle Stone Age sector

91/03 at Taramsa 1. Here, the spatial distribution of refitted reduction sequences seems to indicate the positions of again four individual artisans. When these subassemblages are analyzed in terms of the frequencies of the “transitional” technological attributes mentioned above, it appears that they are significantly more represented in the production of one of the knapping zones (Van Peer, Vermeersch, and Paulissen 2009). Perhaps a particularly innovative individual was at work here. At the same time, this is an instructive case as to the influence of an individual artisan on the overall assemblage constellation.

It is appropriate in this context to raise the possibility that before cores became fully exhausted, they may have been used in practice sessions, where one knapper learned from another. This could have been a passive process carried out through watching, but it could have involved oral explanations as well, particularly while younger members of the group were watching and imitating the adults. Additionally, children imitating while playing could have picked up discarded cores or thick flakes that the adult knappers would consider to be unusable and practice without supervision. This little reflection should leave us with two conclusions: (a) we ignored the role of children in fabricating Paleolithic stone tools or blanks (Ronen 1974; Shea 2007), and (b) the classification of the cores in their final, exhausted phase may very much bias our conclusions concerning the reduction method that was operated by the adult knappers (Bar-Yosef 1998; Shea 2007).

In realizing its capacity to describe technological dynamics at high levels of resolution, the OS approach can possibly make a contribution to, for example, our understanding of cultural transmission in the past (Shennan 2002). It is not at all unreasonable to consider that these populations, who mastered the linguistic flexibility since the last 200,000 years or perhaps since the appearance of *Homo erectus* (e.g., Enard et al. 2002; Lieberman 2002), employed language in toolmaking, as observed for more recent examples (Roux 2007). The latter indicate that learning by watching and imitation alone are insufficient and that oral explanations are a necessity particularly in manufacturing, for example, Levallois products.

An understanding of such cultural processes active at the fine-grained resolution of daily behavior is instrumental for the informed investigation of large-scale spatial and chronological patterns resulting from population histories and evolutionary processes. The uniform distribution of basic technical methods within a particular geographic distribution, for example, may indicate the use of oral teaching tradition and the imposition of a rigid framework of “know-how.” The long-term perspective that is usually implied at the Pleistocene timescale has often confronted us with the sustained stability in material productions throughout the Middle Paleolithic. In the thick depositional units exposed in Levantine cave sites such as Tabun, Kebara, Qafzeh, and Hayonim, long time-depths are represented by stratigraphic units, and the clustered assemblages demonstrate long-term tendencies in the use of one—rarely two—particular reduction methods. For exam-

ple, during the early Levantine Mousterian (“Tabun-type D”) at Hayonim Cave, we find one or at most two methods (Meignen 2000) in use over a length of time of at least 70,000 years (ca. 220–150 ka; Mercier et al. 2007). In the late Mousterian of Kebara Cave, the “convergent unipolar-recurrent Levallois” method was in active use—with a certain amount of morphological variability among the produced blanks—for ca. 20,000 years (65–46 ka; Valladas et al. 1987; Meignen and Bar-Yosef 1991; Bar-Yosef 1998; 2003). This cultural continuity in long Middle Paleolithic sequences dominated by one or two OSs posits an intriguing question related to the issue of human agency, as others have already remarked (e.g., Clark 2005). A similar question was already raised by more than one scholar concerning the production of Acheulean bifaces during a time span that lasted ca. 1.5 Myr in well-defined regions. How can we reconcile this apparent technological stability over the long term with the knowledgeable, innovating artisan operating at the short-term level?

Conclusion

There can be no question that the *chaîne opératoire* concept has brought a fundamentally new perspective to the analysis of Middle Paleolithic assemblages, and it is certainly not our intention to diminish the contribution of technological studies to achieve valid interpretations of human behavior in the past. We have only wanted to draw attention to a number of epistemological problems in an effort to reinforce the ties between the analytic means and the explanatory ends of archaeological inquiry and to reduce the danger of becoming trapped in unproductive formal determinism.

Nor has it been our intention to question the epistemological necessity of technological classification. It is instrumental to reveal patterning in the record and, hence, to provide us with an empirical basis for reflection on population-level processes. In some of its present forms, however, we perceive a degree of overformalization and an inability to be objectively applicable by different researchers due to lack of explicitly described criteria. In a way similar to the redundant typological paradigm that was employed in the past, the technical methods now seem to exist in isolation. In attributing emic relevance to this formalized structure, a perception of Middle Paleolithic behavior is imposed with only a minimal chance of corresponding to any reality of the past, and hence, they do not bring us closer to the real lifeways of past Paleolithic foragers. In our view, our formal reduction strategies and methods are from a behavioral point of view, most likely only situational grades in a general technological system, perhaps forged and maintained through daily communication. While the anthropology of technology identifies the essential role of social contexts explored through ethnoarchaeological or ethnohistorical studies, none of this is really available for the Lower and Middle Paleolithic. In the study of these remote periods, we are still struggling to recognize and understand seasonal movements, foraging strat-

egies, paleodemography, and mechanisms of dispersal, and the social context of lithic production largely evades us.

Given the amount of unexplained toolmaking variability recognized in Middle Paleolithic contexts, perhaps the OS approach should be concerned less with descriptive formal classification of debitage products and methods and give more attention to the search for the causes of patterns in the technological record. Modeling—for example, past learning and teaching habits of Paleolithic foragers under stable or unstable environmental conditions—may provide some clues as to why particular technologies lasted for periods of many thousands of years, and it may get us closer to understanding the social dynamics, or lack thereof, in cultural evolution.

We are convinced that technological studies can bring us closer to building more dynamic scenarios of prehistoric lifeways that in their turn can give way to new methodological constructs advancing the discipline. Therefore, we plead for a reconsideration of the fundamental tenets of the *chaîne opératoire* approach, asking if at present we are not—to paraphrase Glynn Isaac—trying to squeeze blood from stone tools.

Acknowledgments

We are grateful to Anna Belfer-Cohen (Hebrew University) and Mina Weinstein-Evron as well as seven anonymous reviewers for their many useful comments on an earlier draft of this article. Research underlying some of the ideas expressed here was supported by University of Leuven Research Grant OT/01/10. Needless to stress, all shortcomings are our own.

Comments

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Most of what archaeologists know about the Stone Age, the biggest block of the human past, results from a very peculiar activity. Stone toolmaking is mediated by the physics of fracture mechanics, but it rests on knowledge of what will happen when force is applied to particular points on an isotropic mass. Those facts make stone working highly predictive, very anticipatory, and sequentially reductive. Stone tools always start big and get smaller. This reduction sequence can be paused, but it cannot be reversed or undone. Lithic technology is not the only sequential reductive process humans practice, but it is more rigidly structured than most technological processes.

In trying to use stone tools as a record of the human past, lithic analysts around the world have recognized the sequential nature of stone-tool manufacture and have developed conceptual tools to understand how artifacts come to us as they

are (Bleed 2001). These “sequence models” are useful to archaeologists because they summarize production processes, present intermediate steps, and link formally diverse materials. Bar-Yosef and Van Peer address the most well known of these concepts, *chaîne opératoire*, or CO. Their presentation of recent French literature and their assessment of CO applications are useful, but it is unwise to conflate the CO approach with other sequence models and with the sequential approach to lithic analysis. Consideration of the range of “sequence models” developed by Stone Age archaeologists puts French practice in clearer focus. Awareness of the diversity of sequence models is a good first step to avoiding their misuse.

Sequential approaches to lithic analysis have emerged from replications and refitting studies and from both morphological and technological approaches to classification. Bar-Yosef and Van Peer indicate that French applications of CO draw on all of those analytical roots. Beyond that, archaeological sequence models have emerged from very different theoretical perspectives. Behavioral archaeologists, for example (Skibo and Schiffer 2008, 9ff.), conceptually wrap the steps of technology in taphonomy and the sequence of natural and human processes that create the archaeological record. Among Americanist archaeologists, reduction models grew out of the processual interests and middle-range theory building of New Archeology. Thus, Americanists most often use reduction models to address movement, site function, seasonality, territorial range, and adaptive strategy, all issues that reflect eco-functional biases. Those issues are all to be found in French applications of the CO (Boëda, Geneste, and Meignen 1990), but they are not its hallmark. The strength and utility of the French typological approach certainly marked development of the CO methodology. Thus, CO are often presented as classifications of technological systems. Beyond that, with roots in French structuralism, sequences presented as CO lend themselves to discussion of cerebral issues of intent, choice, preference, gesture, event, cognition, structure, symbolism, and agency. This gives them a wrapping of thick jargon that many Americanists find opaque, although sequence models have been used to address cognition in Americanist literature (Bleed 2002). The larger point is that all of these applications are valid but only as good as the analysts can make them. Like shovels, word processors, or statistical tests, sequence models are among the multipurpose tools available to archaeologists.

Bar-Yosef and Van Peer are correct in asserting that the major challenges in using a sequence model are epistemological. Sequence models of any persuasion are archaeological constructs. They exist here and now and are used to address questions posed by modern researchers. Problems are likely to arise when modern researchers uncritically equate their models with past behavioral reality. Refitting can certainly demonstrate sequential actions, and models can describe processual patterns, but as the authors say, this is a long way from laying bare emic categories, much less cognitive struc-

tures or intended strategies. Perhaps lithic processes rested on well-formed intentions and carefully managed customary patterns. The fact of their sequential patterning may demonstrate that ancient stone workers carried teleological plans that they executed. Alternatively, those patterns may simply reflect that stone workers possessed skills they could call up as the tasks before them evolved. These are interesting alternatives that archaeologists can and should investigate with sequence models. Bar-Yosef and Van Peer’s presentation suggests that the CO approach is not simple or straight forward. It also carries serious epistemological problems that make it hard to freely investigate sequential activities. Mention of *chaîne opératoire* seems automatically to lead to discussion of “master sequences,” “discrepancies,” “analytical coherence,” and “errors.” This suggests that even with the cautions Bar-Yosef and Van Peers provide, the CO approach begins with the expectation of idealized behaviors.

The real world is messy, and stone toolmaking must have been an especially cluttered activity. To expose patterns and diversity within that clutter, lithic analysts need conceptual tools that are easy to use and clearly effective. In searching for useful approaches to sequential analysis, archaeologists certainly should consider the CO approach, but the exploration need not—and should not—end there.

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Bar-Yosef and Van Peer are leading researchers on the Paleolithic and have vast amounts of direct personal experience excavating and analyzing lithic assemblages. Thus, students of the Paleolithic should pay close attention to their critique of the *chaîne opératoire* approach.

I am in agreement with many of the points they make, and I agree that the *paléopsychologie* approach generally has not proven to be productive. Also, some forms of technological analysis with the *chaîne opératoire* school achieved little more than providing a taxonomy of methods of knapping stone. Like all forms of taxonomy, these classifications do little to solve archaeological problems when they are not linked explicitly to competing hypotheses and clearly defined research questions. As Bar-Yosef and Van Peer point out, it is by no means possible to put each well-documented reduction of a cobble or minimal raw-material unit into a neat taxon. Additionally, the authors correctly argue that overriding research goals and the relevance of the *chaîne opératoire* approach within a broad anthropological research agenda are often poorly formulated.

The proponents of the *chaîne opératoire* approach—Geneste, Pelegrin, Boëda, Meignen, and others—radically changed the discourse on lithic studies in the 1980s and early

1990s. In Germany and other parts of continental Europe, similar ideas propagated by researchers in Cologne, Tübingen, and other centers of Paleolithic research led to a parallel shift away from typological studies and toward more technologically based studies. This being said, talented researchers had long used both technological and typological approaches, and the rhetoric of the French technological revolution of the 1980s and early 1990s was at times based on false dichotomies and expedient stereotypical depictions of a conservative old guard versus the enlightened advocates of the *chaîne opératoire* approach. During this period, the younger generation of French archaeologists, along with forward-thinking senior colleagues, defined new directions of research that led to a large-scale rejection of many of the orthodox ideas and methods developed by senior colleagues, including François Bordes and some of his contemporaries. The revolutionary dogma and rhetoric of the new French school of technology inspired many researchers inside and outside France and led to the development of highly influential centers of technological research at Paris X, Valbonne, and elsewhere. While much of continental Europe shared a high level of enthusiasm for the *chaîne opératoire* approach, the Anglophone community often remained skeptical of the advances from the continent.

By the mid-1990s, the revolutionary phase of technological research and propaganda waned, and more systematic and, in a positive sense, more routine applications of the *chaîne opératoire* approach became common. While forays into paleopsychology and other less productive areas of research continued, researchers and students conducted vast amounts of direct experimental work, ethnotechnological studies, and highly influential studies of many of the methods of stone working that are documented in the archaeological record. From the onset of this technological revolution in France and much of continental Europe, its practitioners recognized that the approach applied not only to chipped stone but to all kinds of artifacts as well as other forms of human action, be it building fires, butchering animals, or making pottery. This approach is particularly powerful when simultaneously applied to multiple classes of artifacts, as has long been advocated by Geneste and others.

Despite the critical issues that Bar-Yosef and Van Peer address, many of the most important studies of lithic assemblages in recent decades have been conducted by scholars who associate themselves directly or indirectly with the ideas that characterize the *chaîne opératoire* approach. In continental Europe, most lithic studies today take place within the techno-economic approach developed by French technologists. By tracking every step of lithic technology—from procurement of raw material through all the phases of knapping, use, and discard—researchers can pinpoint the concrete actions of past individuals in relative time and space. When combined with similar analyses of organic artifacts and archaeological features, researchers can establish empirically very real direct links to past individual and more generally to past groups. These kinds of analyses have in recent decades helped to make

prehistory a vibrant field in which we are rapidly gaining a wide range of useful information on settlement dynamics, subsistence practices, and synchronic and diachronic patterns of behavioral variation.

What has largely been lacking in these important techno-economic studies is an explicit link to social or evolutionary theory. The greatest weakness of the French technological school is that in most cases, the research operates in the realm of description. My impression is that the technological revolution has been completed in much of continental Europe. Now new impulses are needed to direct researchers' attention beyond the mechanics of tracing the empirically derived life histories of cobbles, tools, and debitage and toward broader causal relationships with more far-reaching anthropologically relevant implications. How to best achieve this important goal is unclear at present. The critical discourse initiated by Bar-Yosef and Van Peer should help to generate the kinds of impulses needed to move beyond the impressive results thus far achieved by researchers using the *chaîne opératoire* approach. This is particularly necessary in France, where a portion of the research community has fallen into orthodoxy rather than continuing to develop innovative approaches for studying anthropologically relevant aspects of past technological systems.

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This authoritative review prompts several important reflections. First, operational sequences (OSs) occur much more widely in nature and culture. For example, Hill and Behrensmeier (1984) established the OS of disarticulation of animal carcasses under conditions of natural decomposition. In these cases, there is no agency involved. But OS descriptions of gorilla leaf processing certainly involve agency (Byrne 2003), although we may dispute the intentionality in those actions. Demonstration of OS alone, therefore, does not establish either agency or intentionality.

Second, defining an OS may allow the identification of missing links in the chain. Van Peer's previous work (1992) involving refitting prompted questions about intentions of the OS, showing that non-Levallois flakes were more often missing from the conjoin sets than the Levallois flakes were. This sounds most unlike a knapping strategy where the Levallois flakes were the *predetermined* flakes. They almost look like accidents within the OS, and this is consistent with Beyries's (1987) demonstration that non-Levallois flakes show use wear significantly more often than Levallois flakes (see Davidson 2002). Again, we need to be cautious about interpreting intentions from the OS we can establish.

Third, our greater sophistication in interpretation allows us to move beyond a simple interpretation of the standard,

Oldowan-Acheulean-Levallois-Mousterian-Upper Paleolithic (OALMUP) sequence, and this relates to the question the authors raise about the objectives of archaeological analysis. This Whiggish sense of the progress of cultural behavior has been the underpinning of much of our understanding of the evolution of human behavior, but it was always an oversimplification, as the authors seem to acknowledge. But it is astonishing that this underpinning implied in the OALMUP sequence persists, for example, in the expectation that elongated flakes relate to modernity of behavior (see the skeptical discussion in Bar-Yosef and Kuhn 1999 and in Davidson 2003). The “sanctification” of blades reached absurdity recently when some authors⁴ claimed that the elongated flakes illustrated in the report of the discovery of the new species of hominin on Flores (Morwood et al. 2004) contradicted that anatomical analysis. A more scholarly approach might have awaited the appearance of Moore’s (2005) definitive analysis of the artifacts and his discussion of how elongated flakes may occur without being the intention of the process (e.g., Moore 2007). But as we move away from the Whiggish view of stone artifact sequences, the search for the “understanding of the simple facts of prehistoric life” might lead us to no greater insight than that our forebears made stone tools and used them (to cut animal or plant materials, or to scrape wood or hide). This is a dilemma that has not been adequately resolved. Turning the facts of life into a narrative almost demands a Whiggish view of progress.

But this dilemma is part of the process that led some of us to look to an interpretation of stone tools in terms of their implications for understanding the evolution of hominin cognition—a fourth issue. The association of stone tools and cognition has a long history, including Holloway’s (1969) brave attempts to sow the seeds of a complex relationship between knapping gestures and syntax and Wynn’s exploration of Piagetian theory to interpret early hominin cognition through stone tools (e.g., Wynn 1999). When Noble and I (Davidson and Noble 1989; Noble and Davidson 1996) began our exploration of the origins of language, our intention was to try to understand something about the emergence of modern human cognition based on the theory-driven assumption that language was fundamental to it. We defined modern human behavior in terms of the combination of information exchange, planning depth, and symbolic conceptualization (Noble and Davidson 1991), emphasizing that some artifacts could be interpreted in such a framework, particularly the backed artifacts from the Klasies River and bone points generally (Davidson and Noble 1993). I would now rather emphasize the more complex aspects of the emergence of hominin cognition using the evolutionary emergence of present-day cognition we established using Barnard’s nine-

4. I do not cite the authors both to protect their reputations and to avoid giving credence to their unscholarly claims. Those who insist that I justify the existence of such claims can probably find the references by a Google search or by contacting me.

subsystem interacting cognitive systems (Barnard et al. 2007). In this scenario, activities involving complex hand-eye coordination evolved early in the sequence, requiring only the six subsystems that are sufficient to account for the known technical abilities of apes, and, by inference, of the earliest stone-tool makers. Cognition with seven and eight subsystems is known only among extinct hominins, and nine-subsystem cognition is known only among humans. Complex coordination of vocal utterances in response to visual stimuli would have emerged at the eight-subsystem framework. This would allow some vocal guidance of the learning of knapping skills without requiring all abilities of language. These emerged only with the ninth subsystem, which allowed reflective awareness without any external stimulus. So I (now) have every sympathy with the view expressed by the authors that some vocal guidance may have been involved in learning Levallois knapping, but I think a more nuanced view of the objectives of Levallois knapping and of language and cognition may be in order.

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I find the analysis of Ofer Bar-Yosef and Philip Van Peer to be perceptive and relevant. I think, as do they, that the increasing interest in the strictness of scientific analysis has distanced it from its objective, which is to understand the facts of prehistoric life. This is illustrated by the following anecdote: recently in France, while organizing an epistemologically oriented colloquium on the means at our disposal to reconstruct daily life in the Upper Paleolithic (Beaune 2007), the major specialists on lithic technology declined my invitation, some of them arguing that it was still “too early” to tackle the question of daily life. This is to be explained by the fact that technology specialists are mainly interested in the making of flint stone tools and rather less in their usage.

The authors allude to the industry of bone and antler raw materials, which has also been the object of technological analyses in recent years. But a third type of remains can also inform us about Paleolithic daily life: nonflint stone tools, which were used in a great number of domestic, technical, and artistic activities (Beaune 2000, 2004). Not complying to operational sequence (OS) analysis in the way that flint tools are, these tools have been excluded from current technological studies. Their sole phases of utilization are perceptible on the basis of their traces of use, but this is actually not a bad way to approach the facts of daily life.

As for the technological analysis of flint stone tools, I will just make a few general comments. I am in complete agreement with the idea that the reconstruction of one part of an

OS and its positioning within the category of a wider sequence is largely intuitive and remains a virtual activity, existing only in the mind of the analyst.

In addition, the authors emphasize the problem of choice of unit of analysis and the level of generalization to adopt. It is often assumed in the scientific literature that the individual assemblages found are in keeping with production standards and that below a certain threshold—arbitrarily defined—the small variations observed are due to chance. They rightly criticize what is clearly an *a priori* assumption.

Another of their criticisms, which seems extremely relevant to me, takes aim at the idea according to which the different types of debitage observed, which follow from the typologies of the prehistorians, reflect the choices made by the prehistoric artisans. It is a case of circular reasoning: these categories may well exist, but they have been created by prehistorians, not prehistoric actors. The rhetoric, which consists in claiming that the products of debitage reveal the “choice” or the “intentions” of the tool makers, is thus strongly called into question. Without denying that, for example, the availability of primary material or the needs of the moment may dictate some technical choices, they rightly suggest that these notions must be relativized. It is thus necessary to speak of end products rather than desired products; if the Levallois blanks indeed have their origins in a series of technical operations, this does not necessarily render them “desired” products, for we are unaware of what the tool makers of the Middle Paleolithic actually wished to obtain. The “choice” of this or that blank can be shown through other secondary modifications, such as traces of use on the edge, which will show that one blank, instead of some other one, had indeed been selected for use. This returns us to the necessity of studying the utilization of tools in order to highlight intentions, choices, or preferences.

In short, I largely share the opinion of the authors except for their ideas on the transmission of toolmaking. They affirm, following Valentine Roux, that oral explanation was indispensable to the transmission of Levallois debitage techniques. If the Neanderthals were most likely capable of language, it is necessary to search for proof of it somewhere other than in the transmission of techniques, because many present-day ethnographic examples indicate that some techniques are transmitted by observation and silent imitation (Beaune 2008). Implicitly, the authors suggest that the repetition of the same technique over a very long time could reflect usage of a shared language. But the great stability of techniques in the Lower and Middle Paleolithic can be explained by other means. Thus, for Gilbert Simondon (1958), the rigidity of techniques may be due to transmission through everyday immersion, starting at the youngest age (which in no way implies the possession of language). It may also result from a perfect adaptation of the technique to the needs; why modify a technique that fulfills its objectives?

Here, too, I concur with the opinion of the authors: the technological approach to tools should not be rejected, but it has little chance of revealing to us the intentions of the tool

makers, and it risks amounting to a mere formal type list of knapping products if one has not examined more general behaviors such as seasonal movements, subsistence strategies, paleodemography, mechanisms of dispersals, and the social context of lithic production.

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I congratulate Bar-Yosef and Van Peer on an exceedingly well-written, timely, and important paper. Their account of the history of the technological approach and their presentation of its various methodologies will be of great use to students of lithic technology, both in the Old and New Worlds (see also Tostevin, forthcoming). Most importantly, however, their critique of the operational sequence (OS) approach—long overdue, in fact—is a significant one, and I hope it will galvanize its practitioners into response through changes in thinking and methodology.

My comment concerns the typology/technology dichotomy and the extent to which typology should, or should not, be used in the OS approach. Historically, one of the main reasons for the development of technological studies in this field has been to counter the “typological approach.” In fact, typology and technology are often represented as opposite approaches, with the latter depicted as newer, more advanced, and more likely to yield important information as opposed to the outdated typological approach. Practitioners of the technological approach are seen as young, forward thinking, and ground breaking, whereas “typologists” are portrayed as doddering old archaeologists unable to break out of a remote paradigm. It is curious that typology and technology have become dichotomized in such a way. After all, one is a tool used by archaeologists, whereas the other describes a category of human behavior. How have these terms come to signify opposite methodologies (or perhaps the word *paradigms* is more appropriate)? In Paleolithic studies, it is clear that Bordes’s typological approach became an end in and of itself. Bordes and his followers insisted that the main task of the prehistorian was the identification of past culture groups, a goal achieved through the use of Bordes’s typology and quantitative methodology. “Typology” is thus where the study of Paleolithic stone-tool assemblages began and ended. The technological OS approach arose as a reaction to the almost exclusive focus on retouched tools in the Bordian approach, which ignored the vast numbers of unretouched lithics as well as the various ways in which these products were knapped. However, what many practitioners of the OS approach failed to realize was that a typology is needed to characterize technological products just as much as it is to characterize morphological categories. Some, like Geneste, realized this and

created explicit typologies to characterize technological products (Geneste 1988). Many others, however, shunned typology while simultaneously creating implicit typologies, such as Boëda's "predetermined" and "predetermining" flake types (Boëda 1994). These implicit typologies, which have conflated description and interpretation, are in large part the source of the problems noted by Bar-Yosef and Van Peer.

In reacting against these poor typologies, Bar-Yosef and Van Peer argue that technological studies should move away from typology. I submit that we cannot, in fact, escape typology. Regardless of one's ultimate goal, artifacts still need to be organized in some way before they can be analyzed. The search for technological patterns cannot begin until lithic artifacts are organized into coherent categories based on technological attributes and features. The creation of a typology of technological attributes should be done carefully and should be informed by the science of classification (e.g., Adams and Adams 1991). Most of all, this typology should avoid confounding description with interpretation. As demonstrated by Bar-Yosef and Van Peer with the Taramsa refits, assigning blanks to a particular stage of a reduction sequence on the basis of morphology is sometimes simply wrong. Instead, the morphological and technological descriptions of blanks must remain scrupulously separate from the interpretation of the role they played in a reduction sequence (see for example Tostevin 2003a). In conclusion, although many would agree that the ability of Bordian typology to inform us about human behavior in the Middle Paleolithic is limited, *typology* itself is not a "bad approach." It is a necessary tool of archaeology because our first task is to organize the artifacts we are studying. We must simply do it explicitly and carefully.

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Bar-Yosef and Van Peer have provided a timely call for a reassessment of the direction of Stone Age tool studies and its current dominant paradigm, the concept of *chaîne opératoire*, or operational sequence (OS). Their call for caution is one we should not ignore. A compelling case is made that the overformalized description of reduction sequences has drifted toward a new de facto typological framework, incurring all the dangers of overly rigid classification schemes. They also question the routine and often uncritical assumption of inferred intent and preferred outcome often embedded in the OS approach, sounding a much needed note of disquiet over confidence in it being able to deliver a satisfactory account of both cognitive and behavioral processes relating to tool production.

Through the author's presentation of the historical origins of the OS approach, it becomes easier to see how this overconfidence arose. For both the Lower and Middle Paleolithic

periods, the range and quality of both data sets and analytical frameworks has changed beyond measure in the past generation, with the targeting of fine-grained archaeological contexts and multidisciplinary approaches to the record enabling an ever-closer reading of stone-tool scatters. To illustrate this, we need look no further than the case study presented by the authors and their critique of interpretation of the Middle Paleolithic assemblage from Taramsa. The authors give a detailed account of the assemblage, combining typological classification with a close reading of scar-pattern supposition analysis and reconstruction of the actual reduction sequence through refitting. All three approaches are shown to have limitations and interpretational problems, and conflicts between interpretations derived from individual approaches are clearly shown. Yet the critique seems itself self-defeating if its aims were to show the overall OS approach to be flawed. The authors have instead presented us with a textbook example of how a stepwise, multithreaded analytical approach, with OS as a component, can deliver a more sophisticated and critical interpretation of the stone-tool technology. The problem is not with the OS approach itself but rather with how rigidly it is employed: whether reduction models are properly derived from the data itself or are used improperly as a priori frameworks that inevitably constrain a realistic appreciation of individual variance.

In our own research directed at the fine-grained record of the Boxgrove paleolandscape, we have tried to engage with the relationship between the spatial organization and typological variation of Lower Paleolithic biface technology (e.g., Roberts and Parfitt 1999; Pope 2002; Pope and Roberts 2005). Close reading of the *chaîne opératoire* has been pivotal in recognizing the complexity involved in the formation of the archaeological record and challenging rigid typological classifications. This has only been achieved through the synthesis of independent lines of analysis within a framework sensitive to the linear nature of reduction sequences. Consequently, we are beginning to engage with problematic and embedded biface typologies through appreciation of raw material limitations (Ashton and McNabb 1994; White 1995), mutability of form through extended reduction histories (MacPherron 1999; White 2006), specialized resharpening strategies (Emery and Pope, unpublished manuscript), and differential discard within landscapes (Roebroeks et al. 1992; Potts, Behrens-meyer, and Ditchfield 1999; Pope 2002; Pope and Roberts 2005; L'homme 2007). It would seem inconceivable to have been able to achieve interpretational levels of detailed refitting and close readings of tool reduction histories organized within an OS paradigm. Studies of archaeological signatures from sites offering in situ preservation of short-term tool-using behavior have delivered complex "life histories" of tools, from raw material provisioning, multiple use, and resharpening episodes to final structured patterns of eventual discard and incorporation into the archaeological record, the eventual "death" of a tool.

The problem perhaps exists at the interface between these

reconstructed life histories of individual tools, which we are now very much equipped to write, and the interpretational move to the reconstruction of life histories of the ancient hominins themselves. Using tools as proxies for the individual in Paleolithic archaeology is something we are certainly beginning to attempt within the current paradigm, but it is something we should be undertaking with more critical judgement. The ability to utilize lithic data set to elucidate the role of individual choices and trajectories in the formation of larger patterns of technological variation is of course premised on inevitably emic approaches to the record. This attempt to isolate the role of the individual in creating patterning seen at wider scales of analysis is very much our new frontier in Paleolithic studies (e.g., papers in Gamble and Porr 2005). Apparently random patterns of individual variation in the Paleolithic, once seen as background noise obscuring relevant processes of technological change (Isaac 1972), are not only now accessible to detailed analysis but also might offer a possible mechanism through which to understand the emic relationship between ancient individuals and stone tools.

This is a task that will undoubtedly be hindered by overly formal quasi-typological approaches to the organization of reduction. Through contributions to the debate such as that offered here by Bar-Yosef and Van Peer, we might be able to bring an approach to the archaeology of premodern humans predicated on OS to maturity, to a stage beyond trying to squeeze ever more blood from dumb stones, and to more fully allow the authors of the Paleolithic record to speak for themselves.

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Stone tools enjoy the widest time-space distribution in the archaeological record, reflecting stone's abundance and enduring value to our ancestors. Their access to and need for stone varied widely, producing a global record of great diversity. Refractory stone may be, but tools and industrial debris were worked in ways that reveal technical constraint, adaptation, and historical descent.

Archaeology's approach to lithic analysis should be as diverse as its subject. Yet until recently, much Old World Paleolithic practice remained typological. Different approaches prevail in the Americas, except where imported, usually European, approaches dominate local thought (e.g., in Brazil). Some analytical differences faithfully reflect the diversity of prehistoric lithic traditions, but some are accidents of imperfect communication in archaeology's historical development and intellectual descent. The resulting provincialism beggars us all and risks reinventions of the same wheels that differ in name more than substance.

Perhaps reflecting disenchantment with the limitations of

typology, in the 1980s, the *chaîne opératoire* concept began to dominate French and then other Old World lithic analysis. Bar-Yosef and Van Peer examine the concept's origin and analytical claims (see also Audouze 1999; Bleed 2001; Shott 2003a). However, befitting the imperfect interaction that characterizes global lithic analysis, its advocates and even constructive critics such as Bar-Yosef and Van Peer remain doubtful of the, as they put it, "claim" that *chaîne opératoire* is largely equivalent to a concept of longer standing that North Americans call "reduction sequence." This view leads them to conclude that Americanists were "adopting" *chaîne opératoire* by a different name when in fact the American concept substantially predates the *nouveau* European term.

Rather than belabor the similarities, which are explored at length elsewhere (Shott 2003a, 2007), I note only that, contra the authors' statement that "the so-called Frison effect is . . . acknowledged" and Tixier et al.'s similar claim—and curiously for an approach that ostensibly celebrates the process of knapping from first flake struck from a core to last use of tools—*chaîne opératoire* is indifferent to the point of blithe disregard to the process of tool resharpening and resulting reduction after first use. This is no mere academic point, because measurement of tool reduction is among the most innovative lines of current lithic methods (e.g., Andrefsky 2008) and has great theoretical implications for the formation of lithic assemblages and for models of technological organization (e.g., Shott 1996a).

Some statements are arguable or undemonstrated (e.g., that typology is necessary to reveal tool "life histories," that "provisioning" replaces the curation concept). Quibbles aside, Bar-Yosef and Van Peer offer a salutary thesis about the limitations of *chaîne opératoire*. The concept may be "illusory"—their word—in practice, but it is not in principle any more than is reduction sequence. Instead, *chaîne opératoire's* practical flaws owe to its advocates' sometimes implicit adherence to typological thought, not to the (dare I say) processual perspective they espouse. The authors demonstrate that broken flake tools may appear to be of Type X, but become Type Y when refitted. But this is a criticism less of *chaîne opératoire* or reduction sequence than of the rigid typology that the concept was intended to surpass but that persists in French usage.

The reduction process accommodates typology, but we might also consider more analytical, even mathematical, approaches as well. Reduction can be modeled in the dimensions of its flake products, for instance, by regression on order of removal. This approach was pioneered in the Americas (Ingbar, Larson, and Bradley 1989; see also Shott 1996b; Bradbury and Carr 1999). Recently, it was applied to Oldowan sequences (Braun et al. 2008), an example of productive interaction between New and Old World scholars and an approach that might complement Bar-Yosef and Van Peer's own painstaking refitting analysis.

Perhaps Bar-Yosef and Van Peer's most important contribution is their plea for higher standards of proof and analysis

in reduction studies. Whether we call it *chaîne opératoire* or “reduction sequence,” the process of reduction from first flake removal to last tool use requires rigorous standards of analysis, not grandiose rhetorical claims. Yet the greatest failure of reduction analysis, *sensu lato*, is to develop those rigorous standards. Old and New World traditions alike share this failing, and both face the challenge of rectifying it.

Reply

We are grateful to our colleagues who took the time to write down their insightful comments on our paper, and we are sorry that due to its circulation during the summer when most archaeologists are in the field, others did not have the opportunity to do the same. We are pleased to note that the motivation of our paper, which was to constructively add to the critical exploration of Paleolithic archaeology’s tenets, appears to have been acknowledged by the commentators. We hope that the issues raised, our own as well as theirs, will result in discussion of the *chaîne opératoire* approach in lithic analysis and its advantages and disadvantages in the ways it is practiced today. As a matter of fact, we concur with the general consensus that, as Pope puts it, the problem is not with the operational sequence (OS) concept itself but with its implementation. While reexamining the ways in which we conduct lithic analysis, we should take into account “rigorous standards” (Shott), “conceptual tools that are easy to use” (Bleed), the individual and social context of lithic production (Pope, de Beaune), and typology and classification (Monnier); they are all essential to “develop innovative approaches for studying anthropologically relevant aspects of past technological systems” (Conard).

We acknowledge Bleed’s precaution that the OS approach is only one manifestation of a more general sequential approach to lithic analysis, and we may have failed to emphasize this point. Certainly OS is also a kaleidoscopic concept wherein several threads, for example various kinds of raw material used to produce functional items, are woven into higher order chains showing groups operating in their landscapes. We have chosen to focus specifically on OS approaches to the lithic thread because these are set apart—often implicitly—by theoretical underpinnings. Our main concern was to present a critique of these principles.

As in other empirical sciences, the creation of analytical method is essential to archaeology. Data are not just simply present in the archaeological record, they need to be brought out by tools devised according to appropriate theoretical knowledge. We believe that Bleed wants to make exactly this point when he asserts that even with the cautions provided, the OS approach begins with the expectation of idealized behaviors. We have to start from a theory about the empirical world we are about to study, a conception of idealized be-

haviors and appropriate units and scales of analysis. A particular challenge for archaeology is that where its subject matter—past human behavior—is sequential in nature, its empirical evidence, except the cases where there is stratigraphic superposition, is not. Even if sometimes we can observe sequences of actions, such as in the refitting of lithic production sequences, the contextual thoughts that bound these sequences together can only be our own theoretical constructs. Analogically, but at a different timescale, the identification of patterns of descent or “technological lineages” is likewise a construct even under the best possible conditions of chronological control. Hence, we find de Beaune’s insistence on the issue of what desired end products might be highly relevant. Davidson points out the questionable nature of end products when confronted with the patterns that emerge from refitted sequences. He is correct, although we would prefer to rephrase: contrary to the expectations under a desired end product model, Levallois flakes often appear to be present among the scatters of their production waste. Yet we agree with his point on intention. “Our” end products, explainable in terms of and consistent with mechanical theory, say nothing about “their” contextual intentions. If we can grasp any persistent general concerns at all underlying myriads of contextual prehistoric intentions regarding Middle Paleolithic blank production, we are inclined to recognize these at the level of size and proportions, which are allometric in nature (Van Peer, Vermeersch, and Paulissen 2009). This also fits much better with the pattern that Davidson refers to.

We also do not deny the use of typology for systematic descriptions of morphological observations, and we agree about it with Monnier. One cannot imagine the transmission of information among archaeologists without the use of accepted formal typologies. As Monnier indicates, it is curious indeed that typology and technology have become dichotomized as two alternative analytical systems and, *a fortiori*, that such a conception might still persist today. OS was introduced to provide the required context for more behaviorally informed interpretations of lithic variability. When they advanced their functional interpretation of the Mousterian facies, Binford and Binford (1966) already alluded—obviously not identifying them as such—to the necessity of techno-economic OS studies. They asserted that typological variability would never be understood without them. The relevance of this prediction was later demonstrated by Turq (2000) in particular. At the most basic level, the classification of tool forms itself arguably needs the context provided by their *chaînes opératoires*, given the amount of equifinality that such classification systems tolerate. In either case, OS has been conceived of in terms of a system of classification as noted by Bleed. Only the criteria were different. Hence, as we have tried to argue, they are homologous approaches in particular when in many present practices the identification of reduction methods prevails. The true dichotomy in our opinion is at the ontological level: do we need classification systems that are essentialist in nature or rather of a materialist ontogenetic

kind, analytical tools capable of dealing with variation and transformation (O'Brien and Lyman 2000)? When Bleed calls our attention to the theoretical context of the Americanist "reduction sequence," and Shott speaks of the processual perspective that *chaîne opératoire* espouses, we suspect that this is partly what they are hinting at. The challenge is there if, as Conard states, the formulation of testable hypotheses grounded in social and evolutionary theory is required to move beyond mere description.

It is almost superfluous to say that such OS studies will have paramount significance for research themes, such as the cognitive abilities of Neanderthals and modern humans, as mentioned by Davidson. As a matter of fact, this is also a reason why we chose to focus on the Middle Paleolithic and, in particular, on the issue of blank-production systems and their significance. Both de Beaune and Davidson touch on the issue of language and, in particular, on whether it is a precondition for the transmission of technological knowledge we tend to associate with Middle Paleolithic blank production. Together with Davidson, we acknowledge the long history of the relationships sought between the rules of material production and syntax as well as the intricately linked evolution of technology and language (Lieberman and McCarthy 2007). However, from the archaeologist's position, it seems unavoidable to admit that this issue is beyond our analytical capacities and must be relegated, at best, to the domain of informed interpretation. Several authorities argue that the suite of methods for sequential core reduction can be simply learned by watching and imitating: others see the necessity for linguistic explanations.

In either case, however, the physical proximity between the skilled knapper and the other person is needed. In this context, Pope brings the role of long-neglected individual agents to the foreground. They can be the source of patterned archaeological variation in local high-resolution conditions and forces of cultural change. We strongly support this view and second his plea to give them appropriate attention. OS studies aiming at detecting variation in long, continuous sequences of data are capable precisely of this. Over the longer term, the transmission of practical and conceptual knowledge takes place among groups of people related to each other, and in a world of foragers, kin relationship provides the strongest tie. Through accidental or intentional encounters, it may further spread among other groups. Provided that enough viable biological units can be involved, a cultural phenomenon may ultimately breach the threshold of general archaeological visibility (Leroi-Gourhan 1964). If some credence is given to this mechanism of ensuing transmissions, it may mean that at least the basic principles of, for example, the Levallois production system emerged in a core area. Alternatively, convergence may be the rule in such technologies with relatively narrow windows of opportunity for variation, especially when time depth is sufficient for multiple reinventions. Developing both an analytical system of the kind suggested above and an appropriate middle-range theory to distinguish among the

archaeological manifestations of such mechanisms is a first step toward answers to questions of this kind (O'Brien, Darwent, and Lyman 2001).

Finally, we would like to comment in a more general order on the dilemma perceived by Davidson. We concur that if Paleolithic archaeology's *raison d'être* were the recording of narratives about the simple facts of prehistoric life, never demonstrating any greater insight than that tools were made and used to cut animal materials or scrape wood, it might just as well be abandoned. Nobody would disagree with this simplistic assertion, but let us make no mistake about the epistemological consequences it can have, reaching as far as redebating archaeology's anthropological or historical perspective and method. We think that its first role is to elucidate human evolution by synthesizing the scattered local remnants of prehistoric behavior to historical scenarios. Yet however sophisticated, a full description of historically contingent processes does not equal explanation of cause and effect and, hence, true understanding and predictive force. We believe that using its unique long-term perspective, Paleolithic archaeology can and must contribute to evolutionary theory. To us, contextual and processual orientations are not as paradigmatically opposed as is often perceived. They are rather enchainned in different scales of analysis and generalization. The concept of OS has the potential to provide analytical tools at a level with a mature scientific enterprise. We hope that this paper and the comments that it has solicited have contributed to this.

—Ofer Bar-Yosef and Philip Van Peer

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