Micro-wear in perspective: a sympathetic response to Lawrence H. Keeley

George Hamley Odell

This paper has been written in response to a recent article published in this journal by Lawrence H. Keeley (1974b). Keeley's article presents a critique of the technique and methodology (or lack of same) of recent micro-wear analyses of chipped stone tools. Far from being a critique of Keeley's critique, the present effort constitutes an attempt to expand his criticisms and present a structure which may be useful to other researchers. Although my viewpoint differs in detail from that of Keeley, our basic outlooks are similar; therefore, I shall expend little effort reiterating points he has already made.

Instead, I shall concentrate on considerations other than methodology and technique – considerations which have an import just as great, but which have not been adequately discussed in the literature. These topics concern the goals and purposes of micro-wear analyses, their foundations and substance, and the factors which govern the variation in edge damage patterns. In other words, as Keeley has already addressed the question, 'How do we get there?,' this paper will address such allied questions as 'Where do we want to go?'; 'How do we know what we know?'; and 'What might we consider before being satisfied that we know it?'

In the substratum under my approach lies the belief that the study of damage patterns resulting from utilization is the most useful method in assessing the function of prehistoric artefacts (i.e. in performing a functional analysis). The potential of such an approach has been demonstrated by Semenov (1964) and others with regard to tools used intentionally on various materials and by Ahler (1970), Odell (n.d.-a) and others with regard to projectile points. The contention that our present need is not for review articles but for primary research (Douglas Price, pers. comm.) is basically correct. Yet certain trends have already developed which should be evaluated before being uncritically accepted and pursued. The present article outlines some of these trends and presents some directions which these lines of inquiry could take in the future.

Experimentation

There appears to be general agreement that reliable micro-wear analyses should be based on a cadre of experimental information (Semenov 1973; Sonnenfeld 1962; Keeley 1974b). Keeley (1974b) expended a considerable amount of praiseworthy effort discussing both the factors to consider in the erection of an experimental framework and the lack of adequate controls which has contributed to our present failure to construct same. He was not very specific concerning the form that such information should take, although many of his general comments apply directly to the publishing of experimental data. The discussion which follows is directed towards the researcher who might wish to enlist the experimental data of others in his own analyses. Certain experiments have already been published. The most recent series (Tringham *et al.* 1974) includes data on which the author's own work is based. The comments which follow should be considered as an attempt to evaluate the presentation of experimental data in this article and in others which preceded it and to present ideas which might help to clarify such experimental data and to facilitate its employment by others.

First and most importantly, *all* the variables must be published. Among the more salient of these variables are:

1 Exact description of the activity performed.
   (a) Type of stone used; where acquired.
   (b) Description of fracture properties of this stone.
   (c) Kind of stroke used in performance of activity.
   (d) Approximate length of stroke.
   (e) Rate of work (approximate number of strokes per unit time).
   (f) Method of prehension.
   (g) Duration of experiment – preferably in number of strokes, rather than length of time.

2 Exact description of the material worked.
   (a) Physical properties; form; hardness.
   (b) Manner by which it is held or placed.
   (c) Kinds of backing used, if any.

3 Exact description of the results.
   (a) Whether or not piece was observed before use.
   (b) Whether stone was washed after use or left unwashed.
   (c) Whether or not observations were made at various stages during use. What they indicated.
   (d) Whether or not the stone was coated or prepared before observation (e.g. by metallization, methyl violet, indian ink etc.).
   (e) Whether or not photographs were taken at various stages during use. What they showed.
   (f) At which magnifications observations and photographs were made.
   (g) Which forms of wear are present; their locations; patterns.

Taking all variables into consideration, the most completely recorded series of experiments, though not mentioned by Keeley, is probably the eight by Keller (1966). Other reported attempts have contained serious omissions, a fact which either makes one doubt the validity of the results or simply renders the procedure inexact and therefore less usable to others. In addition, the experiments become unrepeatable and therefore slightly less than scientific.

An example of the omission of several of these variables occurs in a recent article by Hayden and Kamminga (1973). They report a series of wood-adzing experiments conducted by Kamminga. Their description, however, fails to mention at least half of the variables suggested above: e.g. kind and length of stroke, prehension, duration of experiment, shape of worked material etc. To be fair, their article was intended primarily to discuss the work of Gould, Koster and Sontz (1971), and not to report a series of experiments. Nevertheless, they present enough details to render the experiments tantalizing, but not reproducible. Nor do they indicate that the experiments will be more clearly described in the future.

A similar omission concerns the handling of the experimental (or ethnographic) piece after use. In woodworking experiments, for example, one would prefer some assurance that the tools were thoroughly washed after use and that lustre or gloss present on the piece (see Hayden and Kamminga 1973: 4–6; Kantman 1971) is not merely a remnant of the worked material adhering to the surface. Examples of omission could be marshalled for most of the variables listed above, but since Keeley has already produced several others, these should suffice.

The other major stumbling block to experimental work is the presentation of the results. It includes words, photos and drawings and concerns clarity of expression. Drawings, i.e. inked representations of edge damage patterns, have been used very little in experimental micro-wear analysis. To be useful and accurate, they should be taken directly from photographs. The drawings published by Barnes (1932), though slightly idealized, generally conform to reality and constitute quite faithful representations of scraper wear. On the other hand, Kantman's (1970b: 285) hypothetical representations of scarring patterns on denticulates, though helpful, lack the authenticity of having been taken from actual used pieces. Their didactic power, however, would support their being published along with drawings taken from photos.

The utility of photography (McDonald and Sanger 1968; Mirambell 1964) in the presentation of experimental results is obvious. To be most useful, photographs should remain unaltered, i.e. the scars should not be outlined in ink, as Kantman (1970a) has done. Such outlining obscures the definition along the rear edge of the scar and thereby destroys information. If the precise outlines of edge damage are difficult to determine from a photograph, such information can be provided by supplementary illustrations.

There is a crying need for precision and standardization of terminology in micro-wear studies. Certain concepts remain at best vague. These include 'nibbling' scars (Keller 1966: 506; better defined by Hester, Gilbow and Albee 1973) and 'grignotement' (Kantman 1971) – a concept similar to 'nibbling'. Until a work faithful enough in its representation of edge damage and comprehensive enough in its scope to form a basis for standardization and precision appears, one can only hope for complete and uniform definition of terms by each individual researcher.

The linear evolution of micro-wear studies

S. A. Semenov's (1964) pilot study on micro-wear analysis has had a profound and justified impact on the growth of this analytical technique. Based mainly on patterns of striations found on the surfaces of stone tools, Semenov's work has truly shown us the power of the straight line. His influence can be noticed in the work of several current researchers (e.g. Gramsch 1966; 1973; Keeley 1974b). Many authors, however, flatly state that they find few or no striations, whether on experimentally worn tools (Ahler 1970; Bordes 1971; Kantman 1970a), ethnographic material (Gould, Koster and Sontz 1971), or prehistoric collections (Rosenfeld 1970). Some of the failure to observe striations is due to inadequate technique, as Keeley notes, but much of it is simply because the striations are not present. Semenov himself admits that linear utilization traces can be absent even though the tool has obviously been used (1970: 8; 1973: 110).

Most archaeologists who have expressed an opinion on the subject appear to agree that striations present on utilized tools are caused by sand and grit inclusions in the worked material (Hayden and Kamminga 1973: 7; Semenov 1964: 15). Gramsch (1966) has also suggested that breakage or splintering of the tool within the hole or groove it has produced may cause striating of that tool. This I myself have observed in an experimental situation. The point is that the cause of the striating is probably accidental and will not occur at all times and in all situations. Therefore, to rely on this index in interpreting the functional manifestations of an entire stone tool assemblage would appear theoretically hopeless. Furthermore, although the presence of striae may indicate a great deal about activity, it indicates very little about the type of material worked.

In addition, striations themselves can be difficult to interpret. F. Bordes (1967: 54) has pointed out the natural forces at work striating flints. Technological factors can complicate matters even further. A technique in point is the practice of 'scrubbing' a flint before serrating it. By rubbing the flint edge with a rough stone, one can produce lines which will allow a percutor or other serrating instrument to hold its place, thereby creating tiny platforms for the removal of serration flakes. The lines created by 'scrubbing' can appear quite similar to utilization traces. From just such a situation has recently arisen the Great Stockton Point controversy (Nance 1971; Hester and Heizer 1973; Sheets 1973; Keeley 1974a).

For the reasons stated above, the majority of the work subsequent to Semenov's book has been concerned with forms of wear other than striations. These forms of wear include edge scarring, abrasion and polish. 'Edge scarring' is taken here to mean the tiny chips removed from the edge of a stone tool under pressure. 'Abrasion' refers to the smoothing, or wearing down, of corners and projections produced by external forces. Some researchers refer to this phenomenon as 'edge rounding'. And 'polish' is any area exhibiting a lustrous finish.

Reasons for the popularity among researchers of these three forms of use-traces are not difficult to find. They can be just as informative as striae and they have the advantage of not requiring special high-powered equipment. Rosenfeld (1970), for example, has reported very satisfactory results of wear analysis on Magdalenian end scrapers using a microscope which attains a magnification of only ×80. In fact, she reports that most of her identifications, as those of Semenov (1964), were made at much lower magnifications than this (e.g. ×20). MacDonald and Sanger (1968) also find lower magnification (×24) best for functional analysis. In no way can we accept Keeley's (1974b: 325) statement that with the use of lower magnifications 'potentially adequate and representative samples of implements become unrepresentative and insignificant'. Capabilities for higher power (up to ×200 or more with special techniques) are nice to possess, but the question generally revolves around what one is looking for. There is also a certain element of not seeing the forest for the trees here, and often the minute detail, while important in itself, is not as informative in relation to the entire piece as the macro-damage one sometimes overlooks while ferreting out these details.

There are at least three other major advantages in analysing forms of wear other than striations. First, they usually require no special preparation of the flint, such as vacuum metallization or coating with indian ink or methyl violet. These preparations, proposed by Semenov (1964) and defended ardently by Keeley (1974b), can indeed (if properly applied) be helpful to the recognition of striations under the microscope. Their use can also obscure other forms of wear such as polish and abrasion, so that observation and recording of these variables would have to be performed separately and before the foreign substance is applied to the stone. Second, their use can be very time-consuming. This point is especially true of metallization. If one envisages the analysis and recording of thousands of stones from complete settlement units, as I am engaged in at present, one begins to question those methods which may take more time than they are worth. Third, metallization, in particular, is rather costly and is therefore mainly applicable to small samples. In addition, part of the object is affixed to a base and is thus rendered unavailable for further study. Finally, the very causes of striations, i.e. from granular and foreign inclusions on the surface of the material being worked, suggest that their potential in indicating the nature of that worked material is slight indeed.

By making these remarks I do not wish to discourage or disparage the employment of the striation index in further endeavours at functional analysis. I search for striations myself and find them very useful when they occur. Yet I would firmly agree with R. Tringham (1972: 146) in approaching functional analysis by methods which would employ all categories of use traces, their location on the tool, the form of the tool, edge angles, and any other relevant index which might bring us closer to a realistic and multivariate appraisal. The more of these variables one accepts, of course, the more complex become their interrelationships. The necessity to record and evaluate these attributes, as well as to introduce other pertinent factors into the analysis (e.g. provenience data, location of features, animal bones etc.), leads us directly to the use of quantitative and statistical analytic methods, as suggested by Keeley (1974b: 331).

The forms which edge abrasion and polish can assume have been discussed in Semenov (1964) and elsewhere (Tringham 1972; Ahler 1970 etc.). Interpretations of these two phenomena can be far from simple and their formation in several cases probably exceeds uncomplicated explanations such as lengthy use or the presence of silicates in the worked material itself. There can also be natural causes for them. In addition, my own as yet unpublished work on microlithic projectile points (Odell, n.d.-a) strongly suggests that both abrasion and polish can be produced by forces of percussion inflicted upon a flint as it strikes, penetrates, or caroms off a great variety of possible objects.

In most cases, however, it is possible to distinguish polish and abrasion caused by utilization and that caused by natural means or by percussion. In this regard, the shape which this wear assumes and its location on the piece are the most instructive variables to consider. In neither category does there exist the internal variability which one finds, for example, in utilization scarring. For this reason, most authors who have reported the presence of abrasion on a piece have contented themselves with merely recording its existence (Lenoir 1971; Massaud 1972) or have made a simple double- or tri-partite division (e.g. the 'rounding', 'smoothing' and 'grinding' categories of Ahler 1970). Those who have reported polish have usually distinguished between additive sickle gloss and localized abrasive polish (Witthoft 1967; Tringham 1972: 145–6), and then concentrated on its location on the piece. The reason for the lack of internal categorization within either form of wear is that internal variability is not easily ascertainable through a microscope. Therefore, when these forms of wear are present, they can be quite useful, but their internal variability, by itself, cannot provide very fine distinctions among the various activities and worked materials.

Experiments conducted so far at Harvard University, in which I myself have taken part, have indicated that edge scarring by utilization manifests far greater internal variability than polish, abrasion or striations. This contention is supported by existing published experiments (Keller 1966; Kantman 1970a–c etc.). In addition, scarring usually appears before any of the other forms of wear. These two facts render the scarring index more responsive to function than the others and therefore potentially more desirable to use, especially when dealing with an entire stone tool assemblage. If scarring is the first to occur with utilization, however, it is also the first to occur as a result of other forces, both human and non-human. Crabtree and Davis (1968: 428), for example, call attention to hinge-fractured scars which hitherto 'have been considered results of use; they are really only products of resharpening'. It is encouraging to note, however, that experiments performed in order to assess edge damage as a result of forces other than utilization (e.g. trampling in sand etc., see Tringham, *et al.* 1974) have demonstrated discernible differences between this damage and that produced by use.

There is another aspect of this problem which warrants attention. That is, scarring from use has at times been considered on the basis of a few, but not all, of its attributes. Keeley (1974b: 330–1), for instance, has chided Gould, Koster and Sontz (1971) for providing us with the ambiguous concept of 'small terminated flakes' to describe the wear found on certain Australian aboriginal wood-scraping tools. Hayden and Kamminga (1973) registered similar objections, which elicited a slightly less than dispassionate retort from Dr Gould (1973). In many respects, the objections are legitimate. The category of 'small terminated flakes' as such *is* both imprecise as a term and widespread in its possible origins. Hayden and Kamminga (1973: 4) are undoubtedly correct when they report that, upon inspecting the contents of a bag of flints found in an aborigine's shelter, 'the average frequency of "generally terminated" flake scars . . . for one bag of unused adze flakes was about the same as on used tools'.

But these gentlemen have missed the point. The important aspect of edge damage by scarring is not only in the frequency of this or that type of scar, but in the *pattern* of these scars along an entire edge or used surface. Gould, *et al. did* illustrate a particular form of scar patterning which appears very different, under the microscope, to that caused by random percussion such as one might produce on a flint by carrying it in a bag or by subjecting it to a myriad of other random forces. The main problem which Gould, Koster and Sontz encountered was in not possessing an empirical framework in which to compare this scarring pattern to those produced under other conditions. Thus when it came to drawing analogies to prehistoric collections, such as that from La Quina, they could suggest a relationship on the basis of similarity, but could not indicate the parameters of variability of the scarring pattern on which the relationship was based.

Their difficulties in interpretation are completely natural under these conditions and should not detract from their original observations, which are helpful. The real danger inherent in this controversy is the isolation of certain elements of the entire pattern (in this case, the shape of the scar and the definition along its back edge) and the basing of one's reasoning on these elements alone. If Hayden and Kamminga's logic is correct, then there is little hope for an analysis of this kind and we should all go back to countng striations.

The question which follows logically from the previous discussion is: which *are* the elements of scarring which should be isolated because of their discriminatory value? Experimentation has indicated the desirability of separating use scarring into four constituent elements. These elements should, of course be further categorized. One method of classification is suggested below:

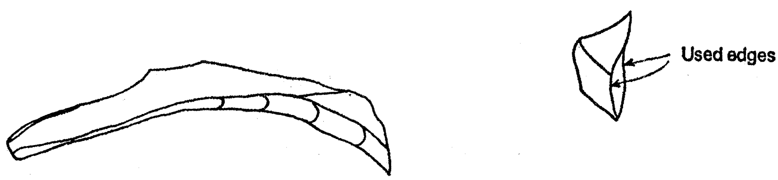
1 The shape of the scar. Employing a simple quasi-geometric system, utilization scars (or intentional retouch scars, or any other type, for that matter) can be classified into: scalar; trapezoidal (generally of the 'terminated' variety); triangular; rectangular (usually 'stepped'); irregular; and 'sliced' (a half-moon shaped removal which cuts into the entire edge, rather than one surface from that edge).
2 The size of the scar. Can be separated into small, medium and large by an appropriate method, or measured.
3 The definition of the scar along its rear border. Can be separated into well-defined, medium-defined and ill-defined.
4 The distribution of scarring along a utilized edge. Can be separated into even and uneven; wide, close and run-together; and occurrence on dorsal and/or ventral surface.

Of course, not every scar on a utilized edge is assessed in such a manner. Instead, this information is best recorded for each utilized section of a piece, oriented on a polar coordinate grid.

Obviously, the above-mentioned categories cry out for further definition. It is not my purpose in this paper, however, to describe a system of recording, retrieval and analysis for functional data. A more detailed report of this nature will appear in another publication. The outline above should merely illustrate some of the factors of any pattern of scarring which can be isolated for analysis and comparison. At this stage in our knowledge, the selection for analysis of any of these factors in isolation from the others can only lead to a 'fossile directeur' approach to functional data which would seriously limit the scope of further endeavours.

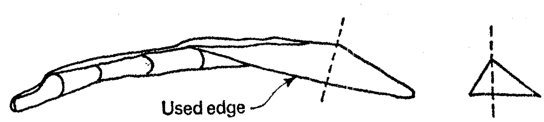
The major variables which we hope to isolate by functional analysis are those contributed by the activity in which a piece was involved and the material on which it was worked. That such a goal can be realized over a considerable range of activities and worked materials has been indicated by Tringham (1972). There are other variables, however, which can also contribute to the formation of particular sets of edge damage attributes. Some of these – natural causes, such as solifluction (Warren 1923, and several others), and humanly-induced causes other than tool use, such as intentional edge retouch (Crabtree and Davis 1968) – have already received considerable attention.

There are other factors which have unfortunately received no discussion whatsoever in the literature. Experiments indicate that, especially for the activities of cutting and sawing, the morphology of the edge itself may be an important determinant of the scarring patterns which develop through use. This observation appears especially relevant with regard to the surface from which the scars are removed. Such factors I have termed: 'edge curvature'; 'convexity/concavity'; and 'edge displacement' (see fig. 17, *a–c*). 'Edge curvature' refers to the arc formed by the edges of pieces when the edge is observed straight on. 'Convexity/concavity' describes qualities of the dorsal and ventral surfaces when viewed in cross section. These qualities are termed 'convex', 'concave', or 'straight'. 'Edge displacement' refers to two characteristics of a used edge: the relative



a) EDGE CURVATURE

b) CONVEXITY/CONCAVITY



c) EDGE DISPLACEMENT

Figure 17 Salient elements of edge morphology. All three pieces illustrated here are of flint and were found in one two-meter square unit at the Mesolithic settlement of Bergumermeer, Netherlands. All three have been assessed as having been used for cutting or sawing a material of medium hardness, such as wood. The piece labelled (*b*) is broken, affording a cross-section of the used edge. The piece labelled (*c*) is shown in two views. A cross-section, taken at the dotted line, is illustrated on the right. Approx. 1/1

distance, or displacement, of the dorsal 'back' to the ventral surface; and the relative latitudinal distance between the edge and the 'spine' (or the top of that facet which forms the edge). If the relative distance between the 'back' and the ventral side is great and that between edge and spine is slight, for example, the angles formed between either surface and the worked material will differ when the tool is held in the hand or in a haft. The same result will occur if the ventral surface is greatly curved or contorted and the dorsal 'back' is concave or is only very slightly displaced from the ventral surface. If the tool is used to cut or saw, the difference in angles caused by this characteristic will produce differential pressures on the edge. Such differential pressures force more, and often different, scars off one surface than off the other, thus altering the scarring pattern.

These variables of edge morphology are – like everything else in this subject – in their initial stages of analysis and concept formulation. While the range of edge damage variation caused by activity and worked material is now beginning to be more clearly defined, the effects of edge morphology have seldom been considered and remain largely a mystery. Other factors have probably not yet been discovered. One variable which Keeley (1974b: 330) feels is important is the raw material of the stone used as the tool. If one is searching primarily for striations, then the hardness of the stone tool may be of importance. Quartz tools, for example, should exhibit fewer striations than flint tools having been used in a functionally equivalent manner (although we are not knowledgeable enough about the forces causing the striations to be very certain of this statement). If one is looking for scarring patterns, grain size will be a major consideration, and there will be enormous differences between, for example, sandstone and flint. Fortunately, where more easily worked rocks were available, Stone Age man, at least in Europe, usually did not choose quartz or sandstone as primary industrial materials. He tended to choose relatively fine-grained, crypto-crystalline substances such as flint and chert. These materials possess hardness properties similar enough for our purposes and break according to laws of brittle fracture, which occur 'practically without plastic deformation over a cross section perpendicular to the axis of the specimen' (Timoshenko 1956: 431). Therefore, one should not take Keeley (1974b: 330) too literally when he says 'that the control experiments be conducted with copies constructed of the same rock type'. To make copies of every individual type of flint, chert, chalcedony etc., which Stone Age man often picked out of surface exposures, beach deposits etc., and distributed liberally around his habitation areas would take more time than most of us have at our disposal and is probably not what Keeley means, anyway.

The use of use – or, why we do it

The initial reason for performing functional analysis is to discover the range of activities engaged in by prehistoric man. If successful, one may possibly be able to deduce certain adaptational and economic patterns and pursue questions relating to culture process and change. To achieve at least a part of these ends, specific sub-goals have in the past been constructed. These include the technological aspects of tool use and re-sharpening techniques (e.g. Frison 1968); the use traces present on single tools (Bordes n.d.; Lenoir 1971), on categories of tools (Massaud 1972; Hester and Heizer 1972; Hester, Gilbow and Albee 1973), or on a small, morphologically mixed sample from a single site (Odell, n.d.-b); and the distribution of discrete functional units over a site, using morphologically limited samples (Ahler 1970; Chappell 1973). For several authors the 'form/function problem' (i.e. does form equal function?) has been a major consideration (White 1967; 1968; Semenov 1973; Sonnenfeld 1962). All of these questions have their place in the construction of a conceptual framework derived from the regularities found in the functional data of many researchers in many situations.

The next goal, at least in terms of sheer magnitude, is the functional assessment of a large, variegated, and representative sample from a Stone Age settlement or habitation area. There are good reasons why such publications have been slow in appearing. First, due to the many problems of interpretation and the non-traditional nature of the subject, there has been a certain understandable reticence among prehistorians towards venturing on to such uncertain ground. Second, as Keeley (1974b: 334) has suggested, it takes a considerable amount of time and effort to attain any reasonable degree of proficiency in this field. Third, we still know very little about the actual content and variability of use traces to be found in a complete stone tool assemblage. And finally, the number of such sites which have been excavated in a manner which would render such a sample meaningful and trustworthy is severely limited.

To achieve any of these goals in the functional analysis of stone artefacts, one first confronts the crucial step, i.e. not in assessing *type* of wear, but whether or not wear is actually present at all, and if so, where that wear occurs on the piece in question. When certain prehistorians, for example, discuss the special consideration of a tool's 'active part' (Bordes 1967: 29; Pradel 1972–3, pers. comm.), one must bear in mind that this part can be extremely difficult to deduce, much less separate from other parts. I am reminded here of one Jean Verheyleweghen (1951), who saw the *retouched* side of Magdalenian backed blades as the active part, used as a file in the manufacture of harpoons. Whether Verheyleweghen is right or wrong, this example indicates that all is not what meets the eye and that a great deal of caution is required when attempting to isolate this working part. At the same time, its successful identification as functional, regardless of type of function, is usually the most difficult step. Only when functional parts are able to be recognized with confidence and consistency by the observer can we begin to discuss goals in terms of that particular analysis.

Recently there have been rumblings concerning a goal not mentioned above: the establishment of a 'functional typology' (Bordes 1970: 202; Pradel 1971: 563). To my knowledge, there has been only one serious attempt to construct one – that by White (1969) for certain aboriginal stone tool assemblages from the Australian New Guinea highlands. Although the actual functional import of White's categories remains somewhat elusive, and there may be considerable internal variation within the categories, they nevertheless appear in regular patterns. The primary reason for their apparent discriminatory power is that the industries with which White is dealing are exceptionally amorphous morphologically, so that function and morphology appear to continuously cross-cut one another. No one has yet attempted to apply White's technique to an assemblage possessing determinable morphological 'types'.

Some authors, however, have supported the concept of a functional typology without going into much further detail. In one such effort, Pradel (1972–3) has organized the elements of the French Palaeolithic tool kit into various categories according to morphology, technology and 'utilization possibilities'. These 'possibilities', however, remain as vague as they at first sound, and the study is apparently based on no, or very little, direct experimental evidence. His typology emphasizes 'the most active part of the tool' (p. 50), but considering the discussion above, which part is that? It takes a detailed microscopic analysis to ascertain it. And if 'functional possibilities' be our main determinant, then we must be aware (and Pradel, in fact, mentions: p. 50) that for many groups of tools, there are already indications of multiple function: projectile points (Ahler 1970; Nance 1971); burins (Bordes 1965); scrapers (Rosenfeld 1970); bifaces (Gramsch 1973); and so on. It is quite evident that by invoking 'functional possibilities' without a great deal of further functional analysis, we are no further advanced in our knowledge of prehistoric activities than we are with the old morphological typenames charged with functional overtones, many of which became 'the types which were suggested only superficially as categories for the pigeon-holing of diverse materials' (Sonnenfeld 1962: 60).

Kantman has also proposed the establishment of 'functional typologies'. Although no more precise than Pradel concerning the accomplishment of such a task, he also supports a system which would combine morphological and functional attributes in order to become more responsive to function (1969: 269). He admits, however, that he would not support a system with categories such as 'lame étranglée aux striations d'usure sur les concavités bilatérales' or 'micro-encoches sériées 2 accidentelles sur 5' (1970a: 278). He would also employ statistical methods (of an unspecified nature) to derive functional regularities (1969: 267).

Although seemingly logical, Kantman's proposals suffer from inconsistencies similar to Pradel's. He criticizes morphological typologies for being subjective, so that observations differ from observer to observer (1969: 265), but then he proposes to link functional observations to this subjective framework. He does not appear to recognize that functional assessments are probably at least as subjective as the best morphological ones at the present moment, and that they promise to remain so for at least a few years. To combine these two subjective elements into a sort of 'grand typology' would probably only compound the misery.

The best alternative when dealing with an assemblage or sample containing definable types is to keep function separate from morphology and technology. 'One must deal with typology and functionology as with two quite different approaches to study archaeological data, in the case of palaeolithic tools' (Semenov 1970: 8). By doing so, not only do we avoid compounding the errors of both, but we run much less risk of one predetermining the other in our analyses. In addition, the statistical methods which one can employ on these variables are stronger and easier to use by analysing functional data separately from morphological and technological elements, and synthesizing afterwards. First, by keeping them separate it is much easier to interpret variability as attributable to one or another major category – whether functional, morphological or technological. And second, small sample size often requires the lumping of categories in the performance of certain statistical tests. By treating function as a separate entity, and by treating morphology and technology in the same manner throughout the same sample of stone tools, one retains greater frequencies within units than if one combined all these elements beforehand. A system of combined morpho-functional categories, such as that implied by Kantman, appears more suitable to the generation of simple percentages than to the employment of actual comparative statistics and measures of significance among groups.

Finally, there arises the question of why we really want to study the function of stone tools in the first place. As mentioned above, there are several subgoals which various authors have addressed. In each of them, however, runs a common thread. Each is attempting to depict some aspect of the behaviour of ancient man, of man's response to conditions which he saw around him. In assessing this response, an author normally categorizes his variables, whether they be forms of edge damage or edge angles or whatever. He is thereby constructing a sort of 'typology'. The point is that one should select variables which will enable him to answer questions concerning ancient cultural activities and behaviour, rather than merely to erect another classificatory system in which to 'pigeon-hole diverse materials'. Obviously, some sorts of 'functional typologies' will have to be used in the future. Their efficacy will be largely determined by the researcher's general approach and the questions he asks of his data.

Conclusion

At the beginning of this paper, three questions were posed. They were, in order of their coverage: (1) How do we know what we know? (2) What might we consider before being satisfied that we know it? and (3) Where do we want to go? In answer to the first, I would agree with Keeley that the experimental approach to micro-wear analysis is the only relatively secure manner by which to proceed. There are preferred methods by which to present this material which facilitate comparisons and render the experimental information easier for others to use. Some of these I have attempted to indicate. Concerning the second, I would support an analysis which utilizes evidence presented by all possible forms of use damage, rather than an overemphasis on one form or another. This evidence must be tempered by a thorough knowledge of the damage produced by forces other than wear. Such forces include not only natural and humanly-induced but not functionally-related variables, but also elements of edge morphology which might cause differential scarring on one surface or the other. And in response to the third, I would argue for an approach to functional analysis which attempts to examine questions of culture process and change by way of prehistoric activities, rather than the erection of another typological construct which would both debilitate extant morphological systems and possess none of the advantages of modern techniques.

This article has attempted to indicate some trends within this rapidly growing line of inquiry. In response to these trends, it has endeavoured to offer some direction and voice some opinions concerning various courses of action. Although written partially in response to Keeley's article, it should form a complement to his. Different questions are addressed and different solutions offered. If it appears over-critical of past efforts, this impression is not intended. The proliferation of literature on any subject necessitates periodic appraisal of direction and goals. In the process of appraisal, those opinions with which an author disagrees will necessarily be exposed, whereas those with which he agrees will often be allowed to gloat in silence.

Acknowledgements

This paper has profited from the input of a number of individuals. First and foremost among them is Dr Ruth Tringham, Department of Anthropology, Harvard University, who introduced me to this field and offered valuable advice in the writing of this paper. The paper has also profited from discussions with the following scholars: Barbara Voytek and Anne Whitman, Harvard University; Douglas Price, University of Wisconsin; and Drs J. Butler and Raymond R. Newell of the Biologisch-Archaeologisch Instituut, Groningen, the Netherlands. The author, however, assumes full responsibility for the ideas contained herein. Finally, special appreciation is due to Dr Newell and

Professor H. T. Waterbolk for use of their facilities at the Biologisch-Archaeologisch Instituut while writing this, and Mr W. Dijkema for producing the illustration.

28.ix.1974

Biologisch-Archaeologisch Instituut
Groningen

References

Ahler, S. A. 1970. Projectile point form and function at Rodgers Shelter, Missouri. *Missouri Archaeological Society Research Series 8.*

Barnes, A. S. 1932. Modes of prehension of some forms of Upper Palaeolithic implements. *Proceedings of Prehistoric Society of E. Anglia.* 7:43–56.

Bordes, F. 1965. Utilisation possible des côtes des burins. *Fundberichte aus Schwaben.* Neue Folge. 17:3–4.

Bordes, F. 1967. Considerations sur la typologie et les techniques dans le Paléolithique. *Quartär.* 18:25–55.

Bordes, F. 1970. Réflexions sur l'outil au Paléolithique. *Bulletin de la Société Préhistorique Française.* 67:199–202.

Bordes, F. 1971. Essai de préhistoire expérimentale: Fabrication d'un épieu de bois. *Mélanges de préhistoire, d'archéocivilisation et d'ethnologie offert à André Varagnac.* Ecole pratique des Hautes Etudes. VIème section, Paris.

Bordes, F. n.d. Bruised blades and flakes in the Upper Perigordian at Corbiac (Dordogne, France). Submitted, 1968, to be included in a volume dedicated to Professor D. Sen, Calcutta.

Chappell, S. 1973. A functional analysis of some Mesolithic scraping tools. Unpublished honours thesis, University of Michigan, Ann Arbor, USA.

Crabtree, D. E. and Davis, E. L. 1968. Experimental manufacture of wooden implements with tools of flaked stone. *Science.* 159:426–8.

Frison, G. C. 1968. A functional analysis of certain chipped stone tools. *American Antiquity.* 33:149–55.

Gould, R. A. 1973. Use-wear on Western Desert aborigine stone tools: a reply to Messrs Hayden and Kamminga. *Newsletter of Lithic Technology.* II:9–13.

Gould, R. A., Koster, D. A. and Sontz, A. H. L. 1971. The lithic assemblage of the Western Desert aborigines of Australia. *American Antiquity.* 36:149–69.

Gramsch, B. 1966. Abnutzungsspuren an mesolitischen Kern- und Scheibenbeilen. *Ausgrabungen und Funde.* 11:109–14.

Gramsch, B. 1973. Gebrauchssporen an alt- und mittelpaläolithischen Silexartefakten Mitteleuropas. *Actes du VIII^e Congrès International des Sciences Préhistoriques et Protohistoriques.* II:137–40. Beograd.

Hayden, B. and Kamminga, J. 1973. Gould, Koster and Sontz on 'Microwear': A critical review. *Newsletter of Lithic Technology.* II:3–8.

Hester, T. R., Gilbow, D. and Albee, A. D. 1973. A functional analysis of 'Clear Fork' artifacts from the Rio Grande Plain, Texas. *American Antiquity*. 38:90–6.

Hester, T. R. and Heizer, R. F. 1972. Problems in the functional interpretation of artifacts: scraper planes from Mitla and Yagul, Oaxaca. *Contributions of the University of California Archaeological Research Facility*. 14:107–23.

Hester, T. R. and Heizer, R. F. 1973. Arrow points or knives? Comments on the proposed function of 'Stockton Points'. *American Antiquity*. 38:220–1.

Kantman, S. 1969. Experimental import or observation and the significance of functionalism in Paleolithic typology – an essay. *Quaternaria*. 11:263–73.

Kantman, S. 1970a. Esquisse d'un procédé analytique pour l'étude macrographique des 'encoches'. *Quaternaria*. 13:269–80.

Kantman, S. 1970b. Essai d'une méthode d'étude des 'denticulées' moustériens par discrimination des variables morpho-fonctionelles. *Quaternaria*. 13:281–94.

Kantman, S. 1970c. 'Raclettes moustériennes': une étude expérimentale sur la distinction de retouche intentionnelle et les modifications du tranchant par utilisation. *Quaternaria*. 13:295–304.

Kantman, S. 1971. Essai sur le problème de la retouche d'utilisation dans l'étude du matériau lithique: prémiers résultats. *Bulletin de la Société Préhistorique Française*. 68:200–4.

Keeley, L. H. 1974a. The methodology of microwear analysis: a comment on Nance. *American Antiquity*. 39:126–8.

Keeley, L. H. 1974b. Technique and methodology in microwear studies: a critical review. *World Archaeology*. 5:323–36.

Keller, C. M 1966. The development of edge damage patterns on stone tools. *Man*. 1:501–11.

Lenoir, M. 1971. Traces d'utilisation observées sur un nucléus à lamelles. *Bulletin de la Société Préhistorique Française*. 68:69–70.

MacDonald, G. F. and Sanger, D. 1968. Some aspects of microscope analysis and photomicrography of lithic artifacts. *American Antiquity*. 33:237–40.

Massaud, J. 1972. Observations sur l'utilisation des burins multifacettés. *Bulletin de la Société Préhistorique Française*. 69:231–4.

Mirambell, S. L. 1964. Estudio microfotografico de artefactos liticos. *Instituto Nacional de Antropologia e Historia*. Departemento de Prehistoria, 14.

Nance, J. D. 1971. Functional interpretations from microscopic analysis. *American Antiquity*. 36:361–6.

Odell, G. H. n.d.-a. Préliminaires d'une analyse fonctionnelle des pointes microlithiques de Bergumermeer, Pays-Bas. (In press.)

Odell, G. H. n.d.-b. A statistical analysis of the chipped stone component of a sample from the Starčevo Habitation Site of Banja, Serbia, Yugoslavia. *Divostin* (eds D. Srejovič and A. McPherson). Belgrade. (In press.)

Pradel, L. 1971. A new classification of burins. *Current Anthropology*. 12:562–3.

Pradel, L. 1972–3. Nomenclature et possibilités fonctionnelles de l'outillage en pierre du Paléolithique en France. *Quartär*. 23–4:37–51.

Rosenfeld, A. 1971. The examination of use marks on some Magdalenian end scrapers. *British Museum Quarterly*. XXXV:176–82.

Semenov, S. A. 1964. *Prehistoric Technology*. Translation into English by M. W. Thompson. Bath: Adams & Dart.

Semenov, S. A. 1970. The form and functions of the oldest tools. *Quartär*. 21:1–20.

Semenov, S. A. 1973. Fonctionologie du Paléolithique. *Actes du VIIIème Congrès International des Sciences Préhistoriques et Protohistoriques*. II:108–13. Beograd.

Sheets, P. D. 1973. Edge abrasion during biface manufacture. *American Antiquity*. 38:215–18.

Sonnenfeld, J. 1962. Interpreting the function of primitive implements. *American Antiquity*. 28:56–65.

Timoshenko, S. P. 1956. *Strength of Materials*. Part II: Advanced theory and problems. Princeton, N.J.: D. Van Nostrand, Co., Inc. Third edition.

Tringham, R. E. 1972. The function, technology and typology of the chipped stone industry at Bylany, Czechoslovakia. *Die Aktuelle Fragen der Bandkeramik*. pp. 143–8 (ed. J. Fitz). Szekesfehervar.

Tringham, R. E., Cooper, G., Odell, G., Voytek, B. and Whitman, A. 1974. Experimentation in the formation of edge damage: a new approach to lithic analysis. *Journal of Field Archaeology*. I:171–96.

Verheyleweghan, J. 1951. La lamelle magdalénienne à dos rabattu et son utilisation. *Bulletin de la Société Préhistorique Française*. 48:354–64.

Warren, S. H. 1923. Sub-soil pressure flaking. *Proceedings Geologists' Association*. 34:153–75.

White, J. P. 1967. Ethno-archaeology in New Guinea: two examples. *Mankind*. 6:409–14.

White, J. P. 1968. Ston Naip Bilong Tumbuna: the living stone age in New Guinea. *La Préhistoire: Problèmes et Tendances*. pp. 511–16. Paris.

White, J. P. 1969. Typologies for some prehistoric flaked stone artefacts of the Australian New Guinea Highlands. *Archaeology and Physical Anthropology in Oceania*. 4:18–46.

Witthoft, J. 1967. Glazed polish on flint tools. *American Antiquity*. 32:383–8.

Abstract

Odell, G. H.

Micro-wear in perspective: a sympathetic response to Lawrence H. Keeley

The field of micro-wear analysis of stone tools has recently witnessed a proliferation of ideas and data. The emergence of certain tendencies and directions makes it both possible and desirable to assess the field at this point. Three major topics are discussed in this paper: (1) the role of experimentation, and a suggested outline for the publishing of the results of experimentation; (2) the forms which utilization traces can assume, assessments concerning the use of each as an index in micro-wear analysis, and some factors which contribute to their formation; and (3) the goals of functional analysis.