Technique and methodology in microwear studies: a critical review

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Introduction

Especially since the publication of the English translation of S. A. Semenov's *Prehistoric Technology* in 1964, there has been considerable interest in the possibilities of directly inferring tool function from the microscopic traces left by use on ancient implements. Since 1964 there have been a number of attempts (mostly in the English-speaking countries and France) to apply this method (microwear analysis) to a variety of archaeological material so that now, after almost a decade since the manifest diffusion of the technique, seems an opportune time critically to review these efforts and, indeed, the whole enterprise of microwear analysis.

For reasons of time and space, the discussion has been limited solely to microwear studies of *utilization* on *stone* tools, since this is the principal interest for most archaeologists. This review attempts to encompass every major report so far published in the principal Western journals (as well as a number of minor reports), and the conclusions that are offered should have some application to others which it has not been possible to mention or discuss in detail.

The general critical approach taken in this paper is based ultimately on the assumption that the goal and purpose of microwear studies is to reconstruct, as completely as possible, the primary economic activities of prehistoric groups. This assumption implies two general conditions of a good microwear study: first, it must attempt to obtain *precise* designations of function for the implements examined, and secondly it must attempt to obtain as complete a picture as possible of the total uses represented on implements from the archaeological unit (i.e. site, site level, living floor etc.) under investigation. With these rather generalized goals in mind, this review will be almost exclusively concerned with technique and methodology. 'Technique' includes all the operations and equipment involved in the initial simple *observation* of the microscopic wear traces themselves – the details of microscopy, implement preparation, microphotography etc. 'Methodology' embraces a wider realm which encompasses all the operations relevant to the *interpretation* of the microwear traces once they have been observed.

Technique

The very cornerstone of any good microwear study is, of course, the actual techniques used in the microscopic examination itself. Unfortunately, the general impression that

one receives from most recent reports on microwear studies is one of degeneration from the high standards of Semenov. Some authors fail to mention technique at all (Witthoft 1967; Massaud 1972), while many only mention in some superficial fashion that they have carried out a 'microscopic examination' (Bordes 1970; Frison 1968; Hammatt 1970; Gorman 1971; Pradel 1973), as though that is all there is to the microwear technique. Semenov (1964) outlines the major components of his technique, which he re-emphasizes in a more recent publication (1970). These are:

1 the use of stereoscopic microscopy (which is often translated as 'binocular microscopy') up to 180 ×, and *also* the use of higher magnifications (300 × +) for more detailed examination;
2 the use of photographs for recording observation, especially stereo pairs,
3 the necessary use of colorizers or metalization on the surfaces to be examined.

All of these recommendations, the accumulated knowledge of over twenty-five years of microwear research, have usually been partially or wholly ignored by investigators in the West.

All published work except that from the USSR and eastern Europe has used almost exclusively low magnification, none (where figures are available) more than 75 ×, although a few mention attempting higher magnification work to no avail (Rosenfeld 1971; Hammatt 1970). A few reports published no photographs, although this does not necessarily mean that photographs were not used in the course of the research, only that they did not use them to present their findings (Hammatt 1970; Bordes 1970; Witthoft 1967). But perhaps the most crippling technical omission made by recent microwear investigators is the failure to counteract the translucency of most artefactual materials. Semenov (1964) devotes a comparatively large part of his discussion of techniques to outlining methods of dealing with this characteristic of most siliceous rocks, particularly flint and its relatives and obsidian. The problem is, of course, that these materials absorb light or rather do not reflect enough light to enable the actual microsurface to be observed. Semenov recommends the use of colorizers (like methyl violet), dusting with powders, or vacuum metalization. Failure to counter translucency will obviously adversely affect the amount, the extent and, most importantly, the kinds of microwear that one will be able to observe. Disappointingly, none of the reports referred to in this article mentions or implies that colorizers or metalization were used or even contemplated, with the notable exception of McBurney (1968), who gives a brief preliminary report of a microwear analysis carried out using metalization. All of the above mentioned technical omissions are systematically related, in fact, so that the failure to metalize makes high magnification work very much more difficult, if not impossible, so that the researcher may conclude it is fruitless. Also, microphotography becomes more difficult and will show considerably less detail.

Failure to maintain the high technical standards of Semenov has, in the belief of this author, accounted for the relatively poor results obtained by most recent microwear studies. C. B. M. McBurney (pers. comm.) claims that for an extensive series of end-scrapers (made of flint) from the Epi-Palaeolithic levels at the cave of Ali Tappeh (Iran), using high magnification, metalization and a systematic search procedure, he was able to observe microwear striations on no less than 70% of the pieces examined. One has only

to compare the above result with MacDonald and Sanger's (1968) figures for a sample of 300 end-scrapers from a fluted-point site in Nova Scotia to realize the importance of strict technique. They observed that only 1% of the scrapers showed 'faceting' and striations on their working edges (all of these were of a non-translucent material, siltstone), with a further 38% showing a 'clear use polish but no additional detail' (these were mostly of harder, translucent materials). From this result they conclude that harder materials (6 to 7 Mohs) retain poor evidence of tool use; however, since they used neither high magnification nor metalization, it seems very likely that the evidence was present, but that they were not able to observe it, especially when one considers the 38% showing polish but no 'detail'. It is, however, reasonable to conclude from their results that harder materials retain traces that differ at least in extent (and probably in kind) from those retained by softer materials, providing both were used in an identical manner. Recognition of the importance of technique in determining results is rare in the West; in fact only Kantman, in what was otherwise a macrographic study of Mousterian and Upper Acheulian notched pieces, admits that he was unable to observe microstriations under the microscope,

> . . . soit parce que la durée du travail n'était pas suffisante pour leur formation, soit parce que nos méthodes d'observation et surtout de préparation de l'objet à l'analyse microscopique n'étaient pas adéquates. (1970a: 273).

These technical omissions mean not only that potentially observable traces of wear are missed, but also that potentially adequate and representative samples of implements become unrepresentative and insignificant – that is, the methodological equivalents of Semenov's selected pieces. White and Thomas give evidence of this when they note for New Guinea that

> . . . attempts to study micro-wear on Highlands tools have not so far proved particularly successful, the main difficulty being that of detecting use-wear on a sufficiently large sample. (1972: 283).

This particular problem means that microwear studies still remain subject to the doubts about statistical reliability that have often been expressed concerning Semenov's initial studies.

Because of the relatively little attention paid to technique outside the USSR, it is not surprising that there have been few technical developments with regard to the microscopic analysis of wear traces. However, Brothwell (1969) mentions in his essay assessing the potential uses for the 'Stereoscan' or scanning electron microscope that this instrument might open up 'a new world of high magnification microtopography to the archaeologist' in the study of microwear traces, and indeed this is a suggestion to be heeded. At the very least, it should extend the minimum size range for observable traces and may even lead to the observation of new types of utilization traces as well. In the USSR, much more progress seems to have been made. In a recent article published in English, Professor Semenov (1970: 8–9) tantalizingly mentions, in passing, two new techniques recently developed there. The first of these is called the 'reflex technique', by which it is apparently possible to measure 'the degree of gloss (in precise units) of polished surface caused by work . . .' (p. 9). The second technique mentioned is even more intriguing and is called the 'micrometric method':

> The essential of the micrometric method consists in using the principle of light section presenting the microprofile (i.e. height and depth) of the worn surface of a tool. Comparing the latter with an unworn surface we can calculate accurately (in microns) a microrelief which suffered changes caused by usage. (Semenov 1970: 8).

Without further details, it is difficult to make any assessment of these new techniques other than to say that they sound very exciting (though perhaps slightly garbled in the translation) and that one hopes full technical details of their scope and operation will soon be made available.

In the overview, it seems absolutely essential that much more attention, in the West, must be devoted to refinement of technique, if microwear studies are to live up to their obvious and important potential. Fuller use must be made of existing techniques and the development of new ones accepted as a goal. Clearly, microwear studies must be viewed as an archaeological specialization, in the same manner as radiometric dating or palynology. But, unlike the latter, the discovery and development of microwear techniques cannot be left to workers in other scientific disciplines in the way that radiometric dating was left to physicists and archaeological palynology was an off-shoot of botany, because, for the present at least, microwear studies are not being pursued in other fields. There is also no getting around the fact that microwear studies are painstaking and time-consuming, and demand tedious attention to technique – to shrug this off or to use technical short-cuts purely in the interests of time will in the long run prove disastrous.

Methodology

The aspect of microwear studies that has attracted the most critical attention has been the methodology involved in inferring use from the observed traces on artefacts. Most of these criticisms have centred around the questions of representative samples and adequate controls (see Thompson 1964; Bordes 1969; Bordaz 1971). These criticisms have been well echoed but not, it seems, very often heeded.

The most progress has been made over the question of representative samples. Since Semenov's results were viewed generally with some reservation because the tools he examined 'were selected for microanalysis by fairly obvious signs of wear' (Thompson 1964: xii), most subsequent investigators have analysed relatively large samples (if not the whole collection) from single levels of site components; these include Wilmsen (1968), MacDonald and Sanger (1968), Frison (1968), Nance (1971) and, in more macrographic approaches, Kantman (1970a, b, c) and Lenoir (1970). However, selected pieces are still being examined and interpreted as in Massaud's (1972) observations on three polygonal burins, or Lenoir's remarks on a single 'utilized' Magdalenian bladelet core. Unfortunately, as pointed out in the preceding section of this paper, inadequacies in technique can often reduce a large, potentially representative sample to the methodological equivalent of selected pieces.

By far the most serious and nagging of doubts about the validity of microwear interpretations arises from the general inadequacy of the controls (if any at all) evidenced in microwear reports. Controls are a necessary part of the process of establishing that the traces to be interpreted are indeed the result of ultilization and, more importantly, of

attributing such traces to a particular use. It is a matter of controlling against microscopic traces that are *not* the result of utilization and controlling for the factors that affect utilization traces, such as the raw material of the artefacts, the manner of use and the material on which the artefacts were used. If these controls are not introduced, then serious and confusing errors can result.

There are many things that can happen to a stone implement before and after use (if it was used at all) capable of producing traces that can be confused with use wear. Semenov (1964) outlines several natural processes that yield such effects, including natural abrasion through the agencies of wind and water, patination and weathering changes. Also soil movements and frost contortions involving archaeological deposits can cause scratches and indeed 'retouch' on stone implements contained in them (Warren 1914; Bordes 1969). These sources of confusion can only be controlled for by examining samples from deposits that are unlikely to have been subject to any form of natural abrasion or soil movement. When this is not possible, as was the case in Lenoir's (1970) study of the traces of use on implements from a Magdalenian surface find, we must follow Lenoir's example and view the results with some caution.

Again, there are many effects resulting from human agency but that are not *per se* the result of utilization. These include, most particularly, technological effects – that is, traces left by the processes of manufacture that may be confused with traces of use. The clearest examples of such traces would of course be found on ground stone tools, but similar effects on a smaller scale may result from various types of platform preparation or even the simple friction of a hammerstone against an implement as a flake is detached. These traces are more difficult to separate by simple observation from utilization traces than are natural traces, for example, since they are the result of purposeful action and tend to be non-randomly distributed. Other effects created through human agency, but not as a result of use, can originate in the treatment an implement may receive after it is abandoned on a habitation floor. Implements can be trodden upon, swept about and treated to all manner of indignities, all of which can be expected to leave traces of various sorts. However, microanalysts can take heart from the expectations that these traces, like those from natural agencies, will usually show a random pattern in form and distribution on pieces, and that in the dark days before the platform sole, humans, even with well-calloused feet, probably treated sharp pieces of flint with a healthy respect.

All of the possible sources of confusion mentioned above should be controlled for, or at least recognized and taken into account. The tendency of most reports is to begin with the problem of utilization without any attempt to establish that the traces being observed are indeed only the result of utilization. A classic example of how much such an oversight can affect the validity of results is Nance's (1971) interpretation of serrated obsidian projectile points from Central California as sawing and whittling implements. He bases this interpretation on 'wear striations', but the present author has suggested that these have a good chance of being the result of the 'scrubbing' of the edges of these implements to carry out the essential preparation of microplatforms for the removal of the fine pressure flakes (Keeley 1973). Obviously then there is a genuine need for controls for eliminating 'non-utilization traces' from such analyses.

Controls for traces arising through agencies other than use can be established in

several ways. With traces created by natural processes, as mentioned above, careful consideration of the stratigraphic and sedimentary context of the pieces to be examined is a necessity and, in most cases, may be all that is needed. But comparison of the study pieces with pieces that are known to have been subject to the relevant natural processes, in the manner of Semenov (1964), allows for more certainty. As for technological effects, the most obvious and fruitful method is to construct a series of experimental pieces that are copies (technically as well as morphologically) of the study pieces, and to examine these copies, prior to any experimental use, in order to gain knowledge of any technological traces that might provide a source of confusion. Ethnographic pieces may be used in place of the experimental copies, but only if it is certain that they have not been used or subject to 'casual' damage since manufacture. In general, it seems that experimental controls are superior to ethnographic controls, since the circumstances of the collection of ethnographic pieces are seldom recorded in the detail necessary for the microanalysts's purposes.

The record of control for these variables in recent reports is scarcely impressive. The only clear instance of the use of experimental controls against 'technological wear' is Kantman's (1970a, b, c) attempt, by the construction of copies, to distinguish between intentional retouches and utilization damage on Mousterian notches, denticulates and raclettes. Unfortunately (for our purposes), his study is essentially macrographic, and the only utilizations he tests are sawing and scraping on wood and bone. All Western authors who mention using experimental copies (Sonnenfield 1962; Witthoft 1967; McBurney 1968; Bordes 1970; Lenoir 1971; Hester *et al.* 1973) appear to have examined them only after they had been experimentally used and not before. Even Professor Bordes, who has clearly devoted a lot of time to thinking up possible sources of serious error in microwear studies (see Bordes 1969 for example), gives no evidence that he is very interested in controlling for these sources of error in his own analyses (1970). Especially on the question of technological traces, much more effort is needed in the devising and implementation of controls to aid in eliminating them from pure microwear studies, preferably by means of experimental pieces.

Once the traces observed have been sorted out and the analyst has decided which of them are most likely due to utilization, then the problem is to determine how the artefacts were used, and on what material. Particularly if striations are present, determination of the *method* of use is not very difficult, using the guidelines of Semenov's (1964: 16–21) 'kinematic' approach. If, for example, a piece is examined and shows striations running closely parallel to the cutting edge on both the dorsal and bulbar aspects, then it is fairly reasonable to designate this implement as a 'saw'. But the archaeologist would be most interested in what this implement was used to saw – bone, wood, frozen meat etc. To restrict oneself to the kinematic approach leads only to a refinement in typology and not necessarily to any better information about ancient economics. Simple examinations of this kind, then, cannot offer any reliable information about the material on which the 'saws', 'knives', and 'scrapers' were used. At this point, extensive, serious experimental and/or ethnographic study becomes necessary if that information is to be recovered.

Most workers in the field have followed a broadly similar procedure when they faced the issue of determining the material on which certain implements were used, which I

shall refer to as 'direct verification'. Stated simply, it first involves guessing the material on which the implement was used (sometimes the method of use is also a matter of hypothesis, but usually this occurs when the wear patterns show no striations). Then either an experimental copy is made, used on the hypothesized material and examined, or ethnographic pieces that are known to have been used on the material in question are examined for comparison. The results of the experimental or ethnographic comparisons are most often held to demonstrate that traces such as those observed on the archaeological specimen(s) could indeed be produced by use on the hypothesized material. Essentially, this is the method used by Semenov (1964; 1971), McBurney (1968), Wilmsen (1968), Bordes (1970), and Gould *et al.* (1971). Some authors used neither experiments nor ethnographic comparisons but nevertheless made functional interpretations, such as Hammatt (1970), Nance (1971) and Massaud (1972), while others who also did not make any experimental or ethnographic comparisons more wisely refrained from attempting functional interpretations (Frison 1968; MacDonald and Sanger 1968; Lenoir 1970; Rosenfeld 1971). While the weakness of interpretations made without experiments or ethnographic comparisons is apparent, the weakness of interpretations based on direct verification is more subtle.

While it is a necessary and indeed minimal step to establish that one particular usage is capable of producing microwear traces identical with those observed on the archaeological specimens, this does not guarantee that another method of use or the same method of use on another material cannot produce a similar or identical pattern of wear traces. In other words, other methods of use and other worked materials are left uncontrolled. For example, Kantman's (1971) experiments indicate, on a macrographic level at least, that it is extremely difficult to distinguish between the wear arising from sawing or from scraping of bone on the one hand or wood on the other, but to an archaeologist it might be crucial to know which of these two different materials was being processed. To avoid errors arising from the convergence of wear patterns of diverse origins, each hypothesis about utilization should be considered against a *framework* of experiments and/or ethnographic comparisons, in order to enable the investigator to say that certain implements have been used in a particular manner on a particular material, not merely because direct verification proved positive, but also because many other experiments or ethnographic comparisons have shown that no other use in any other manner or on any other material is capable of producing similar wear patterns.

Evidence of attempts to use such an experimental framework are rare in the literature but not absent. While Semenov may use direct verification, he has worked upon such a variety of problems that, in a sense, he and his colleagues have built up an informal framework of experiments. One of the only formal attempts to develop such a framework is Sonnenfield's (1962) early and interesting pilot article on a study of the functions of stone 'celts' from the mid-western United States. While his article deals only with the preliminaries to the study and one part of the experimental work envisaged, it is apparent that there was to be more to this study than simple direct verification of the principal hypothesis – that these celts were used as wood axes. In fact, the only experiments he describes in this article are those having to do with the use of stone implements as hoes, which were undertaken as part of a series of experiments encompassing most of

the possible uses of such celts. Such a procedure not only allows a more certain assignment of function to artefacts, but also allows the sorting of any particular artefact class or type into several functional sub-classes, where such a situation exists. Another attempt at something like an experimental framework was Kantman's (1970a, b, c; 1971) construction of a series of experiments, although limited in scope, for use as a basis for a study of certain classes of Mousterian tools. The use of an experimental framework (or a framework of ethnographic comparisons where this is possible) is especially important when the microwear study encompasses a whole collection, or a large sample from one, where a variety of uses on many different materials is likely to be represented.

It is also very important that such an experimental framework be relevant to the ecological situation and other general conditions of the site or sites from which the study materials originate, and to the raw materials utilized in the implements themselves. For example, if only softwoods were likely to be available to the archaeological inhabitants of a site, then it is only reasonable that the control experiments in woodworking be conducted principally on the relevant softwood types. And if the climate of the time was particularly cold, then some experiments in the butchering of frozen meat might be in order. As noted in the preceding section of this paper, the nature of the stone out of which artefacts are made affects the extent and kinds of wear traces developed, so it is imperative that the control experiments be conducted with copies constructed of the same rock type. Another *caveat* to be considered is that prehistoric men, their food sources and their immediate environments were all a good deal grittier than present-day archaeological laboratories and their inhabitants, so that some effort may be necessary to approximate these earlier, dirtier conditions in the course of the experimental work. The traces left on an implement by the well-scrubbed hands of a laboratory technician may bear little comparison with the traces left by the hands of our less hygenic ancestors. And the working of the hides of laboratory or even domestic animals cannot be counted on to produce traces fully comparable to those left by the working of the hides of less well-groomed wild animals. Without some close attention to details such as these, misleading results could be obtained even from an otherwise admirable series of experiments or comparisons.

One case illustrating the problems that can be created by carelessness about relevance is Gould, Koster and Sontz's (1971) attempt to identify certain scrapers of the Quina variant of the European Mousterian as woodworking tools, on the basis of a careful ethnographic study of Australian Aboriginal woodworking. They discovered, from a microscopic examination of Aboriginal chert and quartzite adzes, which they had observed (first hand) being used to work Mulga wood, that this use caused 'an irregular series of small terminated flakes along the bulbar face of the working edge' (*op. cit:* 166). They further confirmed this by a laboratory experiment using an adze copy (in chert) to work Mulga wood. It might be thought that 'small terminated flakes' constitute a rather ambiguous wear trace that could arise from many kinds of use but, nevertheless, these investigators then went on to apply their results to Mousterian Quina-type scrapers (made of flint). When they found on these Quina scrapers small flake scars similar to those they had observed on the Australian material, they proposed that these implements might be woodworking tools. This hypothesis was somewhat upset by S. Binford's

observation (cited in the article) that, because of the cold climate of France during much of the Mousterian period, hardwoods and trees in general were probably quite scarce. The investigators were then left to propose that the Quina scrapers might possibly be bone-working implements. However, when we consider that there is extremely little surviving worked bone from the West European Mousterian, except perhaps in its final stages, while scrapers of the Quina type are relatively common, Gould, Koster and Sontz's results are rather left dangling. But, given the ambiguous nature of the wear trace on which their exercise was based, the looseness of their approach to both artefact raw materials and local ecological situations, the inconclusiveness of their otherwise commendable attempt is not exactly unexpected.

Finally, with the warnings about experimental relevance still in mind, the rather uncritical application of Semenov's observations on Russian material to archaeological implements from other areas, made from different raw materials, without a background of local ethnographic comparisons, seems to be a dubious practice – too many crucial variables are left uncontrolled.

One methodological problem that microwear analysts have not dealt with very effectively is quantification. While MacDonald and Sanger (1968) have noted the difficulty of quantifying microwear observations, it seems that some of the information obtained in microwear analysis could be presented in a more rigorous fashion. Semenov's (1970) accounts of new Soviet techniques indicate that even direct metrication of some aspects of microwear observation may be possible. Even without such technical refinements, considerable improvement could be achieved by the simple quantification of qualitative observations. For example, it should be possible to record how many implements show wear traces disposed on only one lateral edge and how many on both edges, or how often traces occur on both surfaces of the working edge and how often on only one, or how many implements (or edges) show only striations parallel to the working edge and how many only perpendicular striations or how many both, etc. A good deal can be accomplished in this manner and, with more rigorous formulations of the data, statistical manipulation can be employed, which would ease some of the strain of submitting large collections to microwear analysis.

Another methodological omission almost universal in microwear reports is the failure to include supplementary, independent data that is useful in evaluating the results of the microwear analysis itself. For example, a detailed discussion of the archaeological context of the study implements and their *in situ* association with other artefact types or other remains would often be extremely useful and interesting. Even more important would be a discussion of the occurrence or absence of certain 'functional types' (as defined by microwear analysis) in tool 'factors' of the sort derived by Binford and others via factor analysis. For example, it would be interesting to see if implements designated as meat knives after microwear analysis are components of the tool factor assumed to represent a butchery tool kit. Not only is this sort of supplementary information useful for validation, but it allows as well the investigation of the meaning of the contexts, associations and factors themselves.

It seems appropriate to state the conclusions to this section in the form of a series of recommendations that should be carefully considered if microwear studies are to be put on a sound methodological footing.

1 The trend toward using larger samples or whole collections in analyses should continue, and hopefully closer attention to technique will render this strategy more fruitful.
2 Better controls should be set up to help the analyst distinguish genuine utilization traces from 'natural', 'casual' or 'technological' effects.
3 The interpretation of microwear traces should proceed through the use of an experimental or ethnographic framework against which any hypothesis about utilization can be tested.
4 Such an experimental framework should be relevant to the natural situation of the site or sites under study, and to the raw materials used in the construction of the artefacts, and to any other local factors.
5 There should be more serious attempts to quantify microwear data.
6 Supplementary data of the sort useful for the independent validation or assessment of microwear interpretations should be included in all microwear reports.

These suggestions are not merely technical devices capable of extending the analyst's power to obtain actual results; if they became basic to the approach of all workers in the field, they could hardly fail to ensure that more certainty would attach to any conclusions a microwear study might bring forward.

New directions from recent research: a few examples

Considering the serious difficulties with technique and methodology that beset most recent microwear studies, the tentative nature of most of their results makes a discussion of their accomplishments difficult. However, there are a few studies that merit special mention either because they indicate possibly productive approaches to deeper knowledge of prehistory via microwear analysis, or because they have contributed, despite limitations, to our knowledge of ancient economic activities. Several reports would be worth mentioning for one reason or another, but limitations of space only permit three to be selected for comment here.

One study that contains some thought-provoking material is Wilmsen's (1968) study of implements from several Paleo-Indian sites in the USA. While this study is praiseworthy on several counts (use of large samples from several sites; consideration of association between edge-angle and wear traces; use of ethnographic comparisons), it was one particular observation, made rather in passing by the author, that seemed to the present writer to present some of the most intriguing possibilities. In his analysis of the endscrapers in his samples, Wilmsen notes some strong similarities between the wear traces found on some of the Paleo-Indian scrapers and the traces observed by Semenov on some endscrapers from the Russian Upper Paleolithic site of Timonovka. Wilmsen makes no more of this similarity than to add that, 'These striations may have been produced during the scraping of the hair from the hides or in working a clay- or grit-mixed curing agent' (1968: 159). It might, however, be felt that this observation has important implications for the study of Paleo-Indian origins, and points to a possible fruitful use of microwear observations in the reconstruction of culture history. Any

attempt to trace the Paleo-Indian tradition to its presumed roots in Asia, based primarily on resemblances in tool morphology and lithic technology, is likely to run into some daunting difficulties, if the morphology and the technology of the implements varied with the availability and quality of the local stone as much as they commonly do elsewhere in the world. But the 'Siberian Connection' might still be demonstrable through resemblances at a very detailed level in certain economic activities, such as the use of a clay-mixed hide curing agent. Economic activities of this sort can be reflected in wear traces left on implements that need show no apparent morphological similarities. Obviously, the resemblances noted by Wilmsen do not at this stage constitute actual evidence for such a link, but the possibility of making such 'genetic' connections is not without interest.

Another study worth mention is Frison's (1968) study of retouch flakes from a Late Prehistoric site in Wyoming. Although Frison did not make any specific functional interpretations from his microwear analysis, his report is of interest because he noted that many repeatedly retouched implements may come to the archaeologist in a non-functional condition (that is, rendered unusable by a final resharpening) and with this in mind, he submitted small retouch flakes recovered in his excavation to microscopic examination. Not only does this procedure increase the chances for observing wear traces, but it also points to a potentially productive research area in the study of the relations between tool morphology and utilization. Needless to say, the successful operation of this procedure depends directly on the care taken in excavation.

Finally, Witthoft's (1967) study of 'corn gloss', although marred in the presentation by an absence of photographs and an inadequate discussion of technique, is probably one of the most successful of recent microwear studies. Possibly some of this success may be due to the fact that the study of corn or sickle gloss is a comparatively old and well defined problem. Nevertheless, this report is significant for several reasons. It uses a number of experiments and ethnographic comparisons to attack a specific problem; it attempts to establish a one-to-one relationship between a microwear type and use on a specific material (i.e. corn gloss and grass); perhaps most importantly, it provides a list of diagnostic traits for corn gloss; and finally it provides a detailed explanation for the association between the wear trace and the worked material. Witthoft's report would perhaps have been accorded more recognition if it had dealt with a less well known phenomenon among utilization traces instead of a classic one, but it still remains one of the most complete microwear reports published so far in the West and undoubtedly owes its productiveness to this completeness.

Conclusions

The intention of this review, however critical it may seem in places, is neither to create despair among those who have placed great hopes on microwear analysis, nor to provide comfort to those who would prefer their views on archaeological systematics and artefact typology not to be threatened by any clearer direct evidence for prehistoric economic activity and tool use. What is intended, rather, was the demonstration that the *correctible* failings of poor technique and inadequate methodology have adversely affected the

productivity and validity of many recent microwear studies, and that if these faults are put right, better quality and more abundant information should be obtainable. The impression one often receives reading the reports cited in this article is that some of the investigators have interpreted their personal difficulties in using microwear analysis as representing the true limits of the method itself – an unnecessarily dreary picture, since most of their problems are assignable to the various common inadequacies we have been discussing. There are undoubtedly some disappointments in store for microwear analysts, since there are probably some questions about tool use that microwear analysis will never be able to answer, but these true limits will not be reached through the use of haphazard techniques and methodologies.

Another point that soon becomes apparent is that microwear analysis is not for the dilettante. The techniques of examination are time consuming and demand attention to technical details, and the methodology behind any good microwear study must be specially constructed and carefully implemented. These considerations argue for microwear analysis being regarded as a specialist activity to be undertaken, hopefully, in special laboratories.

Finally, there are many who believe that *the* primal microwear study is yet to be done, a study which would serve as the foundation for subsequent efforts. While this assessment is essentially correct, it should be modified. There is not just *one* great study to be done but very many, each related to the specific problems and conditions of various areas and archaeological periods. The sooner these studies begin, the sooner microwear analysis will realize its potential.

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Abstract

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Technique and methodology in microwear studies: a critical review

This review seeks critically to examine recent attempts to apply microwear analysis in the study of the uses of stone implements. It deals particularly with the techniques of microscopic examination that were employed and with the methodologies utilized in attempts to infer function from microwear observations. In both of these areas serious but rectifiable omissions were found to be common. Recommendations are offered for correcting these faults and for increasing the productivity, reliability and validity of microwear analysis.