OVERVIEW OF GREAT BASIN OBSIDIAN STUDIES

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The papers in this volume represent a considerable advance in both the amount of interest and the sophistication of the reports on obsidian studies. I recollect a national meeting only about 15 years ago at which a symposium on this subject was able to attract only five or six people -- the speakers were there but the audience was not. In contrast, the symposium reported here had a large and faithful audience which sat through an entire day of papers relating to obsidian studies. Archaeologists have clearly recognized by now the significance and the multiple applications of these studies to archaeological problems. Particularly in the Great Basin, there is greater appreciation of what these studies can do for archaeological understanding, since the Great Basin is characterized by many shallow (often surface only) archaeological sites, and often the commonest cultural material is obsidian artifacts and chipping waste, due to the multiplicity of obsidian sources in and around the Basin. With so many sites lacking samples datable by radiocarbon, and lacking physical stratigraphy, obsidian dating is often the best source of information on the age of the site, and obsidian sourcing is often the best source of information on trade relationships. Hence the relatively greater interest in obsidian studies from those who work in the Great Basin.

There are four papers here dealing with the important issue of determining the source from which obsidian came. In spite of the increasing attention to this problem, there are still numerous practical and intellectual matters to resolve. Hughes points out the difficulties in determining the sources of obsidian (through chemical analyses of varying kinds), and cautions that incorrect assignment to source may go undetected and unchallenged, leading to misinterpretation of trade routes. This may also lead to confusion in applying obsidian hydration dating, because it is becoming increasingly clear that different obsidian sources show formation of hydration bands at quite different rates.

Hampel also stresses the need to adhere to specific basic procedures in x-ray fluorescence (XRF) studies of obsidian sources, while pointing to refinements of the method. Nelson applies XRF methods and identifies the chemistry of many obsidian sources in the Great Basin. This is particularly valuable since most of the earlier source identifications were made on obsidian from California, at one edge of the Great Basin. Comparative data for Nevada, Oregon, Utah, and Idaho greatly expand knowledge of Great Basin sources and provide a much firmer basis for future studies of contact and obsidian trade.

Nelson observes that prehistoric people usually got their obsidian from the closest available source but that there are numerous instances where this practice was not followed -- hence the basis for study, and hopefully explanation, of long-distance trade in the past. That long-distance trade was very important can be seen in the great amounts of obsidian recovered in areas where there are few or no sources of this material, such as coastal southern California.

The possibility of avoiding the cost and difficulty of laboratory determinations of obsidian sources is considered by Bettinger, Delacorte, and Jackson, who have sorted obsidian on the basis of visual characteristics with reasonable success. This procedure is not without some error, but neither are XRF or other procedures. Obviously an in-depth study requires some chemical analyses to identify source, but once the sources are known the visual characteristics of the obsidian can be carefully tabulated and observed and it then becomes possible to recognize pieces from that source without a full chemical analysis of each piece. This is important because there is no possibility that chemical analyses can be performed on the many thousands of individual specimens that can be collected from even a single site. A judicious combination of chemistry and observation of known traits is probably the only practical way of dealing with large collections.

It may be noted that other "short-cut" methods are applicable as well. Some obsidian sources have aberrantly high or low amounts of specific trace elements, and it will be possible to get a pretty good idea of the source by testing merely for those elements. This requires more thorough knowledge of the chemistry of all the sources than is now available, but as the data base improves it will certainly become possible to identify some obsidian by looking for distinctive marker elements without the need for more detailed analysis. It has also been pointed out by Jonathon Ericson that in some cases the hydration rate for specific obsidian sources is very distinctive (exceptionally fast or exceptionally slow) and that in some archaeological contexts it is possible to know the source by knowing the applicable hydration rate.

All of these short-cut methods involve extrapolation from knowing part of the data to drawing conclusions about other parts of the data, a common-sense procedure which often works but is based on assumptions. All of us making use of such methods are obligated to make clear in our reports what statements are verified objectively and what statements are in fact assumptions or extrapolations from partial data.

These various studies of obsidian sources are aided by the apparent tendency of obsidian users to collect their obsidian from small areas, returning to the same spots (possible to the same ledge or outcrop) over many visits. This behavioral pattern tended to collect and distribute more uniform kinds of obsidian than would be the case if obsidian were gathered at random from every possible location on a given obsidian flow. In other words, the total chemical variability of an obsidian flow does not seem to be strongly reflected in the observed obsidian found in archaeological contexts. This fortunate circumstance of patterned collecting activity aids both chemical and visual identifications of source.

Three papers deal with obsidian hydration analysis. R.J. Jackson points out the methodological problems of hydration studies and makes the same plea for standardized and replicable procedures voiced by the sourcing specialists with respect to their methodology. Jackson provides interesting comparisons between obsidian hydration readings obtained on the same specimens by different laboratories. At least two other studies of this kind are in press or underway, and they provide objective evidence on replicability, margins of uncertainty, and other problems with making the slides and measuring the obsidian hydration bands. Cross-checking between laboratories becomes increasingly important as the number of technicians increases and differing optical and slide-preparation techniques are used. Some of the observed variability between laboratories may be due to measuring the obsidian hydration band at different locations, but whatever the cause we should all have a little humility about the accuracy of our hydration measurements.

I would like to add to Jackson's observations of variability my own plea for a) systematic publication of obsidian hydration readings, and b) systematic cataloging and preservation of the published slides. It is important to know who the technician was, how the work was done, and where the original slide can be obtained for checking and verification of the reading. It is equally important that the original data be made available; it is impossible to know what sites have been studied for obsidian hydration since the majority of the data are sequestered and are either never made public, or appear as an appendix to a site report many years after the readings were taken. It is my impression that the researchers on obsidian sources have done a much better job of publishing their basic data and making it available for comparative study. Only two of the existing obsidian hydration laboratories make systematic efforts to publish their readings, and only UCLA publishes <u>all</u> of its obsidian hydration readings and makes <u>all</u> of its slides available to scholars for re-examination when needed.

The two other articles in this section of the volume (Davis; Termbour and Friedman) deal with the important matter of the temperature variable in the rate of obsidian hydration. Davis presents a most ingenious way of determining effective hydration temperature in the past. Trembour and Friedman stress the importance of temperature in their calculations of hydration rate, with small differences in temperature having a large effect on the rate. They also stress the necessity of sorting out surface from buried pieces and the difficulties arising from obsidian exposed for even a short time to high temperatures (such as a cooking fire or brush fire). This is a formidable set of difficulties in archaeological collections. Not only does the temperature vary seasonally, but the period of hydration may span warmer and cooler climatic cycles. Any obsidian lying in chapparal areas has a high probability of exposure to brush-fires every 50 years or so if not more often. Pieces once on the surface get interred by ground squirrels, while the busy rodents are also bringing up buried pieces and depositing them in the surface sunshine. Finding the temperature in order to work out the hydration rate involves a number of unverifiable assumptions and may lead to the same kinds of difficulties encountered with amino-acid dating, in which incorrect assumptions about past temperatures lead to major dating errors. The system can be scientifically correct and internally consistent but still be wrong as to the age of the specimens due to unknown factors.

Because of these problems, working it the other way around -- that is, using hydration to determine effective annual temperature, seems to me a more promising approach. In many cases such findings would be immensely valuable, for the paleotemperature data would be indicative of other environmental variables relevant to the interpretation of archaeological sites.

Fortunately, the archaeologist has additional data which provide a check on both dating and temperature results. For chronology, these data include other chronological methods such as radiocarbon, stratigraphic placement, and "time markers" of known age in the artifact assemblage. Obsidian hydration dates are tested against these empirical data, and when there is serious lack of agreement, re-evaluation of the results is required. With regard to conclusions about ancient temperatures, many archaeological sites contain an abundance of climatic indicators in the form of pollen, floral, and faunal remains. Plant and animal remains reflect the climate and temperature of the time when the archaeological site was in use, so they can provide a separate line of evidence to evaluate paleotemperature calculations.

The final section includes four papers which apply the methodology to varying problems and use the source and hydration findings to move forward to archaeological explanations.

T.L. Jackson assesses aboriginal obsidian production from two eastern California sources, Bodie Hills and Casa Diablo. In a detailed review of the several exploratory studies done on obsidian hydration from these sources, including his own large sample of 800 pieces from Casa Diablo, it is made clear that "production" curves are only as believable as the supporting chronological evidence. For these two sources, understanding of the chronology (obsidian hydration rate) is still elusive, although the serious consideration given by Jackson should get us closer to understanding the hydration rate(s).

Bouey and Basgall look at trade relationships between the Great Basin and California, based on hydration dating and the model of "economic articulation" which proposes that a change in production and/or exchange in one area will have an observable repercussion in the other. As these authors point out, any general explanations applicable to such a large area are tentative because of the many scholars involved in collection of the data, plus differing interpretations of obsidian hydration rates. Nonetheless, an effort can be made to look at political and economic processes which might explain the observed pattern of obsidian distribution (in space and time). They conclude that Central California was a major consumer area, that the obsidian trade was linked, at least initially, to a demand for status goods, and that there was a production peak between 3000-1600 B.P. Other sub-regions are evaluated and some alternative models are also presented, including population replacement in local regions.

In examining the structure of obsidian exchange, Bouey and Basgall are getting a great deal of historical explanation out of trade in a single raw material: obsidian. Their suggestions go far beyond anything dreamed of by most of the diggers of an earlier era, who never anticipated that general conclusions of broad significance could come from their arrowheads and chipping waste. On one hand, it is encouraging to find that the dating and sourcing of obsidian can contribute to new ideas and that archaeologists are able to advance their field of study to new levels of sophistication. On the other hand, there is also the moral that careful field work, documentation, and preservation of the obsidian collections are increasingly essential. Shortcomings in the traditional archaeology will abort formulations of the new archaeology, and the two need to work hand in hand if the discipline is to move ahead intellectually.

The obsidian hydration paper by R.J. Jackson points out the value of using obsidian hydration for relative dating, and reminds us that obsidian hydration studies have important values whether or not they can be used for absolute dating. However, he cannot resist providing another suggested absolute chronology for Casa Diablo obsidian. This is worth brief general comment:

First, the four Casa Diablo hydration rates in Table 2 differ, but not substantially except for readings under 2 or over 10 microns, sizes that are quite rare in known archaeological collections of Casa Diablo obsidian. I would conclude that all of the proposed rates in Table 2 give reasonable age determinations, even though they were worked out in different ways by different people. It is distressing that we cannot support our formulae beyond question, but it should be encouraging that similar answers are determined independently. In the mid-range, for example, the four rates are within a span of 211 years for 3 microns of hydration, with a span of 156 years for 6 mircrons, and within 610 years for 9 microns. This strikes me as amazing consistency, the greatest variability being ± 12-20% of the true age, assuming the true age to be somewhere in the time range expressed by the four rates. This is certainly far better absolute dating than has been available for most Great Basin sites.

Assuming Jackson's rate to be the most valid, the time intervals between each micron of hydration (omitting the small bands) are:

Micron 2-3	520 years
Micron 3-4	613 years
Micron 4-5	689 years
Micron 5-6	759 years
Micron 6-7	822 years
Micron 7-8	879 years
Micron 9-8	934 years
Micron 9-10	984 years

It is extremely rare to have an archaeological context showing 2-10 microns of hydration, and often the range of hydration is only 3 or 4 microns in a given site or archaeological horizon. I have pointed out elsewhere (Meighan 1983) how this narrow range in a given context makes it impossible to verify an empirically-determined rate formule — there is just not enough variability in most archaeological collections to test the formula. Hence almost any formula can be made to yield acceptable answers. Assume, for example, a collection of Casa Diablo obsidian with a range of 7-10 microns of hydration. With a couple of radiocarbon dates, one might well propose a simple linear rate of 700 years per micron (average of Jackson's intervals for 7-10 microns). Such a rate would differ from Jackson's numbers by as little as 19 years and as much as 537 years (the latter in a Jackson age of 6837 years, or 8% of the age). The 700-year rate would not work at all for hydration bands of significantly larger or smaller size, but in our hypothetical collection it would be as close to the truth as any other rate, and it would be impossible to dispute by any archaeological evidence (including radiocarbon dating).

The rough and ready linear rates used for determining ages of particular assemblages are irritating to the purists, lack elegance, and are "wrong" in the laboratory or theoretical sense. But they work with the real data the archaeologist has at his disposal, and they do provide age determinations as close to the truth as we can measure.

When dealing with collections from a wide area and many sites, as Jackson does in this paper, the rough and ready methods can lead to some big mistakes. Jackson's relative dating chart (Figure 3) shows hydration measurements of 1-10 microns; here he avoids absolute dates but does provide the hydration sequence for six named Great Basin cultures. Since most of these cultures are defined primarily on the basis of surface finds, the objective sequencing is no mean accomplishment and provides a good basis for further analysis.

In the final paper, Tuohy applies obsidian dating to 38 "early man" points from Nevada, including examples of Clovis, Elko, and other types associated with earlier cultures. In this complex subject, Tuohy confronts the morass of typological confusion, varying obsidian sources, and related problems which make it difficult to go beyond tentative conclusions.

One situation observed here by Tuohy (and elsewhere by others) poses a considerable problem to users of obsidian hydration data. This is the confusion caused by abrasion or erosion of the surface of obsidian pieces. Exposure of obsidian to desert winds full of sand and silt can sand-blast the surface and alter the obsidian hydration band. This abrasion can sometimes be detected microscopically, and it is no problem if all the hydration layer is removed. But there is good evidence that abrasion can sometimes remove part of the hydration band and leave a fraction of it intact to be measured by the archaeologist. This shows most clearly when the surface of a specimen has a small hydration layer while a much larger layer is visible in cracks protected from surface abrasion. There does not appear to be any simple solution for this problem, but a large enough sample will usually detect anomalous readings and raise the suspicion that something has happened to the surface of the piece being studied.

On the whole, the papers in this volume represent a lot of brain power. In my view, Great Basin archaeology is put forward by these studies, which reflect the ever-present problem of getting more information out of the fragmentary evidence of past activity. It is a contribution to raise an intelligent question, and the authors of these papers have laid out a challenging agenda of relevant issues to be explored. There is, of course, much more to Great Basin archaeology than obsidian studies, but it is remarkable how studies of this ubiquitous raw material can throw light on multiple lines of investigation: chronology, trade and even social organization and social classes. The editor of this volume deserves much credit for organizing and presenting this set of papers; they will be essential references for future workers.

References

Meighan, C.W.
1983 Obsidian dating in California: theory and practice. *American Antiquity* 48(3): 600-609.