NORTHERN CALIFORNIA OBSIDIAN STUDIES:

SOME THOUGHTS AND OBSERVATIONS ON THE FIRST TWO DECADES

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ABSTRACT

Obsidian studies in northern California have been prosecuted in earnest for about two decades. Reconnaissance, collection and geochemical characterization studies laid the foundation for subsequent research devoted principally to chronological (i.e., dating) issues. This paper briefly reviews previous research, addresses the present "state-of-the-art", then concludes with some thoughts on the ways in which future studies may be able to make substantive contributions to anthropological archaeology.

INTRODUCTION

The Society for California Archaeology celebrated the 25th year of its existence in 1991, and a similar "birthday" of sorts might also be proposed for California obsidian studies. It was, in fact, two decades ago that Thomas Jackson (1971) published a modest little paper entitled "Determination of the Source of Artifactual Lithic Material" in the Treganza Anthropology Museum Papers series at San Francisco State College. Although Robert Jack, along with Ian Carmichael, of the Geology and Geophysics Department at the University of California, Berkeley, had demonstrated the applicability of x-ray fluorescence analysis to California archaeological materials some years earlier (Jack and Carmichael 1969; see also Parks and Tieh 1966), Jackson's early work addressed, at least implicitly, several of the themes that would come to take on increasing importance in future archaeologically-oriented obsidian studies. Jackson (1971) recognized the potential importance of separating samples by artifact type (to investigate whether or not specimens of different types might have been made from different obsidian source materials), and he also clearly appreciated the way in which obsidian sourcing data might be employed as a cross-check on the trade relationships described in California ethnographies. Although he did not state it in this way, one can see in this early paper the foundation for the position that obsidian sourcing studies, properly executed, could serve as a powerful means for rectifying incorrect portrayals of prehistoric obsidian trade in California.

BACKGROUND: OBSIDIAN SOURCING STUDIES

Prior to the advent of physiochemical sourcing techniques, anthropologists and archaeologists interested in prehistoric trade/exchange relationships speculated about such activities by appealing variously to ethnographic accounts, distance, and to extant knowledge of geologic sources. As I have discussed elsewhere (Hughes n.d.a), much of the early work in California was grounded in twin assumptions: first, that by employing what would later be called the direct historical approach, the trade and exchange relationships documented in California ethnographies could be reliably projected into the distant past and, second, that archaeological culture change was a slow, gradual process. These assumptions became axioms that legitimized the view that prehistoric trade/exchange relationships were essentially isomorphic with those documented by ethnographers or, if they had changed, such changes had been largely inconsequential. Kroeber, for example, in 1917 wrote that: "...the natives of the San Francisco Bay region traded the same materials <u>from</u> the same localities one or two thousand years ago as when they were discovered at the end of the eighteenth century" (Kroeber 1925:930; my emphasis).

Given this view, conjecture by Lillard et al. (1939:75) that people who were members of their Early culture had "definite regularized trade relations...with the Napa valley area to the northwest which was the main source for obsidian in the Interior Valley region" and Heizer's (1949:34) opinion that Early Horizon obsidian was "apparently from the Napa Valley quarries rather than from those of the Clear Lake region" could be accepted comfortably by most archaeologists not only because of the proximity of these central valley sites to the Napa region, but because of ethnographic information which substantiated such linkages during the historic period (cf. Sample 1950; Davis 1961).

As is now well known, the obsidian sourcing data Tom Jackson (1974) presented in his M.A. thesis showed that Napa Valley obsidians were <u>not</u> the only ones represented in Early Horizon sites and, perhaps of greater importance, his study indicated that a major "shift" had occurred during the Middle Horizon such that by the beginning of the Late Horizon Napa Valley obsidians predominated over Trans-Sierran glasses in lower Sacramento and upper San Joaquin Valley sites. In many ways, this study was a landmark for students of California prehistory in that it not only showed that the historic-prehistoric continuum <u>assumed</u> by earlier workers was at variance with the facts, but it provided a clear indication that really new things could be learned about the past by studying obsidian.

BACKGROUND : OBSIDIAN HYDRATION ANALYSIS

Compared to the enthusiastic acclaim given the results from sourcing studies, the initial results of obsidian hydration studies in the Central Valley definitely met a cool reception. Although obsidian hydration studies on California materials preceded sourcing analyses by nearly a decade, they definitely got off on the wrong foot. Donovan Clark (1964), a Stanford graduate student who had worked with one of the developers of the obsidian hydration measurement technique (Irving Friedman), tested the applicability of the method by attempting to date obsidian artifacts from several of the key archaeological sites in the San Francisco Bay, Delta, and lower Sacramento Valley regions. Because of his intimate familiarity with these collections at the Lowie Museum, James Bennyhoff was asked to select the samples to send to Clark. Long before I developed an interest in obsidian studies, I have a vivid memory of working with Bennyhoff on a project in the Hearst Gym basement at the Lowie Museum. On one particular occasion the two of us reviewed Jim's notes on the samples, the hydration rim readings and calendric date conversions he received back from Clark. Jim, of course, knew full well what the age ranges for each sample should be, based on independently derived C-14 dates and bead types, and I can recall him showing me these data and commenting as he looked at each date - "nonsense", "worthless", "abysmal". I knew right then there was big trouble in hydration land.

But today I would argue that the problems Bennyhoff and others perceived with early obsidian hydration studies were not so much attributable to the technique per se, but to how it was applied and to the unrealistic expectations held by archaeologists. In the early 1960s it was known that chemistry and temperature were the two major variables effecting the rate at which obsidian absorbs water (hydrates), but none of Donovan Clark's samples were geochemically attributed to source nor were the potential impacts of regional temperature variation taken into account. Today it is also widely appreciated that effects of artifact reuse and burning must be identified and controlled. Ironically, many archaeologists rejected the results of hydration analyses because the rim readings obtained did not stack neatly in "textbook stratigraphy", i.e., all artifacts with large rim values clustered at the bottom of the site and samples with small rims largely confined to the upper levels. What many archaeologists bemoaned, of course, was that specimens with small rims occurred "too deep" in a site and samples with large rims were found at and too near the surface. The irony is that the obsidian hydration results were entirely consonant with what one would anticipate in sites subject to the long-term operation of cultural and natural transformation processes. For some reason, archaeologists in those days (and, unfortunately, even today) expected the hydration results to be in "perfect sequence" even though other evidence indicated that the deposits had been transformed by grave pit digging, housepit construction, etc.

RECENT DEVELOPMENTS AND RESEARCH ISSUES

During the early 1980s sourcing studies were aided immeasurably by the microcomputer evolution, and x-ray spectrometers were driven by computers and analyzers capable of generating quantitative estimates of trace element compositions without sacrificing any portion of the sample. This was a real advantage over the previous semi-quantitative wavelength system that Tom Jackson and I first used (e.g., Jackson 1971, 1973, 1974; Hughes 1978) because it provided much finer resolution capabilities and allowed, for the first time, analysts at different laboratories to compare data directly, which could eventually lead to the establishment of a quantitative western North American data base for obsidians.

From a broader North American vantagepoint, California was from the very beginning far ahead of other areas in sourcing research in part because of technological capabilities, but because archaeologists paid much closer attention to obsidian than they did in most other western states. For example, in the early 1940s Heizer and Treganza (1944) published a statewide compilation of known obsidian sources in Mines and Quarries of the Indians of California. By contrast, archaeologists in Oregon have, until quite recently, paid comparatively little attention to obsidian despite the fact that the state contains a vastly greater number of artifact-quality obsidian sources than does California. So today, sourcing studies in Oregon, Nevada, and Idaho are still largely in what we might call the "Reconnaissance Phase", attempting to locate and geochemically analyze artifact-quality glasses in the area. Because California has largely passed through this phase and moved on to tackle more anthropologically-oriented issues, researchers in other western states look to California obsidian studies as models for their own research. California is, and has been for some time, at the forefront of obsidian studies.

Having engaged in a bit of flattery, I would like now to comment briefly on areas particularly deserving of continued critical thought and research. In my opinion, the resolution of these issues will seriously influence the future trajectory of obsidian studies.

There has been a long-standing hope that obsidian hydration will develop into an absolute dating method capable of yielding dates in calendar years. Those laboratory-based specialists most committed to the calendric approach employ formulae derived from source specific laboratory-induced hydration experiments to convert hydration rim values to absolute years. Embarrassing discrepancies sometimes arise between archaeological expectations and obsidian dates, but:

> If there is error associated with the application of these hydration constraints (rates), the most likely sources are (1) incorrect hydration rim measurements on artifacts; (2) incorrect identification of obsidian source among dated artifacts; or (3) marked difference between temperature at site and that of the weather station used in calculating the effective hydration temperature [Michels 1982a:3, 1982b:3].

I have always been astonished by this statement because it points the finger first at somebody else -- the hydration technician bungled the measurement; the geochemist blew the source identification -- and fails to acknowledge that discrepancies between obsidian "dates" and independent age indicators may relate more fundamentally to the nascent state of knowledge in induced hydration research itself. Those committed to the laboratory-induced chronometric approach are largely silent when it comes to considering the potential complications involved in translating closed-system laboratory results into the open-system of archaeological sites and artifacts. Whereas certain critical variables can be controlled in closed-system environments (time, temperature, pressure, steam/vapor, silica gel additives, etc.), the open-system world of archaeological sites poses a staggering array of problems involving short and long-term cultural (e.g., scavenging, recycling, curation, redeposition) and natural (rodent activity and vegetation change) transformation processes operating within varying climatic/temperature regimens. While carefully controlled laboratory research is clearly the place to begin (cf. Tremaine 1989; Tremaine and Fredrickson 1988), the disagreements between closed system induced hydration experiments on the same obsidian source (cf. Ericson 1989 with Stevenson and Scheetz 1989) serves as a good indication of the problems to be anticipated in the early stages of this research. Consequently, although I am optimistic about the long-term prospects for induced hydration research, at present the conflicting results should dampen unbridled enthusiasm, uncritical applications, and naive acceptance by archaeologists.

As several researchers have pointed out (e.g., Hall 1983; Jackson 1984; Hall and Jackson 1989), the chronometric age estimates derived from induced hydration formulae are often in rather poor agreement with independent age indicators (C-14 dates, shell beads and ornaments, projectile points, etc.). Consequently, Hall and Jackson (1989), among others, have opted to employ source specific regression power functions to convert hydration values to years. Their careful, meticulous work will no doubt be emulated by many researchers.

Rates currently are evaluated on the basis of how well they agree with independent age indicators (e.g., time-sensitive projectile point types, C-14 dates, shell beads and ornaments). Since most sites lack C-14 samples in unequivocal association with obsidian, proxies, like projectile points and shell beads and ornaments, often are employed as temporal crosschecks. Leaving aside the obvious issue of association, one danger in this procedure is that if independent age assessments are drawn from classes of artifacts that did not function in the same system as the one monitored by obsidian they may be inappropriate, if not entirely misleading, for the task.

Acknowledging the above objections, other archaeologists employ source-specific obsidian hydration values (archaeologically observed as well as those artificially induced through laboratory experimentation) to order archaeological artifacts and assemblages relative to one another, and relative to hydration values from other nearby sources (cf. Tremaine and Fredrickson 1988). Particularly impressive results from this approach come from the North Coast Ranges (e.g., Fredrickson 1984; Origer 1982; Origer and Wickstrom 1982; White 1984).

Measurement and sample size issues are extremely important, and they deserve much more detailed discussion than I have space for here. I would like to make two points: first, that there have been too many "hiatuses" and "episodes of site abandonment" proposed on the basis of pitifully few hydration rim readings without taking into account the instrumental measurement error/ resolution factors. I have seen "hiatuses" proposed on the basis of a 0.2 micron "break", with no apparent appreciation on the part of the archaeologists that such an error falls well within instrumental resolution limits. Archaeologists do not interpret C-14 dates without consideration of associated analytical error, and I find it ironic that many users and generators of obsidian hydration data tend to treat such errors as if they did not exist. While observed hydration "breaks" may be real at 1 site based on very limited data, larger samples often fill in the gaps and complement the more general trends evident in the region.

The second point relates to sample size. An example from my own dissertation work will perhaps illustrate the case (cf. Hughes 1986). When I was selecting samples to analyze, I settled on a 20-25% random sample by point type at each of several archaeological sites in northeastern California and southcentral Oregon. But when I got to the Kawumkan Springs midden in southcentral Oregon, I found that after eliminating untypable specimens, I was left with a whopping total of 109 points. That may seem like a lot but, by northeastern California-southcentral Oregon standards, it is not. But as good graduate students will, I initially stuck to my sampling strategy, and obtained sample sizes by point type of 9, 3, 2, etc. Now, 20 or 25% of 109 does not yield very big numbers (22-27 samples) so -- big surprise -- I recognized no patterning in source use by artifact type in this initial run. But when I finally decided to analyze all 109 samples the patterning in source use showed up in the same way it had at other sites in the region. The point here is that my initial sample size was much too small, and that patterns did not emerge until a large number of samples was analyzed. Although this example relates specifically to source analysis, the same applies to hydration research.

CONCLUDING REMARKS

More generally, I am concerned about binding obsidian studies too closely to the time-honored tradition of California's direct historical approach. The lessons we have already learned from California obsidian studies about variability in the archaeological record (e.g., Jackson's 1974 work) should have served as an indication that there very likely were numerous cultural systems in California's past which became extinct and, therefore, for which we lack good ethnographic analogs. However, as a group we still laboriously chop and fit each new piece of archaeological information into a preconceived mold in which archaeo-linguistic players move through time and space across an unending series of storyboards -- we continuously reshuffle cards in the same deck (cf. Hughes n.d.b). Inspection of the recent continent-wide epidemiological assessments of the impact of diseases on native populations (e.g., Ramenofsky 1987; Dunnell 1991) would suggest that we may not only be playing the wrong cards, but ones dealt from a stacked deck.

The real challenge to obsidian studies for the next quarter-century is no less than it is to California archaeology generally -- to look at the archaeological record in new ways, to explore the variability we encounter, and to employ the objects in the ground to ask different kinds of questions about the past. In some cases, I suspect that the implementation of new research strategies will strengthen the link between ethnography and prehistory, but in other instances I would expect little if any resemblance. If this happens, which I think it will, we will have learned something, both about what worked and, more important, about what did not. If the increasing technical and theoretical sophistication evident in contemporary obsidian studies continues, 25 years from now such studies will take on an even greater role in helping archaeologists learn new things about the past.

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