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THERMAL ALTERATION OF BALD EAGLE JASPER: AUTHORS' REPLY TO PATTERSON

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We reply to the comment by Patterson concerning the role of thermal alteration in the lithic technology of Bald Eagle Jasper from Central Pennsylvania. Several points made by Patterson are interesting; however, other aspects of his critique are not relevant to the research we presented. This reply re-emphasizes the physical and chemical changes resulting from the heat treatment of Bald Eagle Jasper, re-examines the lithic manufacturing process represented at the Houserville site, and presents our view of the role of replicative experimentation in lithics research.

Patterson raises a number of interesting points concerning the article by Schindler et al. (1982), some of which have merit and others which demonstrate some misunderstanding of the experimental procedures and the proposed heat treatment methodology. His critique provides an opportunity to clarify several points made in the original article. The analytical approach of our research addressing the lithic technology of heat-treated materials is somewhat different from that taken by most others concerned with the topic; our research was based on a series of systematic, laboratory-controlled experiments derived from the field of materials science. These experiments revealed several features of the thermal alteration of Bald Eagle Jasper which we then integrated with analyses of the lithic assemblage from the Houserville site.

Patterson's comments appear to fall into three broad categories: (1) the physical and chemical characteristics of the thermal alteration of Bald Eagle Jasper, (2) the nature of Bald Eagle Jasper lithic technology, and (3) the roles of replicative experimentation in lithics research. Each of these is addressed.

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THERMAL ALTERATION

Patterson's primary concern regarding what he defines as the fracture theory of Schindler et al. (1982) is that this theory does not appear to be consistent with his interpretation of the findings of other researchers on the thermal alteration of other cryptocrystalline siliceous materials. Within that perspective, it must be emphasized that the results reported by Schindler et al. (1982) are specific to Bald Eagle Jasper and that the text of the article does not allude to any generality of the specific chemical and physical changes described for Bald Eagle Jasper. Nowhere does the article imply that the chemical and physical processes which occur in Bald Eagle Jasper during thermal alteration should be expected to occur in all other lithic materials. We believe that it is unreasonable to suggest that Ocala chert, Illinois flint, Beldon Lake chert, or some of the novaculities which have been reported to be amenable to thermal alteration processes should undergo the same structural, physical, and chemical alterations as Bald Eagle Jasper, as it is equally unreasonable to suggest that modern day aluminum alloys and high technology steels should experience the same structural changes during their heat treatments. Since flints, cherts, and jaspers exhibit a wide range of impurities and secondary constituents in addition to cryptocrystalline quartz, a wide range of different responses to heat treatment can be expected.

However, it is also necessary to emphasize that there are some similarities revealed for different lithic materials by different researchers. For example, both Purdy and Brooks (1971) and Schindler et al. (1982) report that the thermal alteration process does not appear to affect the quartz component of the cryptocrystalline quartz, as most materials scientists would acknowledge on the basis of the low temperatures involved. Furthermore, although the specific goethite-to-hematite transformation which Schindler et al. (1982) documented to occur in Bald Eagle Jasper cannot be expected to occur in all lithic materials, it is possible that there may exist other goethite and/or iron-containing siliceous materials which are suitable lithic materials and which experience similar changes during heat treatment. The Vera Cruz and Macungie jaspers of Eastern Pennsylvania are two such likely candidates.

The systematic experiments of Schindler et al. (1982) clearly demonstrate a correlation between a decrease in fracture toughness, differential thermal response, weight loss, x-ray diffraction confirmation of the goethite-to-hematite transformation, the yellow-to-red color change and microstructural changes observed in Bald Eagle Jasper by scanning electron microscopy. All of the above are consistent with one another and also are fully consistent with the proposed mechanism for the thermal alteration of Bald Eagle Jasper. If similar experimentation were performed on other heat-treatable lithic materials, results different from those observed for Bald Eagle Jasper might be expected, except for the decrease in fracture toughness. It is also possible that other lithic materials may exhibit color changes during heat treatment and, as Patterson suggests, those color changes may not be synchronous with changes in the physical properties for all siliceous materials. However, the yellow-to-red color change is clearly synchronous with the other property changes for Bald Eagle Jasper.

It is a well established and widely understood tenet of materials science that the structure and properties of materials are related. If, in fact, the heat treatment of lithic materials results in improved workability, then readily measured property changes such as a decrease in fracture toughness and clearly identifiable structural changes must be occurring in those materials. Those property and structural changes must be directly related to the improvement through some process or mechanism. All of the aforementioned observations and experimentation concerning Bald Eagle Jasper are self-consistent in that manner. The development of microcracks between the cryptocrystalline quartz regions was directly observed in the scanning electron microscope and relates directly to the transformation of goethite to hematite. It is a "weakening" mechanism directly analogous to the development of microcracks adjacent to quartz particles in commercial porcelains (Oral 1983; Warshaw and Seider 1967). The Bald Eagle Jasper, much like porcelain, exhibits conchoidal fracture. In both instances, the weakening process is directly related to the appearance of microcracks, although the origins of the microcracks are different for each of the two materials. This weakening mechanism may be the one component of the Bald Eagle Jasper study which has universal application since, as previously noted, Purdy and Brooks (1971) also noted the lack of any change in the quartz component of their Ocala chert. If indeed there are no changes in the regions of cryptocrystalline quartz in the lithic materials, then the changes that occur during thermal alteration can only be related to those components binding the quartz regions together. In Bald Eagle Jasper, those regions are goethite and the structural change is caused by the decomposition of goethite to hematite. It is reasonable to expect that in other lithic materials, changes may be directly related to their specific impurities, or binding component chemistries.

LITHIC TECHNOLOGY

The central concern of Patterson's critique of the Schindler et al. (1982) analysis of lithic technology involves the proposed repeated application of heat treatment. At several levels, however, this aspect of the critique is based on a misunderstanding of the nature of the thermal alteration process in Bald Eagle Jasper and of the character of the Houserville site lithic assemblage.

In the specific case of the Bald Eagle Jasper, the beneficial change of the thermal alteration process is distinctly heralded by the readily obvious color change from yellow to red. This color change relates to the change of iron cations in goethite (yellow) versus hematite (red). The yellow core interiors of preforms and artifacts from the Houserville site did not undergo the same transformation as the reddened core exteriors. Had the yellow interiors experienced the same elevated temperatures during heat treatment, they too would have been reddened. In the laboratory experiments, Bald Eagle Jasper cubes exhibited gradual increases in the degree of surface reddening with increased temperature and time of exposure (Schindler et al. 1982:528). Incompletely heated specimens exhibited only a reddened exterior surrounding a yellow core; furthermore, the thickness of this reddened exterior increased with increased heat treatment temperature and exposure time until the entire experimental cube was red throughout. It is instructive and significant that broken archaeological specimens (both tools and preforms) show the same pattern. A few are red throughout their interiors, while some others consist entirely of unaltered yellow jasper. Most, however, consist primarily of yellow jasper, but exhibit reddening along ridges separating flake scars, on tips or bases, or on other thin portions. These characteristics clearly indicate that individual nodules, preforms, or blanks were not thoroughly heated throughout their interiors. Had they been, the finished tools would not only have been uniformly red, they would also have been seriously reduced in fracture toughness and would have broken much more easily. The Houserville Indians no doubt realized this, even without modern scientific instrumental analyses. Contrary to Patterson's suggestion, this technique of heat treating reflects considerable knowledge of and close control over the thermal alteration process. In the specific case of Bald Eagle Jasper, this precise monitoring of the process was facilitated by the obvious color change from yellow to red.

That heat treatment was not only controlled, but was applied repeatedly during the core reduction sequence is demonstrated by the nature of the Houserville lithic assemblage. The key comparison is not (as Patterson suggests) between the percentages of reddened primary trimming flakes vs. reddened biface thinning flakes. The crucial point is that all flake categories (with the exception of primary trimming flakes) exhibit distinctly higher percentages of reddened specimens than do the corresponding preform categories. This pattern can best be explained by the repeated heating of preforms during the reduction process.

Consider the alternative explanation as proposed by Patterson—i.e., that blanks or crude preforms were heat-treated throughout their interiors in a single heating episode, but that the red color change was a surface and subsurface change only. Initial biface thinning would then have produced relatively high percentages of reddened biface thinning flakes, and similarly high percentages of reddened failed preforms. Subsequent thinning, however, would of necessity produce distinctly fewer reddened thinning flakes and failed preforms, since less and less of the outer, reddened surface of the original blank or crude preform would remain. If Patterson's suggestions are correct, we would therefore expect the Houserville lithic assemblage to contain distinctly higher percentages of reddened specimens in the crude biface thinning flake category than in the intermediate or fine biface thinning categories. However, a re-examination of our results (Schindler et al. 1982:539) clearly refutes

Patterson's suggested interpretation. Crude biface thinning flakes and intermediate biface thinning flakes exhibit roughly equivalent and relatively high percentages of reddened specimens. Crude, intermediate and fine biface preforms exhibit roughly equivalent but lower percentages of reddened specimens. The repeated application of heat provides a more satisfactory explanation of this pattern, since cores at all reduction stages may have been heated and then further reduced, thus generating a predominance of reddened biface thinning flakes for all flake categories and distinctly lower percentages of reddened failed preforms for all preform categories.

The surface collection on which our research was based was not taken in an "uncontrolled fashion," as Patterson states. It consisted, in fact, of all material taken from a series of 3-m-radius dog-leash "vacuuming" operations conducted prior to the site's excavation in 1978. We strongly believe that the sample sizes in each lithic category were adequate for our purposes, which were, (1) to investigate the role of heat treatment in the manufacture of the assemblage, and (2) to generally outline the steps by which this assemblage was created.

In contrast to his arguments regarding repeated heat treating, several of Patterson's other comments concerning the lithic reduction sequence represented at the Houserville site are well taken. Of particular note is his suggestion that more primary trimming materials should have been present had the site functioned as a primary lithic procurement locality. Since the Schindler et al. (1982) paper went to press, the discovery of a Bald Eagle Jasper quarry site approximately 1.7 km from Houserville has led to a reinterpretation of the function of the Houserville site as a camping and jasper-processing locality rather than as a primary jasper procurement locality (Hay and Stevenson 1983). Unworked, and heat-treated but otherwise unworked, nodules of jasper do occur at the Houserville site. This indicates that some jasper was either procured at the site or (more probably) transported there from the aforementioned quarry site. However, Patterson's suggestion that lithic reduction at the Houserville site was geared towards the production of preforms rather than finished tools should not be considered as an alternative to the interpretation proposed by Schindler et al. (1982), for it is the same as that proposed by Schindler et al. (1982:536).

Patterson's suggestion that flake blank production may have preceded biface reduction is also well taken. Although some crude preforms from the Houserville site show unmistakable evidence of manufacture from tabular chunks of raw material, others may have been (and probably were) made on flake blanks. In the case of most preforms, however, it is impossible to determine whether bifacial reduction began with a blank or with an unmodified nodule. For this reason, both procedures were subsumed under the "primary trimming" behavioral category.

THE ROLE OF REPLICATIVE EXPERIMENTS IN ARCHAELOGICAL ANALYSIS

The final issue involves the role of replicative experiments in archaeological analysis. This line of research has been fruitful in lithic studies as a basis for modelling the manufacturing sequences of lithic assemblages. However, Patterson implies that conclusions regarding lithic manufacture cannot be made without experimental verification through replicating artifact forms and manufacturing processes. This assertion reflects an unnecessary restriction to understanding the archaeological record. It implies that contemporary experimentation must recreate actual prehistoric behavior. While modern laboratory experiments may reproduce the products of past behavior as found in the archaeological record, they do not necessarily reproduce the exact aboriginal behavior responsible for that record. Most lithic products can be produced in a variety of ways, some of which have probably not yet been discovered. Even repeated replication experiments do not eliminate all conceivable alternative manufacturing strategies; the possibility always remains that prehistoric peoples employed yet another manufacturing strategy. As archaeologists, we learn about the past only by testing hypotheses concerning past behavior against the archaeological record. Within this context, the primary role of replicative experiments is to generate such hypotheses and to explain the behavior of the materials with which past peoples worked. The verification of these hypotheses lies only in the archaeological record; it is not provided by replication experiments in and of themselves.

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PUNAS, PUNDITS, AND PREHISTORY: COMMENT ON WHEELER'S REVIEW OF "PREHISTORIC HUNTERS OF THE HIGH ANDES"

John W. Rick

Wheeler's review, while concentrating on key topics, uses inapplicable ecological data, misinterprets important issues, and makes incorrect statements about the book's contents.

Prehistoric Hunters of the High Andes, reviewed by J. C. Wheeler in this issue, attempted to go beyond the traditional lithic typological-chronological goals of preceramic Andean archaeology. Wheeler raises serious questions on important parts of the study, in welcome contrast to some past reviews. I will here clarify points in the review which misrepresent the book's contents or Andean ecology.

CLIMATE AND PRODUCTIVITY OF THE CENTRAL PERUVIAN PUNA

In the book, I describe the Central Peruvian puna as benevolent and stable; in contrast, Wheeler prefers the terms "harsh" and "unpredictable." My description of puna wet and dry seasons does not imply a seasonless or optimal climate. A government document on my specific Junín study area (Oficina Nacional de Evaluación de Recursos Naturales [ONERN] 1976) shows that the quantity and seasonality of puna rainfall are not major limiting factors. Their rainfall records of up to 58 years in length record no significant droughts.

A probable source of our disagreement on this point comes from the different areas addressed. Wheeler refers primarily to the Pampas Galeras puna, located 450 km south of Junín: an isolated, dry, and vegetation-poor puna outlier. I explicitly limited my environmental discussion to between 10°30′ and 13°30′ south latitude; Pampas Galeras lies at 14°40′ south latitude. A five-year Pampas Galeras rainfall record in Franklin (1978:12) shows a mean 487 mm yearly rainfall. The Atocsaico weather station, 24 km from the Pachamachay site, shows an 859 mm average over an 18 year period (ONERN 1976). The same sources show that Pampas Galeras receives 90% of its rainfall in

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