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METHOD AND THEORY IN CALIFORNIA ARCHAEOLOGY
SOCIETY FOR CALIFORNIA ARCHAEOLOGY

OCCASIONAL PAPERS IN

METHOD AND THEORY IN CALIFORNIA ARCHAEOLOGY

NUMBER 2

NOVEMBER 1978

GARY S. BRESCHINI, SERIES EDITOR

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by Emma Lou Davis
Director, Great Basin Foundation; Research Associate, Archaeology, Natural History Museum, L. A. County

**Abstract**--This report illustrates the use of low-altitude, low-sun aerials to focus and resolve two major problems of desert geoarchaeology. The questions are: 1) How can Paleolithic artifacts that have been erosionally exposed be retraced to strata that originally contained them? 2) How can the artifacts be dated? A Whittlesey tethered balloon and a Cessna 152 light plane were used in photography that established necessary geological correlations. Results of the imageries were dramatic, providing a key for a locked door. Remote sensing revealed the dynamics and relationships of landscape transformation in a PaleoAmerican site area at China Lake, California. Movement along a fault, combined with a Pleistocene water budget, created a combination of topographic features that PaleoAmericans found inviting during two periods. The features were a dry ridge with a soil surface, adjacent to a productive marsh. A strong earthquake occurred between the two cultural intervals, gashing the intervening, lacustrine stratum like a marked card in a deck. By trenching the fault, paired paleosols can be related to paired, datable bogs in the graben. The dates can be extrapolated to the cultural levels and will also bracket the tectonic event.

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by Jeffrey L. Bada and Patricia M. Masters
Scripps Institution of Oceanography

**Abstract**--Racemization analyses of several Paleoindian skeletons from California have yielded ages of 40,000 to 50,000 years. These skeletons thus represent the oldest dated human remains in the Americas. These early racemization dates have been criticized and considered invalid by some, who claim the racemization dating technique is of questionable reliability. To demonstrate the accuracy of racemization dates, the results from numerous sites around the world are discussed. These results show that there is close agreement between racemization ages and those deduced from other chronological evidence. These correlations provide convincing evidence that the racemization dating method is reliable. One possibility that could cause the early racemization ages for the California Paleoindians to be incorrect is that the Laguna skeleton, used to "calibrate" the racemization reaction for the southern California coastal region, may have been incorrectly dated by radiocarbon. We present correlations between the racemization ages for southern California bones deduced using the Laguna "calibration" and the ages determined from both radiocarbon and geological evidence which show that the Laguna radiocarbon date is correct.

The various results presented here demonstrate that the racemization ages of California Paleoindians are accurate, and these ages thus provide some of the most convincing evidence that people were present in the Americas at least 50,000 years ago.
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University of California, Santa Barbara; Archaeological Reconnaissance, Goleta, California.

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EDITOR'S PREFACE

This issue is the second in the Society for California Archaeology's series *Occasional Papers in Method and Theory in California Archaeology*. The first issue appeared in December of 1977, eleven months ago. That issue contained two dissimilar articles, chosen deliberately to emphasize the intended style of the series. One, a theoretical view of the populating of western North America offered a mechanism for population movements which first inhabited this part of the New World. The other article presented an advanced computer technique for producing computerized perspective surfaces from contour data. This emphasis on new methods of gathering and analysing data and on new theoretical approaches to interpreting or explaining data will continue.

The current issue concentrates primarily on techniques, with articles on amino acid racemization, remote sensing, aerial balloon photography and obsidian hydration. The fifth article presents a new interpretation of dental data, dealing with the relationship between "modern" diets and impacted rear molars. The issue also contains an editorial and several reviews.

Contributions to this series are welcome. Contributions can be in any of four categories—articles, editorials, book reviews, and reviews of previously printed articles. Length of contributions can vary from monographs to short articles or reviews. Style can also vary, but the *American Antiquity* format is preferred. Emphasis should be on new ideas, theories, techniques or approaches of relevance to archaeology or prehistory. Assistance is available in drafting illustrations and other phases of the prepublication process, and any inquiries are encouraged.

Future issues of this series may be aimed at certain topics. Two such topics to which future issues may be devoted (if articles can be obtained) are 1) the prehistory of California, and 2) the question of significance in archaeology. The first would be aimed at presenting a narrative overview of the prehistory of the state, synthesizing existing data into a comprehensive study of the state area by area. Emphasis would be on explaining the data rather than on merely presenting data. The second would present articles on significance, which has been a major topic of discussion in archaeology, and which is likely to become even more critical in the future. Contributions or inquiries on these topics would be appreciated.

Communications should be sent to the following address:

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BALLOON AND BULLDOZER
TOOLS FOR GEOARCHAEOLOGICAL INTERPRETATION

by Emma Lou Davis
INTRODUCTION

Geoarchaeology is the prehistory of people as they related to the living earth. The best devices for recording geo-inscriptions are photo tools of the air (a balloon that reads the earth through the eye of a camera) and earth moving equipment (a bulldozer) to disclose the stratigraphic anatomy containing human records. Experimental works of this nature at Stake 25, China Lake, will be described and discussed.

China Lake, the location of this project, is a structural valley at the upstream end of the Death Valley System of Pleistocene lakes (Bland and Cleveland 1961) in southeast California. Figure 1 shows the geographical situation and internal relationships of these lakes, which are approximately 250 kilometers northeast of Los Angeles.

Present surfaces of portions of China Lake Valley support scatters of PaleoAmerican artifacts mixed with bones of extinct animals. The animals represent a local population of the Rancho La Brea fauna, named for the famous La Brea asphalt pits in Los Angeles. The China Lake faunule is more limited than that of the La Brea, comprising chiefly camel, horse, bison and mammoth (in that order of frequency); fragments of dire wolf, coyote and saber tooth cat and a number of species of birds.

Problems of convincingly relating the human with the animal remains are formidable. Correlating these archaeological/paleontological scatters with the stratigraphics that once contained them is equally difficult and has never been pursued at length or with tenacity. It appears, however, that a combination of aerials at low altitude, and bulldozer-cuts for a stratigraphic profile, can supply an architectural plan-and-elevation that helps to solve the complicated geometrical problems of an eroding landscape.

Aerials are essential for understanding the shape, intensity and effects of surficially expressed faulting. Knowledge of recurrent tectonic activity (e.g., that which continues through "geoarchaeological time") is critical for interpreting lakebed archaeology. This is because faults, in some instances, created natural sagponds and/or natural dams which impounded water. The water, if perennial, created a marsh environment of plants, bogs, organic mats, small animals, herds—and bog traps for large animal kills (see Figure 2).

REMOTE SENSING FOR LAKEBED ARCHAEOLOGY

Aerial photos are applicable in all open-site archaeology, being particularly essential in desert work, where relations of people to the land are critical. Spots or patterns of human occupancy are incorporated in vast landscapes that have been subjected to drastic changes over time. Human activities can only be decoded in terms of the natural forces to which they were adaptations. Remote sensing (e.g., the interpretation of aerial photographs) has proved unique in its capability for revealing dynamics and changes in the Mojave Desert lakebed where we work. From the air, old landforms can be detected under mantles of subsequent deposits. Even stratigraphic overlaps only
a few centimeters in thickness can at times be reconstructed. Low and oblique photography in color opens a previously unknown world exciting for discovery and brilliant for correlation. It is like passing through an invisible door into a beautiful and foreign space. Once an archaeologist has crossed this limen the earth will never again be seen through the same eyes. It will have changed from dirt underfoot to a living, vibrant organism.

Aerial imageries are eyes aloft, capable of discerning past landforms beneath mantles of accumulation and enabling a geogarchaeologist to decode ancient people-nature relationships. Each year, technology, literature and logistics of remote sensing expand to supply alert prehistorians with finer in-
Figure 2. A geoarchaeological thought system, engendered by low-altitude, color aerials.

formation and shortcuts to acquiring it (U.S. Department of the Interior Geological Survey 1977). Commercially available photographs are supplied through the Esos Data Center in Sioux Falls, South Dakota and a computer search can be requested for coverage in the desired target area. Aerial photos have been made from many different altitudes, allowing an archaeological planner to lay out a graduated series of imageries and scales from vast overviews showing hydrologic and tectonic configurations, to intermediate sequences as close as 1:20,000 (Table 1). However, for site details, low and low-intermediate shots are required. Getting these closeups calls for special photo runs that are best made using a light airplane at about 1,300 meters above terrain and a tethered balloon at 25 to 350 meters. For 3-D shadow effects the sun angle must be low. Sixty percent overlap of pictures is needed for stereo pairs. During October 1977, PaleoAmerican site areas at Stake 25, China Lake, were recorded in color from a Cessna 152 at 1,200-1,500 meters and also photographed with Commercial Ektachrome at low altitude by a Whittlesey balloon on a tether. This preliminary work shows that a high probability exists for correlating erosionally exposed archaeology with adjoining buried archaeology and dating both facies by the C14 method, using fossil peat in an associated bog. Landform dynamics, tectonically actuated changes and archaeological relationships at Stake 25 would not have come into focus without the use of balloon and light plane for close-up photography. Equally important, however, is the education of the prehistorian-in-charge as THE major interpreter and coordinator of the whole aerial/terrestrial data input. It has to be seen through one pair of eyes.

Photographic records of phenomena are the same powerful tool for an archaeologist as an astronomer. Through the medium of film, the flickering un-
TABLE 1
Scales and Capabilities of Different Imageries

<table>
<thead>
<tr>
<th>Type of Image</th>
<th>Resolution</th>
<th>Scale</th>
<th>Capability</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>A. OVERVIEW (Photo)</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Landsat (spacecraft)</td>
<td>150? to 250m</td>
<td>1:500,000</td>
<td>Large-scale views of entire region. Useful in correlation.</td>
</tr>
<tr>
<td>Skylab (spacecraft)</td>
<td>20 to 110m</td>
<td>1:500,000</td>
<td>Same as above.</td>
</tr>
<tr>
<td>U.S.G.S. stereo pairs</td>
<td>4m</td>
<td>1:62,500</td>
<td>Medium scale. Good overview with detail as small as dirt roads plus advantage of stereo.</td>
</tr>
<tr>
<td>U.S.G.S. stereo pairs</td>
<td>3m</td>
<td>1:47,000</td>
<td>Same as above.</td>
</tr>
<tr>
<td>U.S. Navy (China Lake)</td>
<td>4m</td>
<td>1:52,000</td>
<td>Same as above.</td>
</tr>
<tr>
<td>U.S. Navy (Miramar)</td>
<td>2m</td>
<td>1:12,000</td>
<td>Trucks and bushes visible.</td>
</tr>
<tr>
<td>Hand-held (light airplane)</td>
<td>1m</td>
<td>1:1,200</td>
<td>Archaeological excavations visible.</td>
</tr>
<tr>
<td>Balloon with camera</td>
<td>0.1m</td>
<td>1:100</td>
<td>12 cm targets visible. This was the mechanism for correlating large images with the accurate plane table maps.</td>
</tr>
<tr>
<td><strong>B. DETAILS (Map)</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>U.S.G.S. Topographic</td>
<td>30' series</td>
<td>1:250,000</td>
<td>Includes a whole system of lake valleys. Useful in correlation with satellite imagery.</td>
</tr>
<tr>
<td>U.S.G.S. Topographic</td>
<td>15' series</td>
<td>1:62,500</td>
<td>Includes a portion of one lake valley.</td>
</tr>
<tr>
<td>U.S.G.S. Topographic</td>
<td>7½' series</td>
<td>1:24,000</td>
<td>Small contour intervals, better detailing.</td>
</tr>
<tr>
<td><strong>C. MEASUREMENT</strong></td>
<td></td>
<td>1:1</td>
<td>This is the best means of accurate measurement.</td>
</tr>
<tr>
<td>Plane table map</td>
<td>± 1m</td>
<td>(reduced)</td>
<td>3-D &quot;portraits&quot; of artifacts, Munsell color records of bone.</td>
</tr>
<tr>
<td>8&quot; x 8&quot; edge-punched cards</td>
<td>1:1</td>
<td>(full scale)</td>
<td></td>
</tr>
</tbody>
</table>

certainties of a visual impression are instantly locked onto a flat sheet (whether color or black and white) to be pondered at leisure, compared, measured and matured into a systematic reflection, highly creative in nature. We have used the air for documenting the case history of China Lake from many considerations: Lake Pleistocene archaeology; geological correlations; tectonic events; and a recent episode of flooding during the 1978 storms that has provided invaluable clues to a subtle sculpturing of the topography by water.
LOW ALTITUDE PHOTOGRAPHY WITH A BALLOON

In October 1977, the Great Basin Foundation engaged the services of the Whittlesey Foundation of Wilton, Connecticut (Whittlesey 1966), for two weeks of experimental balloon photography at low altitudes. The Great Basin Foundation team consisted of two archaeologist-surveyors and a volunteer woman-of-all-work who was a peripatetic anchor for the balloon's tether line, reeled off a backpack. The Whittlesey team was led by Christopher Allen, photographer, pilot and balloonist, accompanied by Thomas Erb, trained in geology and photo-interpretation in the Remote Sensing Program, School of Civil and Environmental Engineering, Cornell University. The following gear was used in the work.

The tethered balloon was a 24 foot, hydrogen-lift vehicle, a Whittlesey Foundation model based on the Robert Fulton military design. The shape is a horizontal teardrop with three tail fins (see title page). The camera used in this work was a Hasselblad EL500 with Zeiss Distagon lens and motorized advance. The Hasselblad was supported in a magnesium balancing gimbal and yoke, adaptable for various cameras (also a Whittlesey design). Camera and gimbal hung about 10 meters below the balloon. The camera was operated by remote control, a Whittlesey adaptation of a Futaba design (see Fig. 3). An operator on the ground triggered the camera with an electronic signal from a handheld transmitter. The frequency was 121.7 MHz. The China Lake job suggests that a few modifications would improve performance of this assemblage. We found that the camera suspension badly needed gyroscopic stabilization. In addition, the radio control should have been operated in a different part of the spectrum where it was less vulnerable to accidental firing by aircraft-to-tower radio signals in the area. Nevertheless, photographic results were excellent after we learned what compensations were needed for aberrations of the balloon and the free-hanging gimbal. The balloon, towed by human operators, was raised to the (approximate) desired altitude by gauging measurements on the tow cord. It was then walked down a track and stopped at measured intervals for pictures (stereo pairs require 60% of overlap). A balloon has advantages of lightness, portability and maneuverability. It also has built-in disadvantages. Its altitude changes with thermals, thereby changing the scale of the pictures, if only slightly, and it yaws in the breeze so that each frame is slightly skewed to the others. Both kinds of aberrations can be compensated for in the darkroom and in the laboratory by enlarging and cropping.

THE AIRSPACE AS A TOOL AND A KEY

Figure 4 is a classic illustration of the First Law of Lakebed Archaeology: IN ORDER TO WORK WITH PLEISTOCENE SITES IT FIRST IS NECESSARY TO RECONSTRUCT THEIR PLEISTOCENE LANDSCAPES. This understanding can only be approached from the air. The oblique view in Fig. 4 shows a Late Wisconsin lakescape, being molded by tectonics, masked by deposits and sculptured by erosion. Buried, exposed, and destroyed archaeology, the special problems of a prehistorian, are shown in beautiful juxtaposition. Locked into an emulsion, the picture fixes the information for the interpretation of geo-events. Film perpetuates the hasty glance of a passing flyer for an endless number of reconsiderations, at leisure, under a binocular glass, at the laboratory desk. A geoarchaeologist must exploit film like an astronomer.
Fig. 3. Erb (left) and Allen check the camera after reloading.
Fig. 4. This beautiful oblique was a handheld shot taken by Allen from about 1,200m aloft. It shows everything we need to know about the surface water supply, tectonic crumpling and differential erosion of this landscape. Note the relative positions of the bulldozer trench (just below center), the fault, the uplift ridge and a playa salt pan that now tops the alluvium filling the former sag pond. **BY TRENCHING ACROSS THIS UPLIFT AND FAULT, INTO THIS SAG POND WE SHOULD BE ABLE TO CUT INTO ITS FOSSIL BOGS WITH DATALBE MATS.**
This picture enabled a quantum leap toward interpretation, prediction, and a line of evidence and procedure for beginning a geochronology of China Lake, a natural time-scale for relating people and animals, climate and microgeology. Aerial work with light plane and tethered balloon is a tool of incredible power. Going even further, the air vehicle combines perfectly with the bulldozer. By complementing aerial knowledge with terrestrial knowledge of this same area (provided by trenching) the following chain of predictive logic has been put together.

First, the major control, the earth herself, must be considered. This countryside is in a part of California where interference between the Pacific and North American plates is causing major deformations. It is tectonically one of the most active places in the world. In the China Lake basin, tectonics have had as much effect as climate on the availability of food, water, and a dry place for PaleoAmericans to camp. Deformations within the tiny area visible in Fig. 4 created a sequence of ecological situations that were useful to ancient foragers. They have also been useful to prehistorians by slowing down the natural processes of site destruction.

Three vectors are at work along the critical system of uplift/fault/graben: vertical shear, horizontal shear, and compression. Compression of the uncompacted deposits pushed up a little thrust block along the ridge. Margins of this block are legible as small, erosional gullies of the ridge from the word "Uplift" and to the right (on Fig. 4). Each time the block was thrust upward, rill action rejuvenated the gullies. Most important for this study, each uplift was accompanied by a drop of the graben creating a deeper sag pond and therefore, briefly, a stronger bog. The larger effect within this portion of the fault system was to crumple or accordion-pleat the landscape, which is being stretched in one dimension and shortened in another.

From an archaeological viewpoint, the ridge/sag pond relation is most important. It will enable us to date the fault episode described below by dating two paleosols that bracket it. This can be done by extrapolation of C14 dates from fossil bogs, now buried under alluvium in the graben. Observations in China Lake Valley and New Mexico (Davis 1978, Fig. 80a; Judge and Dawson 1978) document a custom of camping on a ridge near a marsh. In the western Lakes Country, marshes were a primary subsistence focus wherever they flourished as shown in Fig. 5. This illustration is based on observations at Stake 25. People only used this locality when a supporting marsh was present to supply plant foods, industrial materials, and a bog trap for the megafauna. Therefore, when slippage took place along the fault, the sag pond was deepened and an extensive, marshy border created. Lake Mojave/Clovis foragers made occasional use of this inviting ridge/marsh juxtaposition. The stratigraphic relations are understood from a trench, cut in 1976, and clearly visible in Fig. 4.

Fig. 6 shows the trench wall at a point of fracture. Two paleosols, containing Lake Mojave/Clovis tools and animal bones of the megafauna, bracket an earthquake of considerable severity that ruptured the older paleosol beneath. Somewhat later, a relatively intact, younger paleosol was formed on top. The photographic story is very clear. The quake occurred at the end of a series of lacustrine transgressions between the two Lake Mojave/Clovis occupations. These people operated in small, family bands, and their camps left a sequence of sparse debris: flakes, tools, teeth and bones of mammoth and camel. The cultural materials were scattered throughout the layers of whitish "stringers" of CaCO3 that characterize fossil, pedogenic horizons in this val-
FIG. 5. PALEOINDIAN LAND USE: PATTERN I
The earthquake, "E", is extraordinarily useful to a geoarchaeologist. It labels lacustrine gravel overlying the lower pedogenic horizon or paleosol. This "label" can be identified by a long scar of the earthquake fracture, running across the segment in Fig. 4 designated "exposed archaeology". The gash is invisible underfoot and was only noticed after we took to the air.

How can this sequence of paleosol/earthquake/fractured paleosol be dated? I suggest that it can be done by postulating a pair of fossil bogs that match the fossil soils above, on the uplift, trenching with a bulldozer from the crest of the ridge across the fault into the sag pond depths and dating organic mats form the bogs. The relationship is shown in Fig. 6. This thinking forms an elegant amalgam of anthropology, microgeology, regional tectonics, paleontology, and geoarchaeology synthesis made possible by the eyes aloft that looked through a camera lens from a Cessna 152 and a Whittlesey balloon.

**SUMMARY**

The use of remote sensing in a number of imageries is essential both in planning (Phase I) and survey (Phase II) stages of contract and research field work. The large overviews raise prehistoric interpretations from a level of "artifactology" to geoarchaeology and practical interdisciplinary. Use of remote sensing as an adjunct in Lakebed Archaeology is a particularly good example of two applications of aerial photography, with a tethered balloon and light airplane serving as camera platforms.

Pleistocene lakes in the western United States were ephemeral and fluctuated continuously. During dry episodes, there were barren of plants and abandoned by foraging people. However, during intermediate levels of water, the lakes supported miles of productive marshes that attracted small groups of migratory campers. These folk, in their daily work, left fascinating scatters of tools—for manufacture, preparing plant materials, and for smashing bones of huge animals (now extinct) that were occasionally trapped in sticky clay. Many such lake valleys in the Great Basin have been eroded by late Pleistocene flooding or Holocene winds, exposing some of the buried archaeology. Erosion took a number of forms: vertical channel cuts; vertical pot-holing that produced exosutes called "blow-outs"; horizontal stripping where a single layer may be cleanly dissected over a big area. Both erosional mechanisms can cause total havoc and destruction in certain instances. Blow-outs and stripped surfaces, however, sometimes reveal intriguing clusters of artifacts and animal remains—situations that offer excellent possibilities for site reconstruction. Archaeologists have previously contended that tools and bones in such contexts cannot be associated either with stratigraphic sections or valid dates.

**THIS IS UNTRUE.**

Aerial photographs taken at low altitude (25 to 100 meters above terrain) and intermediate altitude (100 to 1,500 meters) are powerful devices for discerning and relating the different archaeological contexts described above. Actual condition of the archaeological materials is: 1) buried, 2) exposed, and 3) hopelessly scattered. In October 1977, a remote sensing investigation was carried out by the China Lake Program, using as a test case an extensive area of PaleoAmerican sites called Stake 25. A Whittlesey Foundation tethered balloon and a Cessna 152 light plane were used for verticals and obliques respectively.

The insights and interpretations made possible by remote sensing are
Fig. 6. You are standing in the bulldozer trench at Stake 25, looking at 2m (6 feet, 7 inches) of natural stratigraphies from the lip of the trench down to its floor. The measuring devices are in feet and inches. Lacustrine sands 1 and 2 are separated by a weak surface. A weak paleosol may correspond to a weak soil at Stake 1, Trench 3, \(^{14}C\) dated 2,465±180 yrs BP (GX-3445). The break at both Stakes comes between sands 2 and 3. Pedogenic nodules are scattered through the lower part of Sand 3, particularly at the extreme left of the picture where a division between Sand 3 and the Upper Paleosol is hard, clayey, made up of layers of CaCO\(_3\) and is divided in the middle by an erosional disconformity (white line). This unit contains Pre-Classic, Classic and Post-Classic Lake Mojave/Clovis artifacts with animal bones. A strong earthquake "E" had occurred before the Upper Paleosol was formed, offsetting and fracturing the underlying layers shown in this figure. By dating an upper pair of fossil bogs in the Stake 25 graben, the corresponding pair of upper soils can be dated by extrapolation. This operation will bracket the earthquake in time.
dramatic, providing a prehistorian with eyes of the air. Thus, theory and methods of prehistory are extended beyond culture-historcial archaeology, tool-types or "artifactology" and become firmly placed in the interdicipline of geoarchaeology (Butzer 1975). A number of changes in the paradigm of archaeology are required to transform it into the geoarchaeology of lakebeds. Theoretical changes apply in particular to sampling, prediction, and the importance of fossil soils as culture-bearing units.

1) In order to treat desert archaeology predictively, it must be placed in a framework of microgeological process, climatic change, and environmental change. Aerial photography, in color and from low altitude, is an incomparable tool for perceiving landscape dynamics. From aloft, ancient landscapes and their processes of transformation can best be detected.

2) This changes sampling strategy. Random samples are only occasionally useful. Aerially perceived landscapes rather than worm's-eye views of grids must be sampled on a basis of experience, like a skilled fisherman working a trout stream. I repeat: PLEISTOCENE SITES ARE UNLIKELY TO BE FOUND BY SAMPLING A MODERN LANDSCAPE. INSTEAD, IT IS NECESSARY TO RECONSTRUCT AND SAMPLE A PLEISTOCENE LANDSCAPE.

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Fig. 7. A combined plan and profile drawing of Stake 25 showing the proposed, new trench and postulated relationships of paired paleosols with paired bogs.
FIG. 7

POSITION OF PROPOSED TRENCH AT STAKE 25
CHINA LAKE

PROPOSED TRENCH AT STAKE 25

CROSS SECTION OF STRATIGRAPHY

A-A
THE ANTIQUITY OF HUMAN BEINGS IN THE AMERICAS:
EVIDENCE DERIVED FROM AMINO ACID RACEMIZATION DATING
OF PALEOINDIAN SKELETONS

by Jeffrey L. Bada and Patricia M. Masters
INTRODUCTION

No other topic in American archaeology generates more emotional dis­
cussion and intense debate than does the antiquity of man and woman in the New
World. The evidence which is generally considered most relevant to this ques­tion concerns the artifacts recovered from several New World localities. How­
ever, the antiquity of artifacts is often open to question, since the age es­
timates are frequently based on subjective and controversial interpretations
(references 1-3). Moreover, it is also hotly debated as to whether the chipped
stones found at some sites are actually artifacts of human beings or are pro­
ducts of natural processes (for example, see discussion in references 2 and 3).

To us, it appears that the single most important piece of evidence
necessary in establishing when people first migrated into the Americas is not
the antiquity or authenticity of "artifacts" but, rather, the ages of human
skeletons found in the New World. If the age of a Paleoindian skeleton can
be unequivocably determined, then the presence of human beings in the New
World is established at that particular time. It then follows that if people
were present in the Americas during a certain time interval, the artifacts
produced by their cultural activities must also exist.

A large number of skeletons have been found throughout the New World,
but most of these have never been directly dated. In 1974 (4) and 1975 (5),
we dated several skeletons from California at 40,000 to 50,000 years, using a
new technique called amino acid racemization dating. (For a discussion of the
principles of amino acid racemization dating see references 5-7.) These early
racemization ages have provided important new evidence that people had migrated
into the New World much earlier than the more generally accepted period of en­
try of 15,000 to 20,000 years ago. However, the early racemization ages have
been viewed with skepticism by some, who either consider the racemization dat­
ing technique itself to be of questionable validity or believe that the radio­
carbon age of the skeleton used to "calibrate" the amino acid racemization re­
action, i.e., the Laguna skull, is unreliable.

In this paper we present the results of a series of tests which demon­
strate both the validity of the racemization dating technique and the reliabil­
ity of the radiocarbon age for the Laguna skull. These results have provided
additional evidence that the racemization ages of 40,000 to 50,000 years de­
termined for several California Paleoindian skeletons are correct.

VALIDITY OF AMINO ACID RACEMIZATION DATES ON BONE

The amino acid racemization dating technique has been applied to bones
from more than 25 sites throughout the world (5, 8-14). Some of the sites
from which bones have been dated include the Klasies River Mouth Caves (South
Africa; 5, 12); Olduvai Gorge (Tanzania; 5, 8); Broken Hill (Rodesia; 9); Tar­
khan and Tura (Egypt; 10, 11); Skhul and Tabûn (Israel; 10, 13); Arago (France;
13, 14); Stráská skála and Kúlna (Czechoslovakia; 10, 11) and Murray Springs
(Arizona; 5, 9). The racemization ages, in general, have been found to be in
excellent agreement with the ages derived from independent evidence such as
radiocarbon dating, geological interpretation, and historical records.

In only two instances have we found a large disagreement between race-
mization-deduced ages and those derived from other information. One of these disparate results came from Olduvai Gorge, in East Africa (5). In this particular case, the racemization age is compatible with the age estimated from archaeological and geological evidence, while the radiocarbon age deduced on the same sample is much too young (see discussion in reference 5). This result has suggested to us that in instances where bones are so badly contami­
nated that the radiocarbon dates are essentially meaningless, the racemization ages still provide a reasonable indication of the actual age of the sample.

A sample from Sakkara, Egypt, has also yielded an anomalous racemi­
ization age (10, 11). In this case, the racemization age is much too young in comparison with the actual historical age and is thus erroneous. Analyses of other Egyptian samples, however, have yielded racemization ages that are in close agreement with the historical ages of the samples (10, 11). The reason for the anomalous Sakkara racemization age is not known, but this result does suggest that if a racemization age is erroneous it will be too young rather than too old.

The above-mentioned results have convinced us that racemization­
deduced ages are reliable and accurate. It is true that most (but not all) of the correlations between racemization-derived and independently deduced ages have been carried out at localities outside North America. However, it would be extraordinarily peculiar if the racemization dating technique were valid everywhere on the earth except the New World.

RACEMIZATION ANALYSES OF HOLOCENE-DATED
SKELETONS FROM CALIFORNIA

When using the amino acid racemization reaction to date fossil bones, it is assumed that the major factor that affects the extent of racemization in a sample is the average temperature to which the bone has been exposed. The "calibration" procedure, in which the extent of racemization in a sample of known age is used to calculate an in situ racemization rate (i.e., the kasp value), is essentially a method of evaluating the temperature history of a site. (See references 5-7 for a complete discussion of the procedures, equations, etc. used in racemization dating.) However, since racemization is a chemical reaction, it has been suggested that factors other than temperature could greatly affect racemization rates (15-17). Moreover, it has been argued that variations in these other environmental factors through geological time might affect the extent of racemization in a fossil bone in a manner perhaps not compensated for using the "calibration" technique (17).

Elsewhere (5, 10, 18) we have shown that the racemization rate con­
stants determined at numerous localities throughout the world correlate well with the estimated temperature histories of the "calibration" samples. We have also demonstrated that variations in environmental parameters such as pH have little effect on racemization rates (18). Nevertheless, arguments still persist that variations in environmental factors other than temperature might produce an anomalously high extent of racemization in fossil bone and that this could explain why, in skeletons like Del Mar Man, etc., the amino acids are highly racemized. We felt, however, that if environmental factors can influence racemization in this manner, these effects should be detectable in Holocene-dated bones. To test this, we analysed Holocene radiocarbon-dated aboriginal skeletons from throughout California.

These results are listed in Table 1. In no case do the D/L aspartic acid ratios determined for these skeletons exceed ν0.2. The Holocene D/L
ratios are much lower than the ratios of ~0.4–0.5 determined for the skeletons such as Del Mar Man and Sunnyvale (4, 5). The Holocene samples that we investigated came from a wide variety of localities which differed greatly in their general environmental characteristics. If variations in environmental parameters could produce anomalously high racemization, we feel that this should have been detectable in at least some of the samples which we analysed. Since the extent of racemization in the Holocene samples from throughout California is consistently much less than in the Del Mar Man skeleton, etc., this indicates to us that the most reasonable explanation for the highly racemized amino acids in these latter skeletons is that they have a considerably greater antiquity than the Holocene skeletons.

RELIABILITY OF THE LAGUNA "CALIBRATION"

The Del Mar Man and other Paleoindian racemization ages could be erroneous if the Laguna skull radiocarbon age of 17,150 years (UCLA 1233A) is incorrect. It has also been suggested (although not in a formal publication) that the "Laguna" skeletal material that was dated by radiocarbon was not from the original skeleton recovered in 1933. The Laguna skull had a rather bizarre post-excavation history. The skeleton was sent to Europe, and even to Africa, for examination by several noted anthropologists, including L.S.B. Leakey. There has been some question as to whether the sample that was returned and dated was indeed the original material. However, the radiocarbon-dated material closely resembled the skeletal remains shown in photographs taken at the time the skeleton was excavated (19). Nevertheless, this uncertainty still exists.

One way to test the reliability of the Laguna "calibration" would be to use the Laguna-derived k_{asp} value to date other bones which, in turn, have also been dated by radiocarbon. This would also test the authenticity of the Laguna skeleton since bones from another locality would have experienced a temperature history different from that of the southern California coastal region and thus would yield a k_{asp} "calibration" constant which would not be applicable to coastal southern California sites.

Using the Laguna-derived k_{asp} values, we have dated several Holocene samples found along the southern California coast, and these results are listed in Table 2. As can be seen, the ages are in close agreement with the radiocarbon ages determined for the various bones. If the Laguna skull radiocarbon date was incorrect, or if the skull that was dated was not the original skull, these correlations would be expected to be much poorer than they are.

Another test of the validity of the Laguna calibration is the fact that the racemization age for the Los Angeles Man skeleton deduced using the Laguna-based k_{asp} value is 26,000 years (4), which is consistent with the radiocarbon age of >23,600 years (UCLA 1430) determined for this skeleton. Concor- dant racemization and radiocarbon ages have also been obtained for a mammoth from Santa Rosa Island (4), although this result should be considered somewhat less significant because of some adjustments required in the Laguna k_{asp} value to account for a lower temperature environment at the Santa Rosa Island site.

A result that provides additional evidence that the Laguna k_{asp} value can be used for dating Upper Pleistocene bones from sites along the southern California coast involves an event that took place on the campus of Scripps Institution of Oceanography. In October, 1975, a portion of the sea cliff ap-
Table 1. Racemization of Aspartic Acid in Holocene Radiocarbon-Dated Aboriginal Skeletons from California.

<table>
<thead>
<tr>
<th>Site</th>
<th>Radiocarbon Age</th>
<th>D/L Aspartic Acid*</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Northern California</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Marin 152</td>
<td>3,270±70 (UCLA 1891A)</td>
<td>0.112</td>
</tr>
<tr>
<td>SDA-66</td>
<td>9,040±210 (UCLA 17958)</td>
<td>0.150**</td>
</tr>
<tr>
<td>Stanford Man</td>
<td>7,750±400 (UCLA 1995C)</td>
<td>0.172**</td>
</tr>
<tr>
<td><strong>Central California</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Tranquility</td>
<td>5,130±10 (UCLA 1861)</td>
<td>0.14</td>
</tr>
<tr>
<td><strong>Southern California</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>W-9, SDi-4660, SDM</td>
<td>2,550±60 (UCLA 1623B)</td>
<td>0.127</td>
</tr>
<tr>
<td>skeleton 19241</td>
<td></td>
<td></td>
</tr>
<tr>
<td>W-12, SDi-4669, SDM</td>
<td>6,700±150 (LJ-79)</td>
<td>0.154</td>
</tr>
<tr>
<td>skeleton 16709</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Ora 64, 50-60 cm level</td>
<td>6,900±140 (GAK-4136)</td>
<td>0.170</td>
</tr>
</tbody>
</table>

* All samples were hydrolyzed 24 hours, except as indicated.
** Sample hydrolyzed 4 hours. A 4-hour hydrolysis would yield a D/L ratio of ~0.025.

Table 2. Racemization Ages of Holocene Aboriginal Skeletons from Southern California Coastal Sites.

<table>
<thead>
<tr>
<th>Site No.</th>
<th>SDM No.</th>
<th>D/L Aspartic Acid</th>
<th>Aspartic Acid Age (yrs)*</th>
<th>C-14 Age (yrs)</th>
</tr>
</thead>
<tbody>
<tr>
<td>W-9, SDi-4660</td>
<td>19241</td>
<td>0.154</td>
<td>5,700</td>
<td>6,700±156†</td>
</tr>
<tr>
<td>W-12, SDi-4669</td>
<td>16709†</td>
<td>0.142</td>
<td>7,900</td>
<td>8,360±75</td>
</tr>
<tr>
<td></td>
<td></td>
<td>0.189</td>
<td>8,100</td>
<td>(Pta-1725)</td>
</tr>
<tr>
<td>Ora 64, 50-60 cm</td>
<td>---</td>
<td>0.170</td>
<td>6,800</td>
<td>6,960±140†</td>
</tr>
</tbody>
</table>

* Dated using kasp = 1.5 x 10⁻⁵ yr⁻¹ (given in reference 4). Except as indicated, all the samples were hydrolysed 24 hours. The t=0 term for a bone sample hydrolysed 4 hours is ~0.05 (Bada et al, in preparation).
† Radiocarbon date on associated shell.
‡ This sample was also analysed by G. Dungworth and H. Kessels, University of Nijmegen, and found to have a D/L aspartic acid ratio of 0.208. This D/L ratio yields an age of 9,400 years.
proximately 60-80 meters north of Scripps Pier collapsed, exposing several fossil bones. Excavation of the freshly exposed cliff face and the collapsed cliff rubble resulted in the recovery of the nearly complete leg bone and the scapula of the extinct horse, *Equus occidentalis*. Since this horse became extinct in southern California near the end of the Pleistocene (20), the horse bones are greater than 10,000 years old. Racemization analysis of these horse bones yielded a D/L aspartic acid ratio of 0.53, which is essentially the same as that of Del Mar Man. (The horse bones were also analysed by G. Dungworth, University of Nijmegen, The Netherlands, and D/L aspartic acid ratios identical to the Scripps results were obtained.) Using the Laguna $k_{asp}$ value, the racemization age of the horse skeleton is therefore $\approx 50,000$ years. If the age of the horse skeleton can be ascertained by other techniques, this not only would provide another test of the validity of the Laguna "calibration" but would also provide an independent reference sample to use for determining the racemization age of Del Mar Man.

The geology of the Scripps cliff has been discussed previously by Carter (1) and by Karrow and Emerson (21). The cliff is composed of Pleistocene alluvial and colluvial sediments, overlying the Ardath Shale and Scripps Formation, which are Eocene in age. At the contact between the Pleistocene sediments and the Eocene conglomerate is a raised marine fossiliferous terrace deposit. Shells obtained from this terrace, collected $\approx 40-50$ meters north of the area where the cliff collapsed, have been analysed by amino acid racemization and found to correspond in age to a period of high sea level during the last interglacial, i.e., $\approx 120,000$ years B.P. (P.E. Hare, unpublished results cited in reference 21; P. Karrow and J.L. Bada, unpublished results). This age thus provides a maximum age for the horse skeleton.

Also exposed on the cliff face, both above and below the horse bones, were small flakes of charcoal. The origin of this charcoal is uncertain, but it is thought to be the result of a natural brush fire, rather than from human campfires, although this interpretation is speculative. Morphological studies (carried out by J. Herring, Scripps Institution of Oceanography) of some of the pieces of charcoal suggested that it was derived from pine, possibly Torrey Pines, which are still found in the area of Torrey Pines State Park, some 5-7 km north of Scripps. In some places on the cliff face, large chunks of charcoal were encountered. Charcoal chunks in sufficient amounts for radiocarbon dating were recovered from two levels, one $\approx 30-40$ cm directly below and the other $\approx 3$ m above and $\approx 10$ m south of where the horse bones were recovered. These charcoal samples were submitted to the Mount Soledad Radiocarbon Laboratory and were found to have ages of $36,800 \pm 2,000$ years (LJ 3470) and $38,000 \pm 3,000$ years (LJ 3530), respectively. Both of the ages are very close to the upper dating limit of the Mt. Soledad Radiocarbon Laboratory and should probably be considered minimum ages. Another charcoal sample was obtained from a large cliff chunk which had fallen onto the beach. This sample yielded an age of $>39,000$ years (LJ 3469) and was believed to have come from a stratigraphic horizon immediately below the horse bones. These ages are consistent with some earlier radiocarbon dates (22, 23) for the Scripps cliff of $21,500 \pm 700$ years (W-142) and $>34,000$ years (W-217) for charcoal recovered from horizons stratigraphically above and below, respectively, the horse skeleton. The location of the various dated samples and their corresponding racemization and radiocarbon ages are summarized in Figure 1.

The charcoal radiocarbon dates and the age of the marine terrace which underlies the horse skeleton indicate that the horse bones have an age in the
range of >30,000 to <120,000 years. This age range is consistent with the ~50,000-year racemization age estimated for the horse. Since the Scripps cliff horse bones have essentially the same D/L aspartic acid ratio as Del Mar Man, this implies that the age of this skeleton also falls in the range of >30,000 to <120,000 years.

We believe that these various comparisons demonstrate that the Laguna skull radiocarbon date of 17,150 years is correct, that the dated Laguna material is indeed the original skull, and that the $k_{asp}$ value derived from the racemization and radiocarbon analyses of the Laguna skull can be used to "calibrate" the amino acid racemization reaction for the southern California coast.

CONCLUSION

The results we have discussed here and which have been published elsewhere convincingly demonstrate the validity of racemization dating. Also, we have shown that the Laguna-based $k_{asp}$ value is reliable and that the racemization ages deduced using the "calibration" constant are in good agreement with those derived from both radiocarbon and geological evidence. These results thus strongly suggest that the 40,000-50,000-year racemization ages determined for several California Paleoindians are indeed correct. These early racemization dates therefore provide some of the most convincing evidence that people were present in the Americas at least 50,000 years ago.

ACKNOWLEDGMENTS

We thank the numerous individuals who provided samples of the various Indian skeletons, H. Suess and T. Linick for carrying out the radiocarbon age determinations, and D. Darling for technical assistance. This work was supported by NSF grant EAR73-00320-A01. JLB is an Alfred P. Sloan Fellow. Special thanks to Anne.

Figure 1. Photograph taken about two months after the collapse of the cliff north of the Scripps Institution of Oceanography pier. The locations and ages of the various samples are as follows: (A) horse scapula, ~50,000 years (aspartic acid racemization); (B) horse leg bone, ~50,000 years (aspartic acid racemization); (C) charcoal, 38,000±3,000 years (LJ-3530); (D) charcoal, 36,800±2,000 years (LJ-3470); and (E) contact between marine terrace deposit (~120,000 years, amino acid racemization on shell) and Eocene. A charcoal sample dated at 21,500±700 (W-142) was obtained in 1960 from a level which was stratigraphically above the horse but ~25-30 m south of the area of the recent cliff collapse. Another charcoal sample, which yielded an age of >34,000 years (LJ-217), was collected in 1953-1954 (?) from a stratigraphic layer which underlies the horse bones at a locality ~50 m north of the collapse. Charcoal was also obtained from a large chunk that had fallen onto the beach. This sample was dated at >39,000 years (LJ-3469) and was thought to have come from the same stratigraphic level as (D).
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CONSIDERATIONS OF BALLOON AERIAL PHOTOGRAPHY AND 35 MM CAMERAS

by Cliff Taylor
INTRODUCTION

The use of aerial photography has become well established in the sciences as a means of research and data collection. Many of the photographic techniques developed by other scientific disciplines have been borrowed for use in aerial archaeology. The purpose of this paper is to share recent developments in balloon aerial archaeology using a 35 mm camera to acquire aerial photographs, including the application of remote sensing techniques.

DISCUSSION

Four of the primary concerns to the archaeologist considering aerial photography would be: 1) ability to acquire photographs during nearly any stage of the project or time of day, 2) capability to readily vary the area of ground-cover viewed, 3) mobility and adaptability in use over land and water, and 4) photogrammetric capabilities with various methods of data collection.

For other purposes the occurrence of aerial photographs taken from an airplane is not unusual, and often quite successful in the discipline of archaeology. Sometimes it is desirable to obtain large-scale photographs which offer greater detail of a site. However, the use of airplanes has its physical and legal limitations when consideration is given to acquiring low level aerial photographs (1,000 feet or less) because the Federal Aviation Administration (FAA) restricts fixed-wing aircraft to a minimum altitude of 1,000 feet (vertical distance) from any structures in congested areas and 500 feet in the open country (Federal Aviation Regulations, Chapter 3, Part 91.79). It is also very difficult to acquire from an airplane photographs of various film/filter combinations (shot from the same altitude) encompassing the identical field of view without employing a multiband camera.

An alternative method is the use of a helicopter. Although FAA restrictions generally do not limit its use (local restrictions may apply in some areas), the hovering platform is limited by vibration. Also, there is a tendency for engine overheating, excessive strain on the pilot and machine, and a rental cost greater than that of renting an airplane.

These are a few reasons why it is desirable to employ a tethered balloon as a photographic platform. Two types of balloons, spherical and aerodynamic blimp shape, have been used successfully by the author. Although the spherical balloon is less expensive to employ than the blimp, the aerodynamic design of the blimp does allow for greater control and lift in the occurrence of wind (title page illustration). The adverse affect of wind on spherical balloons, usually causing them to lose altitude and become unstable, generally requires flight schedules during early morning. But the versatility of the aerodynamic blimp gives the archaeologist a useful stable photographic platform throughout the day. Still, both balloon platforms: 1) allow much control in variability of positioning the camera, 2) afford the archaeologist with time to acquire photographs using different film/filter combinations so as to meet his research and recording needs, and 3) cost less than either helicopter or airplane.

APPLICATION 1: VARIATIONS OF GROUND COVERAGE

Sometimes the universe of an archaeological site will be stratified and
each strata excavated differently so as to implement a research design. Such a case might require not only a documentary photograph of the entire site, but additional photographs of each discrete strata. This requires centering the camera over the strata at a prescribed altitude in order to include in the view of the camera only that portion of the strata desired (for information about different focal length camera lenses and ground coverage see Fant and Loy 1972).

The balloons are easily manipulated directly over each target with the use of three nylon tether lines. The tether lines are attached to the base of a balanced pendulum type camera firing mechanism. This system was developed by the author (patent pending). One tether is usually up wind and the other tether lines are set out to form a triangle (see title page illustration). The altitude of the camera is determined with a balloon theadolite or similar instrument by employing triangulation methods. Continuous communications are maintained among all crew members via walkie-talkies for the duration of the ballooning activities. When the camera is properly located over its target and all systems are ready, the balanced camera firing mechanism is tripped by radio control. A strobe flash signals a response to the transmitter each time the camera shutter is released.

A project recently concluded for Dr. James Moriarity of the Charles H. Brown excavation exemplifies the capability and precision of this system (Fig. 1). He acquired in 3 hours, 76 photographs including three different ground views with each view recorded on three types of film and each film combined with three different filters (results to be published at a future date).

APPLICATION 2: MOBILITY AND ADAPTABILITY

The aerodynamic blimp fits into a small suitcase when deflated and fully dismantled. The spherical balloon fits four into an 8 inch square box when new. Thus, the greatest mass is in the four helium bottles used to transport the gas for the blimp. Still, all the materials necessary to perform a balloon aerial archaeology mission will fit comfortably into a jeep for transport.

The application of tethered balloons for coastal water studies is also feasible. Such consideration might be given to near shore underwater archaeological sites or oceanographic and environmental research. Choule J. Sonu (1969) tested a prototype aerodynamic balloon for this purpose with some success. Although his radio control device failed and the balloon began to breach at the seams, he did acquire photographs disclosing important data pertaining to near shore currents. It would be useful to combine his tether/pully system with the aerodynamic blimp and pendulum type camera firing mechanism.

APPLICATION 3: PHOTOGRAHMETRIC AND DATA COLLECTION CAPABILITIES

There has been much debate about types of camera equipment to use and which camera format gives adequate resolution at various altitudes. In an attempt to resolve some of the questions raised, the National Oceanographic and Atmospheric Administration (NOAA) performed accuracy, resolution, and cost comparisons between the small format cameras and the larger mapping camera intended for environmental studies (Clegg and Scherz 1975). The cameras compared were a 35mm, 70mm and 9 inch format. These cameras were tested by 11 criteria for soils interpretation. Test results showed that at 1,000 feet a six foot square panel, photographed by all formats, was clear (fuzziness is minimal);
Fig. 1. Charles H. Brown Excavation.
corners are not acute). The photo interpretation capability of the three format sizes, showed the 35mm camera rated first in identifying vegetation with the 9 inch camera first in identifying soils. The mean distance discrepancies between photo distance and map distances for all formats showed the 35mm to be the least at a 1.39% error.

The conclusion of the NOAA report states:

...the advantage of (the 35mm) system are that it is the least expensive in equipment and operation (see Table 1), the lightest in weight and the most versatile. The metric accuracy tests using a rectifier-enlarger showed that within tightly controlled areas the smaller formats provided just as accurate point locations as the 9 inch format. A variety of lenses can be obtained and easily changed during aerial operations. The accuracy requirement of aerial mapping is in the magnitude of tens of feet. Small format systems can provide such accuracy, and their initial and operating costs are about 1/10 the cost of 9 inch mapping camera systems (Clegg and Scherz 1975: 686-689).

The high quality 35mm, single-lens reflex camera offers great technical versatility, extreme mobility, and great film economy (Hester, Heizer and Graham 1975: 238). It is little wonder 35mm cameras have become standard equipment among archaeologists. A motorized Canon AE-1 with a data-back is used by the author for aerial photography. The lenses most often used are a Canon 28mm wide angle and a 50mm standard. A 50mm macro lens and its roll in balloon aerial photography is now being tested for flatness and accuracy. If the test results are positive, future efforts will be spent on developing stereoimagery of excavations as recorded by 35mm camera (see also Hallert 1971).

Aerial photography is only one specialty in the science of obtaining data about object phenomena at a distance and is more accurately termed "remote sensing." Another form of remote sensing important to the archaeologist is the recording of infrared radiation. Films sensitized to this invisible portion of the light spectrum were applied to a study of stone enclosures found in San Diego (Figure 2). The infrared photographs could delineate the individual stone enclosures more clearly and distinguish the different plant species and their health.

Differences in soil density, color, and moisture content are also detectable by special remote sensing techniques. Balloon aerial photographs taken with a 35mm camera have demonstrated capabilities of distinguishing soil- horizons, rock formations and various humanly-formed combinations. A six sectional color panel, for which the spectrophotometric reflectance of each section is known, is employed in each project. This color panel yields information

<table>
<thead>
<tr>
<th>Format</th>
<th>Equipment Cost</th>
<th>Cost per Exposure</th>
</tr>
</thead>
<tbody>
<tr>
<td>9 inch</td>
<td>$101,000</td>
<td>$17.51</td>
</tr>
<tr>
<td>70mm</td>
<td>$7,500</td>
<td>$2.85</td>
</tr>
<tr>
<td>35mm</td>
<td>$6,000</td>
<td>$0.80</td>
</tr>
</tbody>
</table>

Table 1. Comparison of Costs per Format
Fig. 2. Infrared photograph of stone enclosures in San Diego.
on the uniformity and quality of the film processing, and facilitates proper focusing and measurement during analysis (Figure 1). Because most of the 35mm photographs acquired are positive transparencies, they can be readily examined with enhancement techniques in which subtle differences in ground conditions can be detected (Molineau 1965). Occasionally, for analysis and publication purposes, more contrast is desired in a print. Greater contrast can sometimes be acquired during the bath part of the printing process by utilizing an ultrasonic vibrator.

CONCLUSIONS

The evidence presented illustrates the advantages of having a functionally integrated balloon aerial photographic system in the archaeologist's tool kit. It has been shown that in terms of versatility, time and expense that this method is superior to both airplane and helicopter-borne systems. Tethered balloon systems have made aerial photographic techniques accessible to researchers whose budgets cannot support aircraft reconnaissance. This factor alone should ensure increasing popularity of tethered balloon applications in the immediate future. Although the technical advantages are already overwhelming in certain circumstances, its full potential remains to be seen. It is toward those potentialities that the author intends to direct future investigations and will welcome correspondence with any interested colleagues.

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INTERPROXIMAL ATTRITION AND MODERN DENTAL CROWDING

by Grover S. Krantz
Dental crowding occurs when the combined mesiodistal length of all of the teeth is too great for the alveolar part of the jaw to accommodate. This commonly results in dislocated teeth and the impaction of the third molars. Crowding is far more frequent in "civilized" peoples than in more "primitive" populations, although recent anthropological literature does not often deal with the subject.

Probably the most commonly offered explanation for dental crowding is that the jaws have become too small to properly accommodate the teeth. This reduction in jaw size supposedly results from lessened usage of the jaws due to the soft prepared foods that are consumed, such that "...disuse, from childhood on, causes retardation in the growth of jaws. The teeth on the other hand reach their full dimensions" (Oppenheimer 1964: 420). This was rightly challenged by Kostaloglu (1966: 355) with the observation that it was only the muscular attachments of the jaws which reduced from disuse, and not the alveolar part. He went on, however, to confuse the issue by attributing crowding to caries in the deciduous dentition.

These arguments, and many others before them, generally make the same assumption—that somehow it is the jaws which have become too small. It would be equally logical to examine the possibility that it has been the teeth which have become too large.

Interproximal* attrition refers to that wear which occurs on the contact areas between adjacent teeth in the jaws. It is a direct result of the independent movements of individual teeth against adjoining teeth, and produces characteristic wear facets on the mesial and distal sides of the tooth crowns. Severe interproximal attrition, which occurs normally in the teeth of "noncivilized" peoples, can considerably reduce the mesiodistal length of each tooth—it can make the teeth smaller.

The nature of interproximal attrition was the subject of a recent and exhausting study by Wolpoff (1971) and I will not attempt here to duplicate or repeat his efforts. Wolpoff reviewed the numerous studies of this kind of attrition by previous workers and noted, among other things, the quantity and timing of the tooth loss that occurs through interproximal attrition. While interproximal attrition continues throughout most of the dental life of an individual, it is only that amount of wear which occurs up to the time of the eruption of the third molars that is of interest in this study.

A small facet is normally worn on the mesial and distal faces of each tooth from contact with the neighboring teeth. As all teeth tend to migrate mesially (toward the midline of the mouth), the last tooth of each row will shift forward by an amount equal to the total of all of the wear on all of the teeth of that row combined. From the first incisor to the second molar

* The term interproximal is used here rather than interstitial (Wolpoff 1971). "Dortland's Illustrated Medical Dictionary (1965) defines interproximal as "Between adjoining surfaces," and interstice as "A small interval, space, or gap in a tissue or structure. Since there is constant contact of surfaces in this case the former term is preferred."
there is a total of 13 faces which receive wear in each half of each jaw. If the wear on each of these faces is only one-fourth mm, mesial drift will cause the second molar to become located more than 3 mm forward from the position it would have occupied if no interproximal attrition had occurred.

Wolpoff (1971: 211) cites several sources which indicate that the total shortening of tooth rows in various "noncivilized" peoples is as great as 10 or 11 mm. Some of the evidence is a bit contradictory, but it is clear that much of the shortening occurs before the eruption of the third molars—as much as 5.3 mm in the case of the Australians. How often would the addition of 5 mm of space have made the difference between twisted incisors and impacted third molars in contrast to a dentition in which all of the teeth fit neatly in place?

In 1962 I examined the human skeletal collection at the Lowie Museum of Anthropology in Berkeley, California, and made note of all jaws in which the third molar was just coming into or near occlusion. There were 85 such specimens, all California Indians, and they could roughly be divided into three categories: those with little or no interproximal attrition (12 jaws), those with what might be called typical attrition, with a 3 or 4 mm reduction in each tooth row (64 jaws), and those which had noticeably greater attrition (9 jaws).

Fig. 1 shows one of the fairly typical lower dentitions—one in which each side is shortened by about 3 mm. Fig. 2 shows a mandible with greater, although not abnormal, attrition which shortened each tooth row by approximately 6 mm. This estimate is based in part on my reconstruction of the same dentition before interproximal attrition (Fig. 3), where each tooth is reconstructed with the contours of the contact faces based on the shapes of unworn teeth. In this reconstruction, the second molar is displaced so far distally that it overlaps half of the actual location of the third molar which was traced directly from the jaw. Without the interproximal attrition there is no way in which the third molar could have erupted properly. The third molar would have pushed other teeth out of place, or would have become impacted, or both.

The amount of interproximal wear that would be necessary to avoid crowding and to allow room for all erupting teeth varies greatly. Some individuals would require none. Many modern jaws would have sufficient room to prevent crowding if just 3 or 4 mm of mesiodistal tooth-row shortening occurred. Still other jaws might be crowded with as much as 6 or 8 mm shortening.

Orthodontists recognize the crowding problem and commonly solve it by extracting a premolar from each side of each jaw prior to orthodontic treatment (Begg 1954). Since some cases do not require this amount of mesiodistal shortening, a more natural method would be to remove a thin slice of enamel at the appropriate age, from as many contact faces as each individual case requires. This is now done by a few dentists and is called "discing" or "stripping."

Some notice has been taken of the fact that modern teeth, as well as jaws, are generally smaller than those of our ancestors. Even in post-Pleistocene times teeth in various areas have become somewhat smaller, apparently in response to cultural habits (Brace and Mahler 1971). This does not alter the fact that certain modern teeth would be even smaller yet if interproximal attrition were still occurring at a normal rate.
Fig. 1. Lower dentition of a California Indian with third molars near eruption (Lowie Museum Catalogue No. 12-6794). Slight wear is visible at the contact faces of most teeth. This might be called "typical" attrition where about 3 mm has been lost between the symphysis and the second molar. Photo by Eugene Prince.

Fig. 2. Lower dentition of a California Indian with third molars in the process of erupting (Lowie Museum Catalogue No. 12-5865). This individual shows more than average interproximal attrition, with each tooth row shortened by about 6 mm. Photo by Eugene Prince.
The actual cause of the wear that occurs between adjacent teeth is discussed by Wolpoff (1971), but his explanation requires some comment. He rightly notes that half of the cause lies in those forces also responsible for mesial drift. The other half lies in the source of relative motion between adjacent teeth, which Wolpoff states is lateral in direction. Similarly, Taylor (1963:121) states "...severe lateral strains in vigorous mastication" are the source of this wear, and he illustrates how far teeth may be moved laterally in the dry sockets of a skull.

These observations might be doubted simply because of the shape of the interproximal wear facets present in many cases. Viewed occlusally, the line of contact between two teeth often has an "S" shape, or approaches a "J" or "C" shape. Direct lateral motion will not produce these shapes.

Whatever the shapes of the contacts may be in the horizontal plane, they are perfectly straight vertically. This indicates that vertical motion of the teeth is the major source of interproximal attrition. In vigorous chewing, the resistance of the food is not likely to be exactly the same on each tooth. Accordingly, if the periodontal membrane would permit lateral motion, it would equally permit vertical compaction of teeth momentarily into their sockets. The degree to which teeth are driven vertically into their sockets is considerably greater than the amount of force likely to be exerted transversely.

It may also be noted that the amount of interproximal attrition is not the same on every contact surface of every tooth. Most notable is the excessive amount normally found on the mesial face of the first molar (see Figs. 1 and 2). The obvious reason for this is the greater length of time in which that molar has been in contact, mesially, with another tooth—first a deciduous molar, and then a permanent premolar. The second premolar is not worn as much on its distal face because it erupts five years later.

The length of time each tooth face has been in contact with another is not the whole measure of its likely interproximal attrition. The first
permanent incisors erupt shortly after the first molars, but show much less of this wear between them. This apparently is because the molar is closer to the fulcrum of the mandible and is used far more often for especially powerful biting, hence it is moved in its socket somewhat more. One can combine the time in use with proximity to the condyle for each tooth to obtain a relative measure of its expectable interproximal attrition.

In Table 1, eruption times used to calculate years in occlusion are based on data from Navajo Indians (Stegerda and Hill 1942) and on Pima Indians (Dahlberg and Menegaz-Bock 1958). Upper and lower eruption times are averaged, and each is expressed in terms of years of use prior to the appearance of the third molars. Relative biting power, hence usage, is calculated for each tooth according to the inverse of its distance from the condyle (see Fig. 4). Thus the incisors are taken as the standard with a biting power of 1; the second molar, which is half as far from the condyle, has a biting power of 2; and the others are in between. These numbers are based on the positions of the centers of each tooth projected to the midsagittal plane, and would be about the same if their positions were plotted along a line from the first incisors to one mandibular condyle.

Interproximal attrition on a given tooth facet results from movement of either of the teeth that contact there. The amount of wear on each facet results from the time that the tooth has been in use, multiplied by the biting

Table 1. Attrition Differential at Eruption of Third Molars

<table>
<thead>
<tr>
<th>tooth</th>
<th>years in occlusion</th>
<th>tooth itself</th>
<th>adjacent tooth</th>
<th>expected attrition factor</th>
</tr>
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<tr>
<td>I 1</td>
<td>10.7 x 1.0 x 1.0</td>
<td></td>
<td>10.7</td>
<td></td>
</tr>
<tr>
<td>Distal face</td>
<td>10.7 x 1.0 x 1.0</td>
<td></td>
<td>10.7</td>
<td></td>
</tr>
<tr>
<td>I 2</td>
<td>9.2 x 1.0 x 1.0</td>
<td>1.1</td>
<td>1.1</td>
<td>10.1</td>
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<tr>
<td>Distal face</td>
<td>9.2 x 1.0 x 1.1</td>
<td>1.1</td>
<td>1.1</td>
<td>10.1</td>
</tr>
<tr>
<td>C</td>
<td>7.1 x 1.1 x 1.0</td>
<td></td>
<td>7.8</td>
<td></td>
</tr>
<tr>
<td>Distal face</td>
<td>7.1 x 1.1 x 1.3</td>
<td></td>
<td>10.0</td>
<td></td>
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<td>15.0</td>
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<tr>
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<td>15.0</td>
<td>15.0</td>
<td></td>
</tr>
<tr>
<td>P 2</td>
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<td>13.1</td>
<td>17.1</td>
<td></td>
</tr>
<tr>
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<td>6.7 x 1.5 x 1.7</td>
<td>17.1</td>
<td>21.8</td>
<td></td>
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<tr>
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<td>11.6 x 1.7 x 1.5</td>
<td>28.6</td>
<td>28.6</td>
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<tr>
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<td>21.8</td>
<td>21.8</td>
<td></td>
</tr>
<tr>
<td>M 2</td>
<td>6.4 x 2.0 x 1.7</td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>
power factor of the tooth itself, times the biting power factor of the tooth which is wearing against that particular facet. Thus the wear on the mesial face of the first molar is a product of 11.6 (years) times 1.7 (power factor of M1) times 1.5 (power of P2). Each wearing facet of each tooth is similarly evaluated in Table 1.

The expectable interproximal attrition calculated for each tooth face shows rather good agreement with observed amounts of wear. The reader might check these figures against any well-worn dentitions at his disposal which are of the proper age.

The relative positions of the teeth are shown in Fig. 4, which also shows the expectable amount of attrition in the appropriate positions along the half dental arch. The actual loss of mesiodistal length in millimeters at each face in a moderately worn dentition was approximated by dividing each number in the table by 40. Thus the mesial face of the first incisor loses about .27 mm, while the mesial face of the first molar loses about .71 mm, etc., and the entire tooth row shortens by 4.67 mm in this instance. For a more heavily worn dentition the numbers could instead be divided by 30 which would give a total loss of 6.22 mm of which almost one millimeter is from the mesial face of the first molar alone.

The gradually increasing strength of bite as the child grows larger would have an effect which is not included here. Different shapes of contact faces on various teeth can also affect the linear rate of wear, a flat surface being the most slowly worn. These shapes change as interproximal attrition progresses, in each case moving from a point to a surface of gradually increasing size. This change in shape should about cancel out the effect of increasing jaw strength.

The greater occlusal surfaces in the posterior as compared with the anterior teeth should increase the probability of their encountering resistances and so being more pressed into their sockets. However, these larger teeth also have greater areas of periodontal membrane to distribute such forces, so the effect of larger crowns is probably cancelled out. There are still other factors which could affect this differential in wear but they are probably minor compared with those noted here.

To summarize, interproximal attrition in normal dentitions results automatically from heavy chewing which differentially moves the teeth in
their sockets. This same chewing also presses the teeth forward and maintains the adjacent contacts along the shortening tooth row. The amount of wear should be directly proportional to the length of time a tooth has been in service, and is greater toward the back of the mouth where the bite is stronger. With the use of soft foods in civilized man, this wear becomes minimal and the tooth row fails to shorten. This results in a dentition which is too big for the jaws to accommodate, and more often than not, impacted third molars.

The present study deals with the relation between heavy tooth wear and dental health. While many dentists still regard tooth wear as pathological, it has become increasingly evident that the opposite is more nearly true. This paper is just one instance in which the study of early skeletal material has shown how dentistry can be modified to better serve the living. An earlier version of this paper was submitted to a dental journal in 1963 only to be rejected, presumably because it advocated artificially induced interproximal attrition. It is gratifying to note that in the last few years this has nevertheless become an accepted practice in some places.

How many more medical applications might be developed from observations of preserved skeletal material? As long as properly documented museum collections are maintained and augmented, this opportunity will continue to exist.

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OBSIDIAN HYDRATION DATING IN CALIFORNIA

by Jonathon E. Ericson

OBSIDIAN & NATURAL GLASS SOURCES
INTRODUCTION

Recent refinement of the obsidian hydration dating technique in California provides a viable way of establishing a chronological framework. The obsidian hydration rates presented herein need to be evaluated in the field and the laboratory. Although the most advanced work to date involves research on the California obsidian hydration rates, further refinement of the technique will require careful evaluation of the proposed rates and the influences of particular environmental variables. Hopefully, this summary paper will provide an impetus to continue research on this important dating technique and serve as a review of research appearing in diverse publications.

The potential utility of obsidian hydration dating for archaeology has been discussed by Michels (1973). As a direct dating technique of lithic material, it has had special utility in determining the degree of stratigraphic mixing (Michels 1967) and relative dating of surface sites lacking diagnostic artifacts. As an inexpensive dating technique it provides the field research archaeologist an effective means of dating a large number of samples. Other aspects are discussed in greater detail by Michels (1973).

The impetus behind the present research has been the standing need for synchronous control of large regional exchange systems in which obsidian was an important exchange item (Ericson 1977a, 1977b). Too, the technique is particularly useful in studying the diachronic rates of production on obsidian quarry sites (Singer and Ericson 1977; Ericson 1977b). These studies demonstrate the importance of the diachronic production rates in providing an overview of regional exchange systems. A further refinement of obsidian hydration dating will lead to a better understanding of regional interaction and exchange in California prehistory. This research will definitely contribute to the field of general anthropology.

OBSIDIAN HYDRATION DATING: PRIOR RESEARCH

The obsidian hydration dating technique has been shown to be useful to archaeology (Katsui and Kondo 1965; Michels 1967, 1973; Meighan et al. 1968; Johnson 1969; Suzuki 1973; Bell 1977; Singer and Ericson 1977) and geology (Friedman 1968; Friedman and Peterson 1971; Friedman et al. 1973). Currently there can be problems in the accuracy of the technique due to uncontrolled variability of several important variables of the hydration process. The hydration phenomenon involves the development of a measurable birefringence stress layer through a sequence of processes which are not totally understood. Atmospheric water is chemically absorbed on the surface of the obsidian. This water diffuses into the interior of the obsidian as functions of time and temperature (Friedman and Smith 1960). It is also feasible on theoretical grounds (Ericson 1973a; Ericson, MacKenzie, and Berger 1976) that the water also reacts with the structure which causes the hydration rate to deviate from the diffusional model proposed by Friedman and Smith (1960) (Ericson 1975). This deviation, observed as a retardation of the rate as function of time, may have been observed for a proposed obsidian hydration rate for the American Southwest (Findlow et al 1975).

In the absence of a complete understanding of the hydration process and the variables controlling the rate of hydration, there has been a considerable debate over the mathematical form based on archaeological evidence (Clark 1961a, 1961b, 1964; Meighan et al 1968, 1970; Johnson 1969; Friedman and Smith 1960), the physical mechanism of the hydration process (Marshall 1961; Haller 1963;
Friedman, Long and Smith 1966; Ericson 1975; Ericson, MacKenzie, and Berger 1976), and the variables which influence the hydration rates (Friedman and Smith 1960; Aiello 1969; Ericson 1973, 1975; Kimberlin 1971; Ericson and Berger 1976; Kimberlin 1976; Friedman and Long 1976; Ambrose 1976). As originally formulated by Friedman and Smith (1960) the obsidian hydration dating technique relied on a general diffusion equation having two variables, namely time and temperature. To facilitate the application of the technique, broad temperature zones were established after the work of Chang (1957), within which a zonal hydration rate was to be used. Later, based on archaeological evidence, Clark (1961a, 1961b, 1964) and Meighan, Foote, and Aiello (1968) suggested that the proposed diffusion model did not fit the empirical hydration data. In support of their original thesis, Friedman et al. (1966) defended their diffused model of hydration with the results of a four-year induced hydration experiment. The impact of these findings was to suggest to researchers that tighter data control was definitely necessary in order to resolve the hydration problem. A summary discussion of subsequent regional studies has been presented in Ericson, MacKenzie and Berger (1976:39).

Even with increased geographical control, yet another form of variability was observed in hydration rate formation. Although Friedman and Smith (1960) did demonstrate hydration rate differences between trachytic and rhyolitic obsidian families, they did not suggest the degree of importance of chemical factors within each family of obsidians. As a result, a series of papers now suggests the importance of chemical composition in affecting hydration rates (Aiello 1969; Ericson 1969, 1973; Ericson and Berger 1976; Kimberlin 1971, 1976; Michals and Bebrich 1971; Morgenstein and Riley 1973, 1975; Suzuki 1973; Layton 1973; Ambrose 1976; Friedman and Long 1976).

In summary, prior research has continued to refine the obsidian hydration dating technique by determining and controlling variables of the hydration process which have been defined as time, temperature, and chemistry of the obsidian. We are not yet assured of the actual mathematical model which best fits the hydration process: the Friedman School suggests and supports Fick's Second Law of Diffusion and the Meighan School suggests an empiricist's approach to modelling the archaeological and hydration data. Fortunately, in a broader perspective of research and development, this debate has stimulated the continuation of research efforts to necessary in resolving complex problems.

OBSIDIAN HYDRATION RATES: RESEARCH DESIGN

The research design, published in 1974, outlines a strategy for defining source-specific empirical obsidian hydration rates (Ericson 1974). The first step in defining these rates was to locate and sample known obsidian sources in California, Western Nevada, and Southern Oregon. The details of the obsidian source program were recently published (Ericson, Hagan, and Chesterman 1976). Secondly, these source samples were chemically analysed by extensive instrumental neutron activation analysis and some X-ray fluorescence analysis.

In turn, the distinguishing characteristics of these data were defined through multivariate statistical analysis, resulting in the chemical characterization of each obsidian source. These two steps provided a means to accurately identify the original source of an obsidian artifact. A large sample of obsidian artifacts and associated radiocarbon dates were obtained through the courtesy of many individuals and several museums. Both types of samples were analysed by applying chemical characterization and radiocarbon dating analysis which formed source-specific data sets.
These data sets were analysed by linear regression to give the coefficients of a set of empirical equations, which have been previously used to describe the hydration process. The objectives of the research were two-fold: 1) to describe a series of source-specific empirical hydration rates for a number of sources used by aboriginal people, and 2) to define the best model of the hydration process. The details of the procedure and results are presented in the pages that follow.

THE SAMPLING OF OBSIDIAN SOURCES

Beginning in 1970, it was recognized that available obsidian source collections were insufficient for the intended research program. The primary objective of the field studies which were conducted was to gain controlled samples from each obsidian source for measurement of their trace element composition. This aspect of research was continued from September 1970 until August 1975.

The strategy employed in establishing the collections was to physically locate a given obsidian source through information provided in the literature or in the field by examining the alluvium and drainage of a volcanic field under study. Once located in the field, the structure of the obsidian source was sampled at multiple, designated locations, in order to eventually determine chemical homogeneity of the structure. The obsidian collections and field notes are accessioned both in the Obsidian Hydration Dating Laboratory, Department of Anthropology, and the Museum of Geology, Department of Geology, UCLA.

The report for the period 1970-1973, on the initial phase of research on the obsidian sources in California, Western Nevada, and Southern Oregon has been completed and appears elsewhere (Ericson, Hagan, and Chesterman 1976; Ericson 1977b, Appendix 1).

With the sampling completed, the immediate problem was to establish chemical criteria by which the source of an unknown obsidian artifact could be identified. This technical capability would allow the obsidian hydration data or other data, e.g., exchange data, to be categorized into source-specific groups. With the availability of the technological support and facilities at UCLA, the obsidian source samples were chemically characterized.

CHEMICAL CHARACTERIZATION OF SOURCES

Chemical characterization can be defined as a process or procedures which defines the chemical parameters by which a set of sources can be distinguished. The function of chemical characterization as applied to archaeology has been to accurately identify the origin or source of artifacts. There is an extensive literature on this subject which has been reviewed by Perlman, Asaro, and Michel (1972).

The analysis and chemical characterization of obsidian sources in California was not without precedent (Griffin, Gordus, and Wright 1969; Jack and Carmichael 1969; Stevenson, Stross, and Heizer 1971; Bowman, Asaro, and Perlman 1973; Jackson 1974; Jack 1976).

Although this information served as a foundation, the research undertaken by Ericson (1977b) examined the problem of chemical overlap of obsidian sources in California. At first, X-ray fluorescence analysis of the sources coupled with a ternary or tri-poled grouping of the elementary composition of zirconium, rubidium, and strontium was tried (after Jack and Carmichael 1969).
This promised to be non-destructive, the least toxic, most rapid, and least expensive means of approaching the problem. However, with the problem of chemical overlap in the numbers of sources involved, which increased the likelihood of statistical β-error, this original procedure was soon abandoned in favor of instrumental neutron activation analysis. It is important to say that the original X-ray fluorescence technique and recent modifications (Jackson 1974; Jack 1976) are useful but their power of discrimination impose definite limitations. Even attempts at characterization using short half-life radionuclides through Instrumental Neutron Activation Analysis did not always overcome the inherent problem of chemical overlap. In fact, this procedure required the geographical grouping of sources. It appears that INAA of long half-life radionuclides has the best promise of overcoming the overlap problem. With the adoption of multivariate statistical analysis in the form of stepwise discriminate analysis and long half-life radionuclide INAA, a new and powerful methodology for chemical characterization has been developed and tested (Ericson 1977b).

SOURCE-SPECIFIC HYDRATION RATES FOR CALIFORNIA SOURCES

The use of unit-level association between radiocarbon data and obsidian samples from regionally-diverse sites, sampled over a wide range of time, provided a means to control the data set. Although these three criteria provided the variable control and insured a sample over spacetime variation, these restrictions acted to limit the quantity of data which could have been utilized. In retrospect, the type of criteria used in formulating the original hydration rates (Friedman and Smith 1960) perhaps would have generated workable source-specific rates for California.

Notwithstanding the difficulties involved in assembling well-controlled samples, literally hundreds of obsidian artifacts and associated organic samples were processed for hydration rate determination. The procedures used in making these measurements merit description. The obsidian artifacts were chemically characterized as to source, and then hydration measurements were made. Organic samples were processed and their radiocarbon content was determined.

The artifacts were chemically characterized by either X-ray fluorescence, short, or long half-life radionuclide instrumental neutron activation analysis following the procedures described for the sources and stepwise discriminate analysis, using the “packaged” programs housed within the Obsidian Hydration Dating Laboratory, UCLA.

Obsidian hydration measurements and associated radiocarbon data grouped by source are presented by Ericson (1977b). These data were used to evaluate a set of mathematical descriptions of interrelationships between time of formation and the hydration layer thickness, called the source-specific empirical hydration rate equations. These formulae have been presented by researchers who have fit their data to either statistically-defined mathematical models or physical models, such as the diffusion model of Friedman and Smith (1960) or the auto-catalytic model of Ericson (1975). Here, the California data as a well-controlled data set was used to evaluate each of the obsidian hydration dating models already published in the literature. Such an evaluation was conducted by observing the degree of internal consistency or statistical fit of each model to the data for all sources. If a particular model is general, a convergence of fit towards one particular model can be observed as successively more controls are imposed on the data.

Five models were selected from the research literature on the hydration process:
1. Linear Rate Model -- Meighan et al. (1969).

2. California Rate Model -- The California rate model was proposed by Clark (1961a, 1961b, 1964).

3. Diffusion Rate Model -- The diffusion rate model was originally proposed by Friedman and Smith (1960).


5. Parabolic-Rate Model -- Findlow et al. (1975).

6. Square Root Rate Model -- Proposed here such that the exponents of all the models would vary from 0.5 to 3.0.

Linear regression analysis of the source-specific data was performed by the computer program BMDPlR (Jackson and Douglas 1975) using each of the above equations. The results of the regression are presented in Table 1.

Source-specificity is an important refinement for the obsidian hydration dating technique which is illustrated by the range of variation of the rates for any given mathematical model. The degree of fit for each case is demonstrated by the Pearson's regression correlation coefficients which are tabulated in Table 2. The diffusion model appears to be the best model in 5 of 14 cases.

The diffusion model can thus be maintained as the general hydration model as long as the obsidian hydration data is stratified by source. There is no doubt that source-specific rates increase the accuracy of the dating technique.

CONCLUSIONS

After extensive testing, it does appear that the original diffusion model (Friedman and Smith 1960) provides the best physical model and a general mathematical description of the hydration process for rhyolitic obsidian. However, this study has definitely demonstrated the need for source-specific hydration rates for increased accuracy of the dating technique. In addition, the measurement of the hydration (thermal) environment, soil pH and alkalinity will be a further improvement. These results suggest that the variety of descriptive models in the literature may be the result of inadequate control of temporal association, source specification, or hydration (thermal) environment.

For California, where many different obsidian sources are in the archaeological record, source specification has required the field identification of sources and the collection of samples which were characterized by both chemical and multivariate analysis. As a result, it is now possible on a routine basis to chemically characterize obsidian artifacts without knowing their regional provenience within California. This can be accomplished by using long half-life radionuclide instrumental neutron activation analysis of artifacts and the "packaged" program housed within the Obsidian Hydration Dating Laboratory, Department of Anthropology, UCLA.

As formulated herein, the application of the long half-life radionuclide INAA technique of chemical characterization provides the greatest precision and an objective means to characterize obsidian artifacts leading to the routinization of laboratory procedure. Nevertheless, depending upon the particular research strategy and the facilities available, two other procedures can be used for source identification. The rapid-scan X-ray fluorescence technique (Jack and Carmichael 1969) or the short half-life radionuclide INAA technique developed are useful if the number of sources, prehistorically utilized at a particu-
lar time or place, is known to be small or chemically distinctive.

Selection criteria in hydration rate determination have been shown to be very important. In this study, three sampling criteria were used in selecting the data, namely, unit-level association between artifacts and radiocarbon samples, from diverse sites, and over the widest time span. The empirical source-specific obsidian hydration dating rates (diffusion model) presented in Table 1 are now available for use and evaluation. Finally, as a result of the above research, a number of new radiocarbon dates are also available for California archaeologists (Ericson 1977b).

ACKNOWLEDGMENTS

I want to thank everyone who was involved directly or who was supportive in any way in the writing of the dissertation upon which this paper was based.

Research was conducted under the auspices, guidance, and support of the following UCLA Professors: Rainer Berger, Committee Chairman (Anthropology, Geophysics, and Geography), Wayne Dollase (Geology), Clement Meighan (Anthropology), Dwight Read (Anthropology), John D. MacKenzie (Engineering), John Wasson (Chemistry/Geochemistry), Howard Reiss (Chemistry), and T. Earle (Anthropology).

I would like to acknowledge receipt of samples for analysis from the following individuals—without their assistance the project would not have been possible: D. Herrod, R. Norrick, R.F. Heizer, D. Fredrickson, J.A. Bennyhoff, J. Michels, W. Clewlow, C. White, L. Nelson, W. Upson, and R.L. Orlins.

Special acknowledgment is due Jerry Kimberlin for his assistance in performing neutron activation analysis and Victoria Bennett, who conducted all obsidian hydration measurements.

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### TABLE 1. RATE CONSTANTS OF SOME OBSIDIAN HYDRATION MODELS

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### TABLE 2. DEGREE OF FIT OF THE HYDRATION RATE MODELS (PEARSON'S R)

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EDITORIAL

AFTER EXCAVATION--THEN WHAT?

by Gerald H. Grosso
Washington Archaeological Research Center

While the absolute necessity of extensive artifact conservation effort may have been brought into the spotlight because of the very large volume of perishable material recovered from the excavations at the Ozette Village Site (45-CA-24) on the Pacific Coast of Washington, the same rules apply at almost every archaeological site.

If perishable artifacts are to be saved, immediate, first-line conservation efforts must be made at the excavation site, followed by technical treatment at a laboratory staffed and equipped to handle the myriad problems involved in preventing extensive damage or disintegration of artifacts.

At the typical "wet sites" of the Pacific Northwest there are the obvious problems of waterlogged wood and other vegetable materials. Anyone who has seen an artifact excavated which appears to be an absolutely intact specimen of carved wood and then noted its shrunken, cracked, disintegrating state after a relatively short time of air drying can truly appreciate the need for conservation treatment.

But similar problems apply to perishables from dry sites. I recall a canoe which was excavated from a site in the desert of Southeastern Washington in the summer of 1964. It was in fine condition when excavated and there was a realization that its exposure to the hot, dry air might cause some problems so it was covered with burlap bags which were kept wet. I still can hear the dreadful snapping sound of the canoe wood as it separated into a thousand slats. Simple, effective measures could have been taken which would have stabilized that rare artifact.

Wooden artifacts aren't the only subjects which need conservation treatment. Bone objects may crack and check with similar speed. Leather goods quickly become as brittle as a maple leaf following a hard frost. Though perhaps not so obviously or immediately reactive to the changes in environment, serious damage can occur to objects made of glass, silver, iron, steel, lead, copper, brass, bronze, wool and paper unless appropriate conservation measures are taken.

There is no "cook-book" which gives sure remedial measures guaranteed to solve conservation problems with any or all of these materials. Of course, there are books, scientific papers and reports which describe treatments on each of the materials mentioned, as there are similar writings about treatments for medical problems. As in medicine, blind application of "shotgun" treatments results in the loss of some patients because of variations in subtle but key features of each individual. Perhaps even more important in archaeology are variations in the environments from which the artifacts are taken and into which they are placed. It should be easy to understand that an object taken from an acid soil would be in a different state than one taken from an alkaline soil. Therefore, even if they were made of identical
materials, they would need different treatments to achieve successful conservation. Consider, additionally, that they might be of materials of the same sort but of a different variety, making further differences to be taken into account.

Conservation must be considered an art as well as a science. Because there are so many similarities between medicine and conservation, the efforts of the conservator have come to be known as "a practice." This carries with it the implication that there is a continuing experimental character to the work and a tacit admission that we don't know all the answers.

A successful treatment for "sick glass," for instance, has been elusive. Since there doesn't seem to be a method of stabilizing unstable glass to prevent its spontaneous and continuing shedding of flakes, the best measure known at this time is to keep it in as stable an environment as possible. Meanwhile, experiments and research continue in an effort to find a cure for sick glass.

And there are divisions of opinion among conservators on the best way to treat artifacts of other categories. In many cases, these divergent views really represent different means of arriving at the same goal—successful stabilization of an artifact.

There even are some concepts on which most conservators agree. These include the basic idea that irreversible treatments be avoided (for instance, don't use Elmer's Glue-All); the concern for safety of persons working with conservation chemicals; and, most importantly, the belief that artifacts are rare, irreplaceable resources which need to be protected.

It is felt by many conservators that many archaeologists give inadequate consideration to the fate of the artifacts which they are excavating. Examination of a number of collections in the Pacific Northwest indicates that there is reason for such feeling.

The same thing applies to many ethnographic collections and there is a strong feeling that curators do not give sufficient attention to the care and protection of artifacts.

What can be done to remedy these problems?

Daugherty (1976: 5) suggests one approach:

Now that archaeologists know how to find these sites containing perishable artifacts, and are turning to such sites for their major excavation efforts, a full partnership with conservators is in order. The archaeologist needs to be instructed in field conservation procedures, and is dependent upon the conservator for the permanent conservation of his collections. In preparing his budgets, the archaeologist must become accustomed to supporting the conservation efforts of his colleagues.

Barkman (1977: 47) takes a different tack:

It is difficult to say why the science of conservation has not earlier followed the progress of this age of technology. It depends, of course, partly upon the conservators themselves but the main cause has been, and in several places still is, a different thing. In the museum domain the humanistic ideas and science have been prevailing for centuries and this, perhaps naturally, has resulted in conserva-
tion being a concealed activity in the museum work. The humanistic staff has disposed of the grant of money. They have formed a sort of federation, apparently with the object in view of taking care of each other's interests. They have considered only themselves to be scientists and have either done the conservation work personally in their spare time or left it to an underpaid worker. The result has been bad, in many cases, completely devastating to the objects. Many a dissatisfied conservator has speculated on the reason why the members of the humanistic staff have considered themselves to have a monopoly on all activities and resources, including all well paid positions.

Both Daugherty and Barkman touch on two key points: money and proprietary attitude. Part of the money available, and in many cases a significant part, must be allocated for staff, equipment and materials necessary for conservation of artifacts. The archaeologists and the curators must share authority and responsibility with the conservators. These things must be done in order to adequately insure the continued safety of the artifacts.

While it may be relatively easy to enumerate these key points and almost as easy to get agreement with them, it is very difficult to see them brought into practice.

Since most archaeological projects have scant funding, it becomes very difficult when preparing a budget to cut back on food, equipment, supplies and salaries for the archaeological crew in order to add a conservator, conservation supplies and chemicals. In practice, the more likely course has been to decide that the archaeologists can take care of most conservation and maybe someone will be called in if an unusual problem occurs.

Inglis (1977: 85) comments on this subject:

I think conservators should (be) an integral and equal part of any archaeological project. Archaeological conservators are certainly well trained in archaeological methods, history of technology, physical and chemical properties of the various materials you're dealing with, other than the straight conservation aspect...I think until we do treat the conservator as an integral and equal part of field crews, especially when dealing with wet sites, we are going to get these problems of materials disintegrating.

Similar approaches and changes in attitude among curators and others in charge of collections must be made in order to prevent the continuing decay and destruction of artifacts. There appears to be only one museum in the Northwest which has a conservation laboratory and employs conservators.

But the recognition of a problem, the funding, and the acceptance of the idea of change aren't enough in this case because there are very few archaeological conservators available in this part of the country, in this hemisphere, in the world.

Until training programs are developed to bring more conservators into practice, a real solution to the immediate problem will be achieved only with great difficulty.

Perhaps the immediate action would be to take advantage of those facilities and personnel which are available at the present time. This could be accomplished in several ways:
--Arrange short courses in conservation for archaeological field laboratory personnel to give them at least enough specialized information that they can cope with routine matters, can recognize problems, and know how to get help for solving them.

--Back up the expertise of the field laboratory personnel with recognition by supervisory staff that conservation of artifacts is important.

--Budget some of a project's funds for consultant fees and conservation chemicals. Until a heavily subsidized regional conservation laboratory is in existence, it seems unfair to expect professional conservators (or the projects which employ them) to donate time, materials and travel expenses so they can come to help you with your problem.

As a final consideration, perhaps a site should not be dug if there is no arrangement for funds, equipment and materials to conserve the artifacts.

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Barkman, L.G.

Daugherty, R.D.

Inglis, R.

This paper was initially presented at the 31st annual Northwest Anthropological Conference, Pullman, Washington, during April 1978.
IN RESPONSE TO...

The following replies have been received in response to "The Populating of Western North America" by Grover S. Krantz which appeared in the first issue of this series.

* * *

Grover Krantz' "The Populating of Western North America" is, as is usual with his ideas, simultaneously intelligent, stimulating, and controversial. I will first address the last quality.

The first difficulty is in his Rules of Movement. Rule number one is First Group In or the rule that "no hunting people can move into or through an area which is already occupied by another similar group." This rule is based on the notion that adjacent hunting groups would have about the same population density and the only advantage, territorial knowledge, would accrue to the group already in place. This seems a very questionable notion. It is quite possible that there might be a difference in, say, aggressiveness or some other military virtue which would upset the equilibrium. It may be that this is not the case, but it seems at least worth considering rather than simply and dogmatically asserting the contrary. In a similar vein, Krantz goes on to say (p. 5) "Successful migrations are made possible only when the movers have the military capacity or its equivalent to overcome any real or passive resistance. When two agriculturally based people engage in a dispute, the one with the greater resources will ordinarily prevail for obvious reasons." This sounds like an explanation of why the U.S. won the war in Viet Nam. If everything was as simple as Krantz seems to think we could easily dispense with all historical disciplines.

Krantz' second rule, that technological advantages may cause regular shifts of language boundaries seems less open to criticism although one might raise a question about the definition of advantage. Do we call it an advantage if and only if a language boundary moves?

The second major difficulty with Krantz' thesis is linguistical. He says (p. 1) "This is nevertheless not a linguistic study in any usual sense of the word. It is an attempt to account for the distributions of the generally accepted language phyla, but not by means of analysis of the languages themselves." This must mean, at the very least, that the language phyla exist. But if this is so, we must also accept the methods and results of historical linguistics and comparative philology since this is how language phyla are defined. This means we cannot simply accept or reject linguists' findings but must take the most careful of them to help us in our search for culture-historical "truth." This however is not what Krantz does, he accepts what he wants and rejects the rest. I will give three examples of this.

The first example is the Ritwan language group, consisting of Yurok and Wiyot at the mouth of the Klamath River and around Humboldt Bay respectively. Many years ago Sapir suggested Ritwan was related to Algonkian, the great language family of the Northeast. Sapir gave considerable evidence for this position (Sapir 1913) and more was given later by Haas (1958). Krantz, however, rejects this notion and cavalierly assigns them to Penutian on the basis of his rules. This is simply unacceptable.

It is conceivable that Krantz could turn out to be right about this, but if he is able to assign languages to phyla by fiat why does he bother with linguists' formulations in the first place? Why not make his own language groups de novo?
A second example is provided by his interpretation of Athapaskan. Krantz assumes a time depth of 11,000 years for this group rather than the 2,000 years the linguists allow. The discrepancy is apparently bothersome because he later goes to the trouble of inventing a theory of linguistic change which explains that "linguistic drift is directly proportional to the square root of the population." This reminds me of Mark Twain's theory concerning river systems, from which he concludes that "any person can see that seven hundred and forty-two years from now the Lower Mississippi will be only a mile and three quarters long, and Cairo and New Orleans will have joined their streets together." He then goes on to remark "There is something fascinating about science. One gets such wholesale returns in conjecture out of such a trifling investment of fact."

A third difficulty with the Krantz paper is its failure to recognize subgroups of the language phyla. It is as though he was dealing with Indo-European without recognizing Germanic, Celtic, Romance, etc. This leaves serious defects in his treatment of Penutian in particular. One of the problems with California Penutian has been the difficulty of determining the relationship of various subgroups. In particular it could not be determined whether Wintun was more closely related to Miwok or to Maidu. Now Kenneth Whistler (1977) has proposed a novel solution, i.e. that it should actually be grouped with Oregon Penutian. If this is correct then we will have to bring the Penutians into California at least twice (he thinks four times), once very recently. I would not think this would fit at all well into Krantz' rules but this doesn't disturb Krantz since he does not deal in subgroups at all.

Thus there seem to be a number of questionable aspects to this paper, but that does not mean that I disagree with it as a whole. I think it is true that a depopulation or at least a diminution of population in various places and various times does account for a good many of the migrations that must be invoked in order to explain language distributions. Krantz' notion that the central part of California was depopulated in the altithermal thus paving the way for Penutian immigration has a certain plausibility to it. The difficulty is that it is almost impossible to test because of the alluviation and other soil formation that has taken place since then. For example, during the recent (1976-1977) drought, the water in Lake Berryessa, a Coast Range reservoir, was lowered over 40 feet. This presented D.L. True and I with an opportunity to investigate the sides of a small valley after it had been under water for 20 years. We surveyed a stretch of several miles of this denuded land and discovered several hundred crude implements scattered almost everywhere. We also found three archaeological sites each with thousands of exposed tools and in one of these sites there was still some undisturbed midden. We have also investigated adjacent territory above the maximum pool elevation and have found only two or three prehistoric tools. This can only mean that the tools are everywhere but are covered by a soil layer. In valley bottoms, of course, they are covered by deep alluvium. Thus in order to test Krantz' notion we will be forced to strip the soil off the entire Central Valley.

On the whole I don't think one can accept Krantz' idea of a sea migration by Athapascans 11,000 years ago. Nevertheless, Krantz is right that the Pacific Athapaskan distribution is very difficult to explain otherwise. It would be difficult, for example, to postulate a once continuous distribution subsequently invaded at six or eight places. It is similarly difficult to imagine five or six tiny groups wandering here and there until they finally reach the Pacific. I would prefer to leave the question open.

Finally I must congratulate Krantz on his bold proposal. If nothing else it will provide innumerable hypotheses to test and will thus endear him to writers
of environmental impact reports and to their governmental auditors who seem to love tests of hypotheses above any living thing (unless it be design of research).

Martin A. Baumhoff
University of California, Davis

REFERENCES


* * *

This monograph presents an enormously speculative flight into an area ordinarily looked after by demographers and linguists. It is doubtful if enough physical evidence will ever be forthcoming to prove that Krantz' idea, essentially, that all, or most, western North American Indian groups assumed their present (ethnographic) locations about 11,000 years ago, is wrong. The kind of detail needed to support his "one time" migration theory, by the same token, is simply not available. Moreover, any new information referring to such concepts even as the Altithermal climatic period will probably always be open to differing interpretations by concerned scholars, hence Krantz will probably be able to draw some support from one or the other of these.

In California, Carbon 14 dates allegedly dating cultural material around 11,000 years ago have not yet permitted the drawing of relationships between these materials and any specific linguistic group. Archaeologists working with later dates, of which a fairly great number is available, are hard pressed to draw this relationship. In northwest California, which contains a number of factors crucial for Krantz' scheme, all of the Carbon 14 dates so far known are "late," and the situation still presents some confusion to archaeologists.

It is in the linguistic sphere that Krantz, in my view, has really gone out on a limb. So far as I am concerned Hoijer, Hymes, and Kroeber have submitted sufficient linguistic evidence pointing to an initial Athapaskan entry to California somewhere around 1,000 years ago that a date of 11,000 years ago simply seems out of the question. While some of Krantz' strictures against glotto-chronology may well be taken, it appears that his physical migration theory itself, without considering its linguistic aspects, is subject approximately to the same order of doubts. In addition, Krantz takes it upon himself, as it were, to doubt certain tentatively established relationships between languages (e.g. Yurok as Algonkian or Tshimshian as Penutian) on geographical grounds. This is certain to be an absolute anathema to linguists, but even to non-linguists like the present reviewer it will be difficult to accept that a Penutian language originally was sufficiently influenced by an Athapaskan language to produce one that seems, after careful consideration by authorities, to be related to Algonkian. Krantz should have allied himself with at least one reputable linguist willing to be quoted as in support of his linguistic notions.

While comparing migration patterns of human beings with those of other mammals may have something to recommend it in general terms, the presence of culture serves to make the whole Krantz picture appear fuzzy. Thus the introduction
of the leaching process to acorn meal production in California may have been tremendously significant in the spread of the custom of eating acorns, and this may have given rise to some unexpected human migrations. But "pine-nut eating" is hardly in the same category suggested by Krantz as underlying reasons for "population explosions" and seems a thin explanation or part-explanation for the movement described as the Numic (Shoshonean) irruption in the Great Basin some thousand or so years ago.

In sum, I cannot see that the Krantz theory is going to revolutionize our thinking about migrations of the first western North Americans. It may stimulate some further discussion among human geographers, but it is destined to create a fair amount of righteous rancor among numbers of linguists who take cognizance of it.

Albert B. Elsasser
Lowie Museum of Anthropology

* * *

For the past few years strategy games--sometimes called "war games"--have been very popular. For a modest investment one purchases a playing board printed with a map overlain with a hexagonal pattern, playing pieces, dice, and a set of rules for moving the pieces about the board. The objective is usually to recreate a historical battle situation. For the more imaginative there are games which require only the purchase of a rule book. The players invent their own playing boards and playing pieces. Krantz has taken the strategy game a step further. His playing board is Western North America; the playing pieces are groups of hunting-gathering peoples; the objective is to move the pieces, which represent linguistic groups, from a starting point in central Alberta to their recorded ethnographic location by following an explicit and rigorous set of rules. No random advantage determined by a roll of dice is permitted. This is a game of history rather than a game of strategy, but it is equally challenging--it could even be published to occupy the time and intellect of unemployed or otherwise idle academic anthropologists.

The end product of Krantz's game--the conclusions about linguistic relationships of Indians in Western North America--will probably create a hue and cry. Many of his notions counter an orthodox explanation based on different rules of playing the game of historical reconstruction. Critics will probably miss the point--the conclusions are consistent with the rules. If the game is to be challenged the rules must be criticised, not the conclusions. With respect to my own area of expertise I have some quibble with the conclusions; my criticism concerns the validity of a single rule. The 12,000 BP starting date is untenable. There is simply too much unequivocal evidence for human occupation in the New World which predates the magic time of the Bryan-Haynes opening of the "ice free corridor." These data cannot be dismissed out of hand. The game must be more complex and the players must account for ethnographic distributions of linguistic groups on the basis of an additional variable--a pre-12,000 BP population.

And then there is the possibility of another "scenario." How do we account for the fact that the New World seemingly "filled up" after 12,000 years ago but not before? Since there was an antecedent population, Krantz's game should have the board filled with players before 12,000 BP to the same degree that it is filled after that near mystical date. The archaeology indicates such was not the case. Perhaps those mid-Wisconsin age people were not playing by the same rules?

Well, a most interesting game. Shall we go another round, Grover?

Frank Leonhardy
University of Idaho
Krantz's paper, "The Populating of Western North America" is a fascinating work in the genre of anthropological fiction. Krantz uses the United States as his game board, human populations as pieces and languages as markers of population occupation. The game operates according to a set of movement rules, stated clearly and unambiguously.

If we view Krantz's work at this level of interpretation it is impeccable, enjoyable, and provides food for the imagination. I am virtually certain, though, that countless anthropologists and linguists who read Krantz's paper will think that Krantz intended his efforts to be considered a serious social scientific investigation. It is with this intent in mind that I offer the following comments.

If one aspect of the paper had to be singled out as most important it is Krantz's effort to outline the principles by which human populations move. Whether or not the principles are valid is unimportant. What Krantz has done is to hit head-on the habit of some anthropologists who move populations over the countryside willy-nilly. The contribution is not in what is substantively proposed but what is opposed with substance.

If the weakest aspect of the paper had to be isolated it is this; Krantz totally fails to deal with the process of group movement (what are the dynamics of extending a frontier?) and the process of communication (what are the dynamics of speech in a society?); the latter being referred to metaphorically as language.

Beyond the obvious strength and weakness of Krantz's paper itself, is an important epistemological error. Krantz's entire paper is based on an hypothesis which Krantz confuses as a truth. The confusion begins on page one with line one: "The classification of Indian languages into families and phyla is largely complete for western North America." This is a linguistic hypothesis, not a truth or a conclusion; the classification is an hypothesis. Krantz elaborates with 60 pages of explanation, or rather hypothesis, to account for an abstract hypothesis which classifies Amerindian languages of western North America. Krantz's overwhelming error is not that he strikes out but that he expected a fastball and was thrown a curve.

Linguistic methodology and theory is the idol of the tribe of anthropologists and linguists. In the 1950's anthropologists borrowed the idol to assist them in "objectifying" and making "more scientific" their subjectively laden discipline of cultural anthropology. Before long a cry for sanity was heard from a member of the tribe (Burling 1964) who asked whether anthropology's bastard son was "God's truth" or "hocus-pocus." Why do anthropologists persist in thinking that if they touch the idol they too will be holy?

Krantz failed to understand the intellectual history of the period that produced Veogelin and Veogelin's wall map of North American Indian languages. Perhaps the colors and sharp dividing lines on the map lulled Krantz to sleep or into a state of security which contact with the idol has so often done. Rather than creating still another idolatrous cult, Krantz would have been better off picking it up and carefully examining it.

Krantz is not entirely responsible for the situation. Anthropology grew as a holistic discipline; however, linguists have created a secret society, the collective representations of which are abstract theory and methodology, most often wholly unintelligible to outsiders. Before linguists take pot-shots at Krantz who makes frequent and serious mistakes with his linguistic assumptions, they should prepare a statement of "do's and don'ts" for noninitiates who plan on using products of linguistic investigation.

If anthropology is to continue as a holistic science then scholars within each of the subdisciplines must feel a responsibility toward communicating
clearly with their colleagues in complementary fields.

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Most reactions to my paper pull in various directions with a gratifying mixture of approval and disapproval. The requests for amplification of various points were such that they could not have been satisfied in a paper of that length. Further reviews would only add still more areas to be elaborated upon. I shall take this opportunity to discuss only the most frequent criticisms, and to offer a modification of the original reconstruction in a particular area.

There are at least three separate approaches that may be taken in reconstructing population prehistory. Archaeology should eventually give us the ultimate answers, but it is still a long way from providing a single, all-encompassing picture. Linguistics shows a set of relationships which are taken to indicate prehistoric connections and migrations. I have offered a third model, based on animal geography, which provides a rather specific pattern of movements and locations of group divisions. Each of these three models stands alone based on its own data and theory. Each may then be compared with the others. It is not necessary that the geographical model agrees with the other two any more than they must agree with it or with each other. Each can be equally criticized for not matching the conclusions of the other two.

My geographical groupings were created de novo by following postulated rules of movement. The fact that some 95% of the results compared favorable with the usual linguistic classification and distribution led to the hypothesis that geography was the major factor. That 5% of the results did not agree simply showed that one of these models contained some errors. I did not take this disagreement lightly—should my model prevail over the weight of linguistic opinion of many years standing? This contrast may not seem so overwhelming when it is remembered that I had the advantage of being fully familiar with both positions.

The major area of criticism of my paper concerns some small enclaves on the Pacific Coast. Initially I tried to derive these populations from the nearest linguistically acceptable sources: Athapaskans from the Nicolette of British Columbia, and Ritwan from the Kutenai of northern Idaho. With the advent of archery in this area, some 2,000 years ago, these peoples should have spilled out over the Columbia Plateau in a movement exactly analogous to the Apache irruption into the High Plains. (For a complete discussion of this topic see the original paper.) If this movement left remnants we might look for them on the far side of this plateau, perhaps near the Pacific Coast. But, after many attempts I could find no mechanism to describe such movements that would put these peoples in anything like their recent locations. I would be very interested to see any set of geographical rules which would place them correctly at that time.

To assume arbitrary differences in "aggressiveness or some other military virtue" for particular groups would be speculative in the extreme. My practice has been to use only those variables for which data is available. This procedure does not speculate on what people could have done, but merely tries to predict what they would have done under describable circumstances.

The concept of an 11,000 year old Athapaskan expansion down the Pacific Coast at first struck me as being just as absurd as it later struck my critics.
Yet this expansion, following the geographical rules, predicted the location of these people exactly as ethnographically recorded. Even the linguistic objections began to weaken when I later discovered that the Tahltans, far up the coast, just as predicted, were the ones linguistically closest to the Pacific Coast Athapas- kans. Only recently I have learned that one linguist has shown (unpublished) that Wakashan grammar can easily be derived from Athapaskan, thus virtually completing my postulated Athapaskan strip. The reported linguistic dating of the separation remains a seemingly insurmountable problem. If we knew for a fact that Athapaskan languages changed over time at a rate similar to that known for large populations of historic Indo-Europeans, when we would have a difficult choice to make between conflicting lines of evidence.

The Ritwan languages of northern California (Yurok and Wiyot) have been shown to be linguistically relatable to Algonkin, but the geographical model shows them to be an extension of Takelma Penutian from southwestern Oregon. If this one group is exempt from the rules of animal geography it is difficult to see why all others should have followed these rules so neatly. If the Ritwan situation invalidates the rules, then it would follow that the close agreement in all other cases is mere coincidence.

My first reaction on seeing this particular contradiction was to reexamine the meaning of the linguistic connection. Does a remote linguistic connection necessarily mean that the people have a corresponding ethnic phylogeny? Or is it possible that their similarities could instead follow from a combination of common retentions from more distant ancestors and from parallel innovations in more recent times? Again we are faced with a choice between conflicting approaches. (Biologists long ago learned to deal with this occasional contrast between morphological classification and actual phylogeny.) A similar case is Greenberg's proposed grouping of California Yuki with the Gulf languages of Louisiana (Merrit Ruhlen, personal communication). For that matter, Takelma exhibits all the basic characteristics of Indo-European. The linguistic techniques at this high level of classification appear to contain too large a random factor to be accepted automatically in all cases.

Throughout, I have apparently given the impression that I began by accepting the language phyla proposed by Sapir in 1929, and then simply threw out those parts that didn't fit my scheme. This was true initially, but ultimately it developed that I accepted no phyla per se, and all my groupings of peoples were based solely on the geographical model. The fascinating result was not that a few bizarre contrasts appeared, but rather that the agreement on the whole was so close. For example, had the Penutian group not already been proposed, I would have done so.

Subgroupings within each phylum received relatively little attention, mainly for obvious reasons of space. The Salish divisions, on two levels, were chosen for complete coverage because they so precisely matched the geographical predictions. They served to illustrate how these procedures can be extended down to the finest detail in any phylum. Utan subdivisions, in the case of the Numic expansion, were also discussed in order to account for the position of the dividing line in Oregon between Utan and Salish. This should now be expanded upon in an adjacent area.

In reconstructing the post-Altithermal distributions in southern California I originally allowed recent language boundaries too much influence on my thinking. A more careful check of rainfall patterns suggests a surviving pocket of Hokan speakers in the southwestern corner of the state during the Altithermal. These should later have repopulated a considerable adjacent area, thus blocking the Utan re-expansion well short of the Pacific Coast. On the other hand, the
Fig. 1. Altithermal populations are shown with lightly dotted lines. Their re-expansion follows the general trend of the arrows to their meetings along the solid lines. The Utan boundary is further marked by a row of large dots as in the maps of the original article.

Fig. 2. The last stages of the acorn-piñon expansions. The lightly dotted line in central California marks the Penutian (Yokuts) limit which was overrun to the south. The Takic expansion continues with the dotted line marking the earlier Hokan-Utan boundary.

distances involved would indicate a Utan occupation of part of the Central Valley of California following the Altithermal. The Penutians expanding out of the northern Sierra Nevadas would have met the Utans in the middle of the San Joaquin Valley if both groups expanded at the same rate (see Fig. 1).

With the acorn revolution beginning somewhere to the north, there should have been a series of southward shifts of various Penutian groups. The last of these would be the Yokuts to their recent boundary. The transfer of acorn processing would then be to the adjacent Kitanemuk who would in turn overrun their neighbors for a considerable distance to the south. The resulting Takic language group falls into two natural divisions, the Serrano who replaced a related Utan speech, and the Cupan which overlies a Hokan substratum (see Fig. 2).

The Takic spread may be viewed as the fourth arm of a technologically-based expansion, the other three arms being Numic speakers of the Great Basin with their shift from acorns to pine nuts. This also seems to account for the missing Hokans who ought to have survived the Altithermal in the southern Sierras. These people may have existed for a time, only to be overrun and incorporated by acorn eaters on the west and by piñon eaters on the east.

Additional modifications of my original model will certainly be made, hopefully in the same spirit as the above example. Further criticisms are expected because this is a very unusual approach to reconstructing North American prehistory.

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