MAMMOTH TRUMPET

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Center for the Study of the First Americans
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In the Footsteps of Ancient Masters

Stone was the only material the first Americans had, and they developed sophisticated technologies for shaping it into weapons and tools. Gene Titmus has spent a lifetime studying ancient techniques of working stone—and mastering the skills himself. His longtime colleague James Woods, director of the Herrett Center at the College of Southern Idaho, considers him one of the foremost flintknappers alive today. CSFA director Rob Bonnichsen thinks Titmus is *the* best. Some of his creations are replicas of specimens made by ancient cultures like Clovis and Folsom, faithful in the tiniest detail—they would make an ancient knapper jealous. Others, like this whimsical obsidian eccentric with antler haft, he makes to practice a particular technique or just for pleasure. See a few of his magnificent creations and read his story on page 4.

The Center for the Study of the First Americans fosters research and public interest in the Peopling of the Americas. The **Center,** an integral part of **Oregon State University,** promotes interdisciplinary scholarly dialogue among physical, biological and social scientists. The **Mammoth Trumpet,** news magazine of the **Center,** seeks to involve you in the late Pleistocene by reporting on developments in all pertinent sciences.

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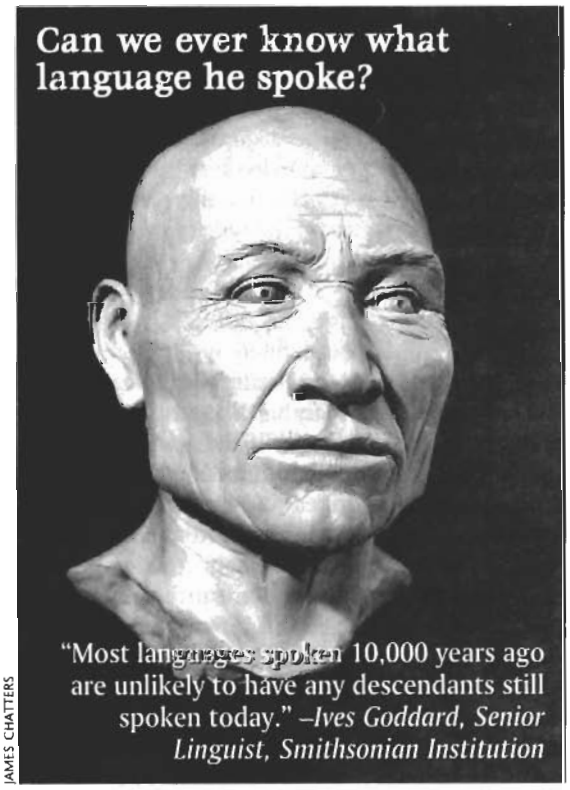
THE CASE OF KENNEWICK MAN:
Linguistic Evidence and Cultural Affiliation

Under the provisions of the Native American Grave Protection and Repatriation Act (NAGPRA), American Indians, individuals or groups, can take possession of cultural artifacts and human remains if they can establish "cultural affiliation" with the material they claim. Cultural affiliation is defined as "a relationship of shared group identity which can be *reasonably* traced historically or prehistorically between members of a modern-day Indian tribe and an identifiable earlier group." Such affiliation is established "when the preponderance of the evidence—based on geographic, kinship, biological, archeological, linguistic, folklore, oral tradition, historical evidence, or other information or expert opinion—*reasonably* leads to such a conclusion" (italics added).



JAMES CHATTERS

Given the nature of languages and the people who speak them, no reasonable relationship based on linguistic evidence can be established between living people and those of remote antiquity. This, however, has not prevented some from trying, most notably a coalition of Indian tribes in eastern Washington State that has claimed the 9,500-year-old skeleton of Kennewick Man, found along the banks of the Columbia River near where the tribes now live. Today litigation involving this claim, begun in 1996, is still in the courts.

It is in the interest of proper legal procedure, and to the benefit science and the public, to formulate protocols and rules of evidence to explain what science can reasonably determine about the linguistic past and how linguistic evidence may be used to establish cultural affiliation under NAGPRA. It is not the purpose of this article to define such guidelines and to provide such explanations, but only to show, by reference to claims made in the case of Kennewick Man, why they are necessary.

The linguistic landscape of the Plateau Culture

The location where the skeleton was discovered lies in an area of diverse native cultures that shares enough traits to define it as a distinct "culture area" within Native North America. The region, known as the Plateau Culture Area, extends from the Cascade Range in the west to the Rocky Mountains in the east, and from the Fraser River in British Columbia to a line that runs through the mid re-

INSIDE

gions of Idaho and Oregon. Since the Fraser and the Columbia Rivers drain this region, it was the scene of trade in pre-contact times and thus shows influences from neighboring culture areas. In *Handbook of American Indians* (vol. 12), Dale Kincade et al. report that two territorially extensive language families, Salishan and Sahaptian, characterized the Plateau at the time of contact with whites, as well as six lesser languages and one language isolate.

A family is a group of languages descended from a common form; for example, Spanish, Portuguese, French, Italian, and Romanian, all descended from Latin, form the Romance Family. A language isolate is related to no other known language. For example, Basque is an isolate standing alone in Europe, where languages of the Romance Family are spoken in the south, Germanic languages in the north, Celtic languages along the northwestern fringe, and languages of the Slavic Family in the east from the Arctic to the Mediterranean.

Seven languages called Interior Salish form a subgroup of the Salishan family and are distributed for the most part in the northern part of the Plateau Culture Area, with some extension into the southern part of the area where the Sahaptian languages predominate. Whereas Interior Salish consists of seven divergent languages, only two languages constitute the Sahaptian family, Sahaptin and Nez Perce, both of which are structurally very similar to one another.

Linguistic arguments for the coalition's claim of cultural affinity with Kennewick Man

Anthropologist Eugene Hunn makes the linguistic argument for the cultural affiliation of the tribal coalition of eastern Washington State that claims the remains of Kennewick Man. The tribes in the coalition are speakers of Sahaptin and Nez Perce; the exception is the Coleville Confederation, which is predominately Salishan in speech. By any estimate, the common ancestral language from which Nez Perce and Sahaptian emerged was first spoken no earlier than 5,000 years ago, probably much later than that. This, of course, does not reach back to the time of Kennewick Man.

Hunn thus invokes the hypothesis that the Sahaptian Family is a member of a group of families that form the phylum, or super-stock, of languages known as Penutian. Linguists who accept this classification have varying opinions about which languages belong to the super-stock. Joseph Greenberg makes the broadest claims; he includes in the Penutian phylum not only the languages of California, Oregon, and eastern Washington, but also languages spoken as far away as Mexico.

Greenberg also claims that Proto-Penutian, the common language from which supposedly sprang all modern Penutian languages, and several other super-stocks are descendants of what he calls Amerind. This, he presumes was the language of the Clovis People, the founder population of the Americas who are said to have crossed a land bridge from Siberia into America some 12,000 years ago. The argument for continuity of Penutian speakers in the Columbian Plateau, says Hunn, is supported by lexical evidence in the form of words for the flora and fauna of the Columbia Plateau. He also says that the geographical pattern of linguistic differentiation within Penutian (as defined by Greenberg) suggests that "the Pacific Northwest, including the Columbia Plateau, is the most likely region of initial dispersion."

Hunn concludes that "Kennewick Man spoke a Proto-Penutian language" nearly 10,000 years ago and is thus linguistically related to the Sahaptian speakers living there in historic times. His conclusion is directly contradicted by Ives Goddard, senior linguist at the Smithsonian Institution who says, "I doubt that there is a single 9,500-year-old human anywhere on the planet whose language can be established with the de-

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gree of confidence expressed in the quotation from Hunn. His assumptions beg the whole question. For example, given the general secular trends of linguistic expansion and replacement, most languages spoken 10,000 years ago are unlikely to have any descendants still spoken today."

Problems with the argument of continuity

Hunn is careful to couch his conclusions in terms like *possibly, controversial, speculate,* since some of the classifications he makes and methods he uses have either been rejected or are considered doubtful by most linguists. For example, Greenberg does not examine regular sound correspondences to establish genetic relationships among languages; instead, by a method he calls mass, or multilateral, comparisons, he looks "at many languages across a few words" rather than "at a few languages across many words." "No technique which relies solely on inspectional similarities," as Greenberg's does, says historical linguist Lyle Campbell, "has proven adequate for supporting [genetic] relationships." Indeed, says Campbell, Greenberg's methods have been "weighed and rejected by most mainstream linguists." The super-class Amerind, therefore, has no basis in demonstrable linguistic facts.

Also, the belief that the Clovis people, supposedly Amerind speakers, were the first humans to arrive in America has been steadily eroded by archaeological evidence over the years. A consensus is emerging that there were multiple entries, perhaps by boat along the coast, over a period of possibly 20,000 years or more. There is evidence that the physical characteristics of people who lived there 6,000 years ago were different from those of living Indian populations, thus suggesting that different populations may have migrated to the Americas before the ancestors of the present native population. This, in fact, is one reason why the nearly complete 9,500-year-old skeleton of Kennewick Man is so important for the study of American prehistory. The skeleton bears little resemblance to any American Indian population today. In fact, it more closely resembles the Ainu, an aboriginal pre-Mongoloid population of Northeast Asia, than any other living population.

Another problem, when proposing a theory of linguistic continuity on the Columbia Plateau, arises when Penutian is treated as a genetic unit. Roland Dixon and Albert Kroeber first suggested this genetic category for several languages of Native California. The name Penutian comes from the words for

continued on page 17

The implications for the Kennewick Man case reach far beyond studying the Kennewick Man's skeleton. We are concerned about the power of government agencies to restrict access to information and to ignore sound scientific methodology. The resolution of this case will affect scientists' freedom to study other skeletons, other sites, other traces of the past. If these scientists are successful, there will be a future for archeology and physical anthropology in this country. If not, the future could be bleak for a scientific understanding of the past. We created Friends of America's Past because we believe the evidence of America's prehistory must be preserved. We believe all citizens, now and in the future, have the right to learn and freely ask questions, especially about the ancient past from which we have all come. Friends of America's Past is working to raise public awareness that this basic right is being challenged.

The past is at risk

It's time to stand up for the right to learn about the past. We all share the past—no one owns it. Imagine if a few people could decide by whom, when, and how evidence from the past can be studied. Is this the legacy we want to leave to future generations?

It's happening now. The limited studies of the Kennewick Man skeleton conducted by government-hired scientists in 1999 are unverified by independent scientists. Their work is unreviewed by their peers and far from conclusive. The government has ignored other possible interpretations of the data and important information has not been gathered. Yet the government is moving forward with their decisions. All scientific access to the skeleton and the discovery site continues to be denied. The Kennewick Man case is the most visible example of arbitrary limits put on our right to learn about America's past. Other ancient evidence has been lost. Without action, there will be more limits, more losses.

JAMES CHATTERS

Taking action

Friends of America's Past was organized by people who believe the evidence of America's prehistory must be preserved. The past should be available to everyone now and in the future: to ask new questions, to discover, to learn.

The past will be lost unless we

- **defend our right to learn about the heritage of all Americans**
- **insist that good scientific methods be practiced, free of political motives**
- **bring needed clarity to NAGPRA and other laws that impact the study of the past**
- **prevent the loss of valuable scientific information due to biased interpretations of the law**
- **keep our country free for exploring and expressing all views and explanations of our past**

To learn more about this issue and to find out how you can help us, log on to www.friendsofpast.org


HERRETT CENTER

At Copan, Honduras, in 2001.

A Passion for Ancient Technology: Gene Titmus

STONE TOOLS are the most enduring clues to the lives of ancient peoples, for artifacts made of plant material rarely survive and even those of bone and ivory are subject to decay. Because stone artifacts represent the last, and perhaps the only, remnants of our earliest archaeological sites, the quest to understand the peopling of the Americas places extensive reliance on the practiced skills and insights of lithic technologists. Such a source of wisdom is Gene L. Titmus, a Jerome, Idaho, man with a lifelong passion for stone tools and ancient technologies.

For more than a half century—and strictly because he wants to—he has been studying and replicating stone tools from various parts of the world. In the process he has tried to think as ancient tool makers thought. Though Titmus can create a stunning replica of a Folsom or other Paleoamerican point, and can even haft it with pitch and sinew to a shaft, he can never be sure that he employed the exact technique that ancient people used. Still, from the research and experimentation of flintknappers of Titmus's expertise, archaeologists can interpret much about the stone artifacts they unearth.


HERRETT CENTER

Always popular with students, Titmus here is interviewed by Idaho State University student Earl Mitchell, ca. 1998.

Titmus became interested in archaeology about 60 years ago when as a small child he was shown the handiwork of Native Americans near his home in southern Idaho. "I lived in the Snake River Canyon's Shoshone Falls, at a well-known natural attraction," he says. "There are numerous archaeological sites at this location, and I was always fascinated with the stone tools I would find when out roaming the landscape." He never lost that interest, and after graduating from high school in Twin Falls and spending four years in the U.S. Air Force, he returned to the area. "My fascination was rekindled, and I became extremely interested in how stone tools were made."

Titmus quickly discovered that there was little information available on the manufacture of stone tools, so he proceeded to teach himself how to shape stone by pressure flaking. "Later, when I was in the process of learning percussion flaking, I met Don Crabtree." Thus Titmus began learning from the man widely considered the preeminent lithic technologist of the 20th century. Crabtree had perfected his own flintknapping skills in the early 1940s, and by the 1960s the Idaho-based researcher had earned an international reputation as a flintknapper and teacher.

"I met Don Crabtree through a mutual friend," Titmus explains. Because of their shared interest in flintknapping, the friend thought the two should get to know each other, and Titmus and Crabtree began what was to be a 20-year-long friendship that included working together on many projects involving the creation and analysis of stone tools. Only Crabtree's death in 1980 halted their collaboration.

"His enthusiasm for the study of lithics always inspired me," says Titmus. "Without his pioneering efforts I am not sure where we would be in the study of lithics." Titmus, however, has continued to do his best to carry on in the tradition, and he now possesses his own long record of teaching, field work, consulting, and demonstrating lithic techniques.

Are makers of fine stone tools especially gifted individuals? Can anybody learn?

"I'm not sure if this is a gift or simply just a matter of dedication, drive, goals, and a striving for a sense of accomplishment," says Titmus. "In my case the need to know how different stone artifacts were made in the past was almost an obsession." He believes that anyone with good hand-eye coordination and motor dexterity could, with dedication, learn to produce some of the more-difficult-to-create stone artifacts. "Dedication," however, means years of practice and study to Titmus.

He cites the painstaking research aimed at understanding the Folsom point: "Study about how it was made has been ongoing for over 40 years, and we still don't have the varmint by the tail." Folsom, he says, is one technology that will take still more years to understand, and even then "we may never be able to state that *this* is the way it was done."

Asked how he would go about creating the best stone tool for bringing down a huge Pleistocene beast such as a mammoth or a bison, Titmus demurs. It isn't a reasonable question, for, as he points out, Clovis and Folsom people possessed weapons systems tied to their hunting strategies, based on success, and perfected through trial and error. "We really don't have enough evidence about their weapon systems to determine exactly how they functioned," he says, adding that it is not even certain that these Ice Age peoples had atlatls, the spear-throwers that greatly magnify the force and range of a projectile. "So, without more knowledge I would not speculate on how I could improve on their system."

Titmus knows, of course, that he is a product of a very different culture. "My system would probably rely on a .30-06 with a scope for a long-distance kill."

Titmus says archaeology had "just been a hobby" for him until he retired several years ago "from the job that kept the wolf away from the door." Archaeology has always been something he did

Master of a Lost Art

HERRETT CENTER

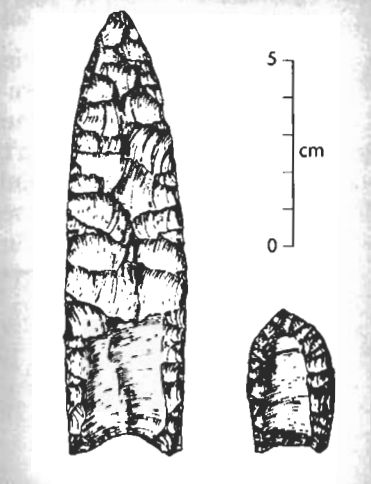
The object Gene is holding is a replica of a Maya eccentric. The original was manufactured of chert sometime during the late Classic Period, ca. A.D. 600–800. Gene's replica is made of obsidian. Maya eccentrics are generally found in burial offerings, or in dedicatory caches to commemorate new buildings. They are knapped using basic percussion techniques to make a large bifacial preform (the same basic technology used in North America to make bifaces), then using a combination of indirect percussion (using a punch) and pressure (using antler or bone flaking tools), the forms are given their final shape. These are often in odd, geometric forms, but a few are in the form of a deity with headdress and extended arms and legs, and often the deity's hand is holding some unidentifiable object. Many of the simpler eccentrics are made in the form of serpents, crescents, star forms, etc.

Anthropomorphic eccentrics, like the one Gene is holding, are much less common. There are only a handful of flintknappers who can replicate these large eccentrics. Gene is surely among the best. He has a forthcoming paper on this very topic in a volume being edited by Dr. Kenneth Hirth of Pennsylvania State University. The largest Maya eccentrics are from the site of Copan, Honduras. To see some spectacular examples, see *National Geographic*, September 1991, pp. 102–103!

It is worth noting that the margins on the eccentrics are very sharp, just like those on a Clovis point. There is no evidence they were ever used. Some speculate they were hafted to a wood staff and used in some sort of public ceremony, but this is conjecture. Some paintings on Maya pottery and stone stelae show Maya elite holding what may be eccentrics in their hands; some are on poles. A few have been found wrapped in bundles of cloth, painted blue.

–*James C. Woods*

Points with Pedigrees



Classic Clovis (*left*) and Folsom points. This Folsom point, according to Titmus, "was an isolated surface find in 1950 on Lake Channel, a basaltic coulee located about 15 miles southwest of American Falls, Idaho. The point exhibits basal and lateral margin dulling 3 cm up from the base. It appears to be near the end of its use life and may even have been discarded and replaced with a point with longer cutting margins. Since it appears to be approximately half its original length, it has been resharpened numerous times (perhaps because of dulling, or it may have broken at the distal end from impact and been resharpened). It is interesting to note that, when resharpening, the knapper maintained the small sharp, pointed distal end that helps initiate easier penetration into game."

Of the Clovis point, Titmus says, "It was part of an ethnological collection assembled between 1866 and 1869 by Edward S. Storrer, a civilian surgeon stationed at Fort Lapwai on the Nez Perce Indian Reservation, Idaho Territory. The collection was originally sent to the Army Medical Museum, and in 1869 the Smithsonian Museum accessioned it into their collections. It is not known if the point was recovered in northern Idaho. It may have been obtained from one of the Nez Perce Indians living on the reservation; since the Nez Perce made hunting and raiding forays onto the plains, the point may have been recovered there. The Clovis point, made from chert, appears to be as first produced, not resharpened. It has the typical spaced collateral flake scars seen on the Western Clovis, and the basal and lateral proximal margins have been dulled, evident in the straighter and smoother appearance of the first 3.5 cm of the proximal margins up from the base. Both faces have two channel flake scars. Multiple channel flake scars appear to have been used to create a flat-surfaced, wedge-shaped cross section at the proximal end to meet Clovis hafting requirements. This is more like a basal thinning procedure than the fluting procedure found on Folsom points. The point is close in overall dimensions to larger Clovis points recovered in Clovis caches—Fenn, Simon, Drake, and the like." (Titmus emphasizes that he hasn't personally examined this Clovis point; his comments are based on photos and drawings, and on an article by S. R. Potter and D. D. Aageson that appeared in 1974 in *Tebiwa*, the Journal of Idaho State University.)

Gene Titmus has an encyclopedic knowledge of lithic artifacts of the Americas. He admits he's "pretty good at cracking rocks."

simply for the love of it, yet he has never been anything other than professional about his work, and his résumé includes lengthy lists of archaeological field projects, technical publications, presentations, workshops, consultations, and demonstrations. Because he is not a professor, he is not shackled to any anxiety that his experienced opinions need to be backed by published references. Since 1984 Titmus has been a research associate in ancient technology at the College of Southern Idaho's Herrett Center for Arts and Science. In 1991 he added to that the co-directorship of the lithics laboratory for a research project that is affiliated with the University of California at Los Angeles, and that concerns Mayan cities in Guatemala. He's a past president and board member of the Idaho Archaeological Society and a life member of that organization's Snake River Chapter.

James C. Woods, his friend and colleague at the College of Southern Idaho, says Titmus has a special knack for demonstrating and teaching lithic techniques. He can explain what he is doing and why in a way that everyone can understand. "He is really good at working with students," Woods says. Very patient, he's the classic instructor who demonstrates, encourages, and explains that "this is going to take you a little while." He devotes a great amount of time to demonstrating and teaching lithic techniques in classroom, laboratory, and field situations.

Titmus believes that a student who is truly dedicated can learn these skills. "But," he cautions, "that student would probably have to dedicate himself to maintaining and learning new knapping skills over a long period of time."

Having worked with stone-tool traditions from different parts of the world, Titmus possesses a broad understanding of ancient technologies that probably is rare among flintknappers. He says he became involved with many differing technologies because little was known about them. "Their study entailed considerable time, research, and numerous failures to finally come to some resolution," he says. "And sometimes the resolution was not absolute—you could only say that 'I think, from my experiments, that *this* is the technology used to produce *this* type of stone artifact in the past.'"

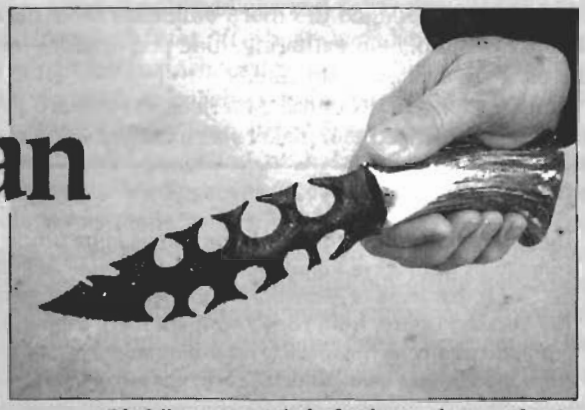
Now, Titmus adds, a dedicated student would have an advantage "because he could be taught what I know, and thus would not have to go through the experimental phases of different technologies—but he would still have to go through the learning phase."

In prehistoric societies, was flintknapping expertise vested in a few skilled specialists, or did the majority of people practice such skills?

"I can only speculate at an answer," he says. However, he notes that the production and use of stone tools and weapons surely was imbedded in the culture of ancient people. "It is difficult not to believe that from childhood on, each individual knew how to produce stone tools necessary for his own needs and survival. I would think that every male would know how to produce the stone projectile points needed to equip his weaponry." If one were separated from his group, for example, he would have had to be self-sufficient in that unforgiving environment in which they lived. He notes that there seems to have been a

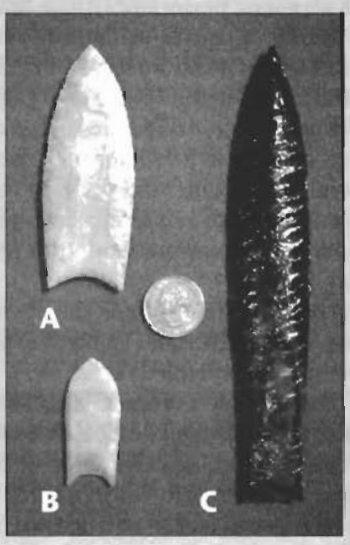
Showcase of a Master Craftsman

Here are just a few of the exquisite creations from the hand of Gene Titmus. Some are replicas of types known to every student of the peopling of the Americas, others he made to perfect his skill at a particular technique, and some he made for the pure joy of it.



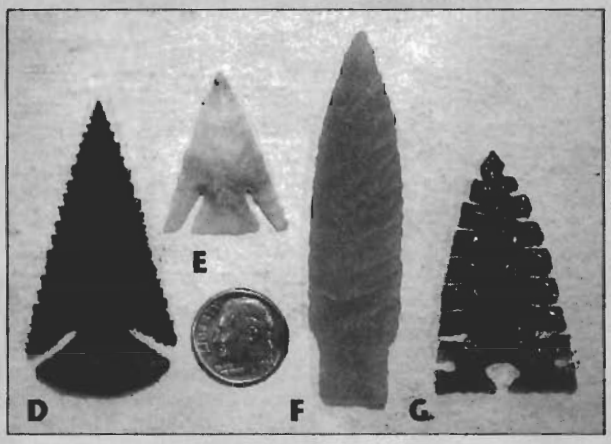
Obsidian eccentric hafted to a deer antler handle, made as an exercise in notching. Titmus calls this kind of exercise "freeform" (to create something different). ▲

◀ Chalcedony eccentric, not representative of any point type. Another freeform creation—"just practicing notching and pressure-flaking inside notches," according to Titmus.



Point **A**, made of chert, represents the Clovis ▶ point type. **B**, also chert, represents the Folsom point type. **C** doesn't represent any type of point; it is made of obsidian as an exercise in parallel oblique pressure flaking.

▼ Point **D**, chert, is similar to the Elko point type; point **E**, also of chert, is similar to the Rose Springs point type; both types are found in the Great Basin. Titmus made stemmed point **F** of quartzite to practice parallel oblique flaking. Point **G**, an obsidian eccentric with deep serrations and a basal notch, doesn't represent any type of point—"just fun to make," he says, "and to practice notching."



Obsidian sacrificial knife (*below, left*) is similar to those used in Central America (which were usually made of chert). Titmus made the obsidian point (*below, right*) as an exercise in removing a long channel flake, not to represent a type of point.

ALL: GENE TITMUS & JAMES WOODS

division of labor, in which case "women probably would have known how to produce the more utilitarian tools needed for food preparation, food gathering, hide preparation—scrapers, knives, awls, drills, and the like."

Titmus says some Central American groups are known to have had specialized tool makers who crafted certain types of the more formal stone tools such as obsidian pressure blades, large bifaces for axes, stemmed macro blades, and the like. "Although, from my experience in the Maya area, it appears that all the people knew how to make the less-formal tools such as scrapers, perforators, flake knives, and so on."

Our apology. . .

The suggestion that we feature Gene Titmus in an article came to us from Steve Kohntopp, who was himself featured in the December issue of ***Mammoth Trumpet*** ("Lithic Caches: The Puzzling Legacy from Early Knappers"). We expressed our gratitude poorly by misspelling his e-mail address. Steve's correct address is **swk@filertel.com,** not swk@filtertel.com

Being a man with 40 years' experience with flintknapping, he observes that it is possible that older individuals in prehistoric groups made more of the projectile points, for, as he notes, "the more you do it the better you become at it." Older men were likely more skilled at point production "and perhaps they didn't enjoy running around the landscape chasing game any more, and could trade their points for food. How's that for speculation?" he quips.

"Being serious, I think there was considerable cooperation and interaction of all types in these ancient groups—their lives depended on it."

Because of a decade-long association with RAINPEG (Regional Archaeological Investigations of the North Peten, Guatemala), Titmus is quite familiar with the lithic tools recovered in Mayan cities of the lowland tropical forest, particularly one known as Nakbe. Titmus and Woods, who is director of the Herrett Center for Arts and Science at the College of Southern Idaho, share the directorship of the RAINPEG lithics laboratory.

"Jim and I have been involved with excavations and experimental archaeology at Nakbe since 1991," says Titmus. "We also do the analysis of all the stone tools recovered from the many different excavations at the site, but this is done at a lab in Guatemala City. What Jim and I have been focused on in Nakbe is the limestone quarrying process—how the blocks used to build the temples were cut out of the limestone. We excavated a number of the ancient quarry pits and determined what stone tools were being used to cut the limestone, made similar stone tools, hafted them. Based on wear on the ancient tools, and with the help of Guatemalan workmen, we cut numerous limestone blocks out of the bedrock." He expects their report on the work will be published early in 2003.

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Titmus with an armful of hafted Scottsbluff point replicas, 2001.

Can a lithic specialist tell the probable age of a stone tool just by looking at it?

Not all stone tools, Titmus says, explaining that scrapers and other utilitarian tool types were similar through time and across geographical areas. Identification by typology works best for projectile points, knives and spear points. He lists criteria of typology—shape, flake scar patterns, type of notching, placement of notches, margin and basal abrasion or polish, base morphology, cross section, and sometimes size. "If a certain projectile point, knife, or spear point type has accepted dating, and if what you have been asked to identify has provenience, you can usually give the probable age."

However, he cautions that if the artifact has no provenience he would be hesitant to give an opinion about the age. "Some modern knappers are adept at making close facsimiles of many ancient point types."

Another problem of identification arises, says Titmus, because a point looks different when it is broken and then resharpened. "Some published point types are erroneous," says Titmus. One point type is sometimes classified under several types, when in reality "what you have is resharpening stages of the same point type."

Fluted points, typified by those made by highly skilled Clovis and Folsom flintknappers, are widely accepted—assuming

known provenience—as proof of a known antiquity. Titmus has extensive knowledge of both.

What makes Clovis projectiles unique? "Clovis points were in almost all instances made from cherts," Titmus replies, "even in the West where obsidian was available." Chert was more resistant to breakage," he explains. "In cross section the points were somewhat thick and biconvex, a break-resistant construction." As a result, he says, many resharpened points have been recovered. "The basal third of the length of the point was usually narrower than at mid section; this provided space for wrapping cordage or sinew to secure the point to the haft medium and not exceed the maximum mid-section width of the point." The shape, he notes, eliminated drag as the point penetrated an animal. "To help prevent breakage in the haft area, Clovis knappers abraded or polished the basal margin and usually about one third of the basal lateral margin of each point." If the point broke, he believes, "they wanted it to be outside the haft area, thus saving the haft medium from breakage." Titmus says there remains much controversy on exactly how Clovis people hafted their points.

HERRETT CENTER

Titmus studies an eccentric cache at the National Museum of Archaeology and Ethnology in Guatemala City, 1998.

Regarding those hallmark Clovis flutes, he says that the removal of the channel flakes formed a V-shaped cross section at the point base. "When fitted to a corresponding cut in the haft medium and bound with cordage or sinew, it formed a union that could withstand considerable impact force." Titmus believes this part of the Clovis weapon system was durable and break-resistant.

Folsom knappers also may have opted for durability over the sharpness of obsidian. Titmus notes that though the Folsom tradition occurred mainly east of the Continental Divide, where there was little access to obsidian, a few Folsom points have been found in Idaho, where there are a number of obsidian sources. Still, a majority of those are made of chert. (Titmus is aware that his observation differs from some published reports. He bases his statement on personal knowledge.)

As is true for Clovis points, the method and media used for hafting Folsom points remain somewhat uncertain. "Many years ago Don Crabtree proposed a hafting method for the Folsom point based on its attributes," says Titmus, noting that the Folsom point usually had channel flakes removed the full length and from both faces. "It is boat-shaped in outline and has abraded or polished basal lateral margins. The basal margin is less often abraded or polished, for whatever clue that provides." Crabtree's theory for hafting, he says, assumed wood as the haft medium and suggested that because the removal of the channel flakes provided grooves up both faces of the point, the haft medium was made to fit snugly into the grooves on the faces and was positioned so only a small portion of the distal end projected out of the haft while leaving the lateral cutting margins slightly exposed outside the haft medium. That medium, he says, may have been a foreshaft, a dart shaft, or a spear shaft; "we don't know which for sure." However, Titmus does see a real advantage in the boat shape of the Folsom point, which he is sure provided the same benefit as with the Clovis. "The Folsom point is actually extremely durable when it is encased in its haft medium." Because little stone is exposed to break, many resharpened points have been recovered. For persons interested in knowing more about a possible hafting system for the Folsom point, Titmus recommends a paper by Stan Ahler and Phil Geib (see "Suggested Readings") that describes an elaboration of Crabtree's system.

To the casual observer, it may seem that Paleoamerican stone tools are more beautifully made than those left by later peoples. Titmus sees some grounds for that premise. "I think the oldest stone tools were constructed to be durable and efficient," he says, "so more time and effort were put into their construction."

–Don Alan Hall

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Suggested Readings

Ahler, Stan, and Phil Geib 2000 Why Flute? Folsom Point Design and Adaption. *Journal of Archaeological Science* 27:799 820.

Titmus, Gene L., and James C. Woods 1991 Fluted Points from the Snake River Plain. In *Clovis: Origins and Adaptation,* edited by R. Bonnichsen and K.L. Turnmire, pp. 119–131. Center for the Study of the First Americans, Corvallis, OR.

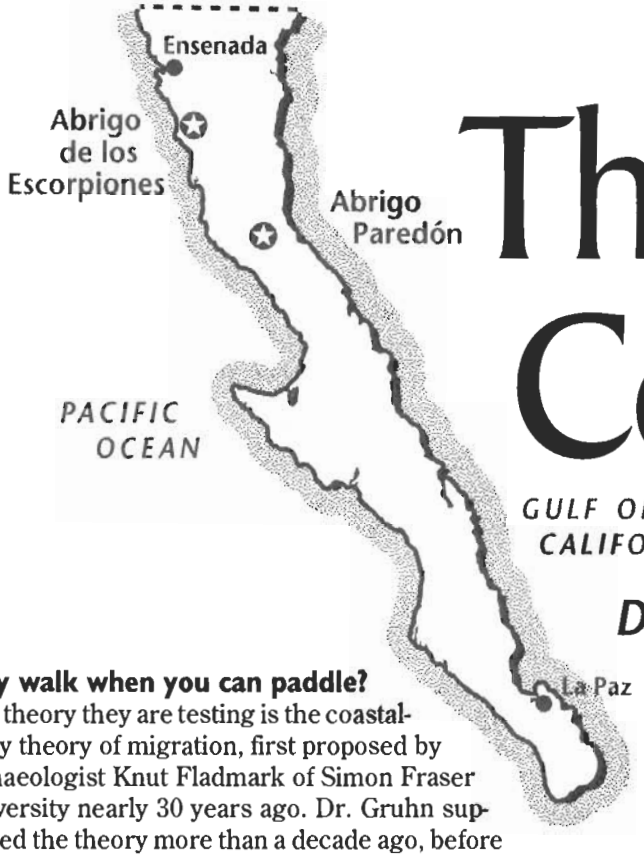
Titmus, Gene L., and James C. Woods 1991 A Closer Look at Margin "Grinding" on Clovis and Folsom Points. *Journal of California and Great Basin Anthropology* 13(2)194–203.

Plew, Mark G., James C. Woods, and Max G. Pavesic (editors) 1985 *Stone Tool Analysis: Essays in Honor of Don E. Crabtree.* University of New Mexico Press, Albuquerque, NM.

The Baja Connection

Did the first Americans make the crossing from Asia by boat? If so, they may have stopped here.

IT ISN'T AN EASY DRIVE from Alberta to Baja California, but Ruth Gruhn and Alan Bryan, archaeologists from the University of Alberta (and husband and wife), have been making the journey since 1991. The barren desert peninsula is the spot they chose to test the theory that the earliest settlers of the Americas traveled by boat, not on foot. Their excavations at two rockshelters have yielded tantalizing results. They have found evidence of human occupation at the early Holocene—about 9000 RCYBP (10,500 calendar years ago)—and they have hopes of pushing dates back even further.



Why walk when you can paddle?

The theory they are testing is the coastal-entry theory of migration, first proposed by archaeologist Knut Fladmark of Simon Fraser University nearly 30 years ago. Dr. Gruhn supported the theory more than a decade ago, before the collapse of the Clovis-First model—and even made a convert of Dr. Bryan. In recent years increasing numbers of North American archaeologists have become supporters of the theory.

The coastal-entry model cuts through complicated theories contrived to explain how humans crossed the Bering land bridge on foot, how they found a route through the Cordilleran and Laurentide Ice Sheets to the temperate areas of North America, and what they found to eat during their journey. Even enthusiastic Clovis-First advocates admit it wasn't an easy journey, and the timing was crucial, since their model depends on an Ice-free Corridor between the glaciers at the time of the supposed trek.

Why couldn't the first Asians have made the trip by boat instead? Dr. Fladmark contends that the trip would have been possible anytime during the last 60,000 years. Smithsonian archaeologist Dennis Stanford points out that "everyone knows boats have been around for 50,000 years" (**MT** 17-1, "Immigrants from the Other Side?"). It needn't have been an unremittingly arduous journey, since the coast along the Pacific Northwest had ice-free pockets that could have provided relief for southbound voyagers. And instead of relying on megamammals for food (that image of the Paleoamerican hunter with spear stalking woolly mammoths is a hard one to shake loose), the colonizers could have been conditioned to subsisting on bounty furnished by the sea—mollusks, fish, and the birds and mammals that fed on them.

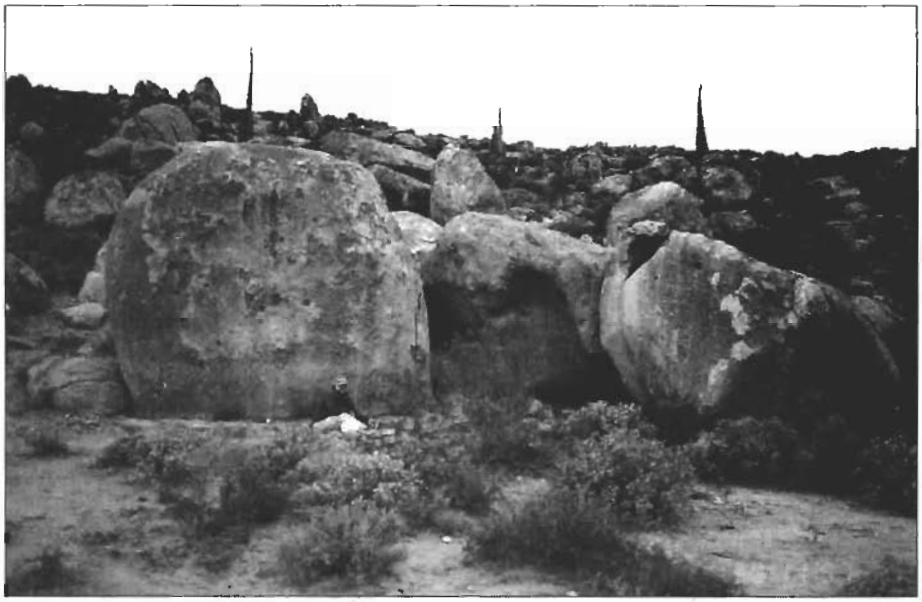
The coastal-entry theory has an especially attractive advantage over models that have humans traversing the continent—and the entire hemisphere—on foot: speed. Paul Martin's prehistoric-overkill hypothesis, for example, contends that humans made the crossing from Beringia about 12,000 RCYBP (14,000 calendar years ago), then developed sophisticated stone-tool technology that made possible rapid population growth. Martin believes that, thanks to their efficient hunting tools, the first Americans reached the tip of South America as early as 1,000 years later. For Fladmark's boat people, that's a snail's pace. "Even primitive boats," he submits, "could traverse the entire Pacific coast of North and South America in less than 10–15 years."

Central and South American sites can't be ignored

"It is very clear that South America," Gruhn insists, "in all the major environmental areas, even the tropical forest and southern Patagonia, was already settled by Clovis time in North America." She points to Monte Verde, the site in Chile excavated by University of Kentucky archaeologist Tom Dillehay, which dates to 12,500 RCYBP, or about 14,700 CALYBP. (The validity of the Monte Verde finding is still contested by some archaeologists.)

Gruhn names other sites—Taima-Taima in Venezuela (which she excavated with Bryan), with a date of 13,000 RCYBP, or about 15,500 CALYBP; and several sites in Patagonia with

dates around 12,000 RCYBP, or about 14,000 CALYBP. She speaks with authority. In 1969 and 1970 Gruhn and Bryan drove their brand-new Land Rover through Central America, then hopped a ship to Venezuela and drove to Patagonia and Brazil. They visited just about every early site known at the time, making connections and laying the groundwork for future research.



ALAN BRYAN

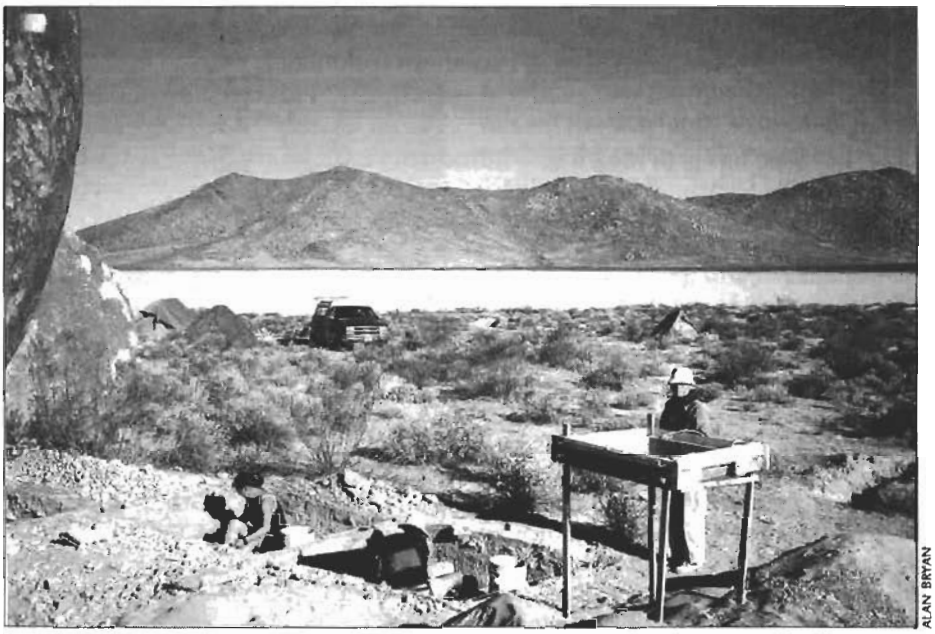
◀ **Abrigo Paredón is the space defined by three enormous boulders. The largest dwarfs volunteer Jim Wilson.**

For a good overview of South American sites that show promise of expanding our knowledge of the peopling of the Americas, Gruhn recommends *The First South Americans* by Danièle Lavallée, whom Gruhn met at a conference in Belgium. Although Lavallée has explored sites in Peru, that isn't the topic of her book, nor is it her strength. "She is European," Gruhn explains, "and has a different perspective on American prehistory."

Why did Gruhn and Bryan choose Baja California as the place to test the coastal-entry theory? Glaciation during the Pleistocene produced great changes in sea level, down 130 m (450 ft) at the Last Glacial Maximum. Consequently, the Pacific Northwest as far south as Washington experienced radical changes in its coastline. Any evidence of early travelers would likely have been either submerged by elevated waters or obliterated by shifting and melting glaciers. (Archaeologist Daryl Fedje retrieved a stone tool from the ocean bed, 175 ft down, while dredging a riverbank 11,500 calendar years old in the Queen Charlotte Islands of Canada. It's certain he would be the first, though, to declare the working conditions less than ideal.)

Baja California is a good fit. The peninsula is remote and sparsely populated. Because the ocean floor drops off sharply, changes in sea level had little effect on the contours of its coastline; the shore today looks quite the same as it did in the Pleistocene. Other scientists have made cursory explorations of Baja California since the 1950s and found intriguing artifacts. In 1952 Aschmann reported the discovery of fluted points in the northern part of Baja California Sur, and in 1957 Brigham Arnold found large bifaces on old lake beaches that he suspected might date to the Pleistocene. Don Tuohy, who had made several trips to the interior in the 1950s, accompanied Bryan on an exploratory tour of the entire peninsula in 1991. In 1997 Gruhn and Bryan received a permit from INAH (Instituto Nacional de Antropología e Historia, pronounced *EE*-nah, the Mexican national archaeological agency) and, with graduate student Loren Davis, launched a geoarchaeological study of the Laguna Seca Chapala basin, a dry lake bed (*playa*). The pilot project led to the discovery of Abrigo Paredón, the first of two rockshelters, and work in Baja has continued ever since.

Their work is highly focused. Previous finds were surface-collected, Gruhn notes, and therefore lack provenience and dating. What is more, the discovery of Clovis fluted points merits only passing interest. If their theory holds true, they expect to find evidence of a culture that predates Clovis, that of "generalists who had long exploited the productive Pacific coastal ecosystems with a relatively simple lithic technology." The earliest colonizers would probably have supplemented their tool kits with imple-



ALAN BRYAN

▼ **Gruhn (standing) and U.S. and Canadian student volunteers work at Abrigo Paredón in 1999. That's Laguna Seca Chapala in the background.**

ments made of shell and wood; like boats of animal skin and wood, however, these objects are unlikely to have survived over the millennia.

Abrigo Paredón

The first rockshelter Gruhn and Bryan explored isn't much to look at, just three enormous boulders at the foot of a slope overlooking Laguna Seca Chapala 100 m (330 ft) off to the northwest and 3.5 m (11½ ft) below. A small alcove could shelter one or two persons, "but it leaks," Gruhn notes wryly. Its best feature is shade, a precious commodity in the desert. Campers using it today have made a stone-lined firepit and strewn their garbage about.

Early people also welcomed relief from the sun. Excavations started in 1997 and continued in 1999 reveal that the rockshelter served as a flintknapping station. The entire basin is granitic, Gruhn explains. Weather exfoliates the boulders, crazing and pulverizing their surface. "In some places," she notes, "it looks as if someone backed up a truck and dumped a load of gravel." About 10 or 20 minutes' walk from the rockshelter are a series of felsite dikes that traverse the granite bedrock. Felsite is a very fine toolstone, fine grained and quite flakable. Knappers collected felsite blanks at the dikes, then brought them back to the rockshelter. In a comfortable setting they worked the blanks into smaller, thinner bifacial preforms, then into projectile points and knives.

Excavations carried to a depth of 55 cm (22 inches) below surface uncovered the products of long-term occupation: many foliate (leaf-shaped) projectile points and preforms, and "tons" of knapping flakes—about 25,000. Also found were thick scraper planes and humpbacked-core scrapers. A type common in the region, they have flat platform faces and steeply retouched peripheries, sometimes with noses or sharp graver spurs. Gruhn and Bryan suspect they were used to work wood or to process agave for fiber.

Abrigo Paredón resists precise dating. The artifact-bearing stratum is brown sandy silt. The upper 20–25 cm (8–10 inches) has been extensively disturbed by burrowing rodents and insects; the texture of the silt suggests it was deposited by the wind and by slopewash, and high winds and heavy rains have likely scoured and eroded the site. Stratification is therefore absent, but charcoal samples from 20–50 cm (8–20 inches) date between 6800 ± 580 and 9070 ± 60 RCYBP. Sand and silt apparently began accumulating in the early Holocene.

Among the few animal remains found in the brown sandy silt at Abrigo Paredón are bones, mostly of hares, but also of large grazing animals like deer, evidence that Chapala once held enough water to support wildlife. "Certainly there were pluvial lakes in what is now desert in Baja," Gruhn remarks. She knows of several others besides Chapala. "Lakes, or even marshes, would have attracted wildlife and humans to exploit it in the late Pleistocene and early Holocene."

In recent times, Baja California has shown a tendency to revert to a wetter state. A few months before Gruhn and Bryan arrived for the 1997 season at Abrigo Paredón, a hurricane had crossed the peninsula and deposited about 8 inches of water in Chapala. "Normally we drove right across it to set up camp," Gruhn reflects. "Oh no, this time we had to work our way around it." Storms in November and December blew down tents and made mud so deep one of their vehicles got stuck. To live or even work in this land, you have to pay a price.

Abrigo de los Escorpiones

Excavations at Abrigo Paredón found sterile cemented gravel 55 cm (22 inches) below surface, and a backhoe trench 4 m away hit sterile cemented sediments at 1 m (3¼ ft). Convinced that Abrigo Paredón had no more stories to tell them, Gruhn and Bryan moved to the Pacific coast. They led a crew from the University of Alberta that started digging in 2000 at Abrigo de los Escorpiones, a rockshelter formed by the high overhang of a volcanic outcrop about 100 m (330 ft) from the rocky Pacific shore.

Unlike Abrigo Paredón, which played out when only 2 ft deep, Abrigo de los

Escorpiones didn't get exciting until they reached 6 ft deep!

The sheltered area of Abrigo de los Escorpiones, about 1–3 m wide (3.3–10 ft), extends 35 m (115 ft) and slopes down to the west (toward the ocean) and south. Most of the sheltered area surface is rock rubble except for the northwest quarter, which is a shell midden. Shell fragments are exposed on the surface far down the slope. The excavations in 2000 were made at the northwest end of the rockshelter through the midden deposit. One test pit was dug in increments of 10 cm (4 inches) to a depth of 5.3 m (17½ ft). Although there was at least another 2 m (6½ ft) of rock rubble overlying bedrock, no more undistrubed cultural materials were found, and the excavations were filled with hay bales and covered with dirt in anticipation of resuming work in 2001.

Vandals visited the site, however, and disturbed the upper 6 ft of the excavations. The mischief-makers unwittingly did Gruhn and Bryan a favor, because in digging out to the north to stabilize the walls of the test pits, they found that the rockshelter opened up—the sheltered area expands toward the north under the overhang. Excavations in 2001 reveal that Abrigo de los Escorpiones, a much larger rockshelter than was first apparent, has been completely filled by a shell midden that overlies sterile silt and heavy rubble.

The shell midden has three distinct stratigraphic zones. The uppermost, to a depth of 2.8 m (9¼ ft), consists of mottled brown silt containing shells (mussel, abalone, and marine snails) and fish bones. Since the area lies directly under a raptor's perch, there are also bones of small mammals and birds that were the bird's prey; identifying them will inform us about the paleoenvironment. The uppermost zone also yielded many lithic artifacts including large unmodified flakes (most common), flaked cobbles, cores, choppers, hammerstones, manos, milling stones, and retouched or utilized flakes.

The middle zone is compact black/brown ashy silt about 1.5 m (5 ft) thick that slopes down to the north and west under the overhang into an area not yet excavated. Distributed throughout this zone are the same kinds of shells found in the uppermost zone, besides clams, giant chitons, and limpets in the lower levels. Besides lithic artifacts like those in the uppermost zone, Gruhn and Bryan also found several large lanceolate projectile points and substantial flaking debitage. Samples from the middle zone were dated at 6340 ± 100 RCYBP at a depth of about 2 m (6½ ft), and 8040 ± 70 RCYBP at a depth of about 3.5 m (11½ ft).

The lowest midden zone, brown loamy silt with rubble about 1 m (3¼ ft) thick,

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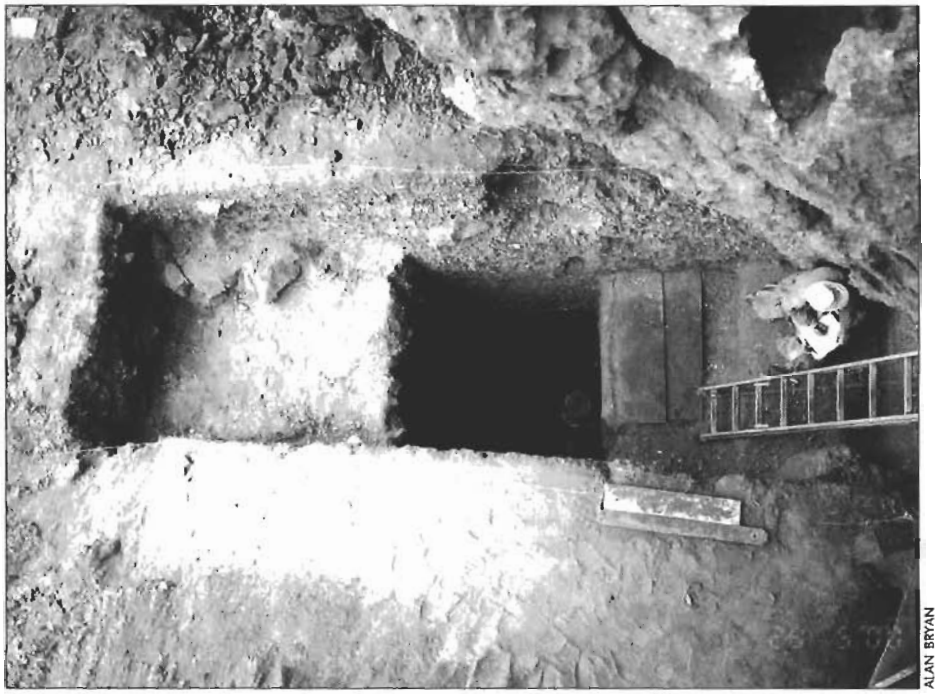

ALAN BRYAN

▲Abrigo de los Escorpiones. The volcanic outcrop forms an overhang; much of the recess underneath in the northwest corner (marked by the test pit and ladder) has been filled by vast deposits of shells. By excavating back under the overhang, Gruhn and Bryan hope to find evidence of Pleistocene occupation.

◀ Working at Abrigo de los Escorpiones in 2000, the first season. Ruth Gruhn is doing a profile in the test pit, about 16 ft deep at the time. (Just visible is a bucket at the bottom.)

Article Questioning Radiocarbon-dating Accuracy Draws Fire from Scientists

In "Terrestrial Evidence of a Nuclear Catastrophe in Paleoindian Times" (MT 16-2), authors Richard Firestone and William Topping theorized that C-14 levels in carboniferous materials, by which the age of organisms and artifacts is measured, are grossly misleading, the result of neutron bombardment from a supernova in late-Pleistocene times that "reset the radioactive clock."

Mammoth Trumpet has received a rebuttal to Firestone and Topping's article from two respected authorities on radiocarbon dating: John R. Southon, Center for Accelerator Mass Spectrometry, Lawrence Livermore National Laboratory; and R. E. Taylor, Radiocarbon Laboratory, Department of Anthropology, Institute of Geophysics and Planetary Physics, University of California–Riverside. Their rebuttal to the article is printed below in its entirety.

We offered Firestone and Topping the opportunity to respond. After the year that has elapsed since the article was published, however, the authors no longer agree about the events theorized in their article. Firestone's reply is printed below. Topping declined to respond, pending new experimental data.

Brief Comments on "Terrestrial Evidence of a Nuclear Catastrophe in Paleoindian Times," by Richard B. Firestone and William Topping

In a recent issue of *Mammoth Trumpet*, Richard B. Firestone and William Topping propose that about 12,500 CALYBP (solar time, not ^{14}C time), the earth was subjected to a sudden cosmic ray bombardment event due to a local supernova.[1] They postulate that one effect of this event—most pronounced in the north-central and north-eastern portions of North America, e.g., the Great Lakes region—was the production of secondary thermal neutrons which converted nitrogen to ^{14}C in charcoal samples contained in surface archaeological sites. According to Firestone and Topping, this in situ production of ^{14}C had the effect of, in their words, "resetting" the ^{14}C age of charcoals in these sites of up to the equivalent of about 40,000 years depending on latitude and the amount of overburden at a site.

For example, at the Gainey Site in Michigan, a 2880 RCYBP ^{14}C determination and a 12.4 ka [CALYBP] thermoluminescence (TL) date are cited. Firestone and Topping argue that the Gainey TL date, which, in their view, should be about 39.0 ka, was "reset" to about 12.4 ka by the heat generated by the nuclear bombardment. At the same time, they suggest that ^{14}C was added to the 39,000-year-old Gainey site charcoal sample as the result of a thermal neutron fluence of about 10^{17} neutrons/cm^2. They allege that the increased amount of ^{14}C produced in situ in the charcoal by these neutrons caused the inferred age of the sample, based on the measured ^{14}C content, to appear to be 2880 RCYBP rather than about 40,000 RCYBP.

We would like to make the following observations and pose several questions: (1) The thermal neutron fluence of 10^{17} neutrons/cm^2, which supposedly affected a large region around the Gainey site, is equivalent to an hour's irradiation in a typical research reactor,

Response to the Comments by J. R. Southon and R. E. Taylor

Southon and Taylor have commented on the immensity of the cosmic ray events reported in our *Mammoth Trumpet* paper[1] and questioned whether a common problem with radiocarbon dates at Paleoindian sites even exists. The magnitude of the events that we reported is based on neutron fluxes derived directly from uranium and plutonium abundance anomalies that were measured by reputable independent laboratories. Recent new results now exclude the massive neutron irradiations that we reported. Nevertheless, the cosmogenic isotope record, coupled with recent astronomical observations, provides direct evidence for the likely influence of nearby supernovae on radiocarbon dates at Paleoindian sites.

In our paper we reported measurements of depleted ^{235}U in cherts and sediments using Neutron Activation Analysis (NAA) at the McMaster Reactor. These results, summarized in Table 1, were consistent with a cosmic event that would have produced a terrestrial neutron flux of ca. 10^{20} neutrons/cm^2. We also measured ^{239}Pu concentrations with radiochemical methods at Nuclear Technology Services that were consistent with a 10^{17} neutrons/cm^2 event. At the USGS in Menlo Park[2], additional $^{235}U/^{238}U$ ratios were measured using Thermal Ionization Mass Spectroscopy (TIMS). The TIMS results for representative cherts and a uraninite standard are shown in Table 1. In each case ^{235}U depletion was found to be less than 1 percent, consistent with a neutron flux $<10^{19}$ neutrons/cm^2. TIMS is a more direct method for the analysis of isotope ratios. McMaster has not provided additional information to support their results, although corrections for fast neutrons produced by fission within the samples may have been important. The TIMS results indicate that large uranium depletions can no longer be considered credible.

The measured $^{239}Pu/^{238}U$ ratios are also summarized in Table 1. An additional result for Bayport chert flake from the Leavitt site has been added. Although these measurements are consistent with a large enhancement over expected background ^{239}Pu concentrations, no correction for contamination from fallout was applied. The measured ^{239}Pu activity was typically ca. 0.2 Bq/kg in both sediments and cherts. However, ^{239}Pu activity in surface sediment from nuclear testing is typically ~5 Bq/kg[3,4,5,6] at 40°–50° latitude. The ^{239}Pu activity is maximal at that latitude although activity is expected to decline sharply with depth. Our original observation of ^{235}U depletion inferred the

and for material with the composition of animal soft tissue, corresponds to an absorbed radiation dose of about 2 Megarads.[2] For acute (short-duration) exposures, this is roughly 3000 times the lethal whole-body dose for humans[2], and Firestone and Topping correctly note the lethal characteristics of the purported irradiation event. Since it occurred only 12,500 years ago, would not the effects be so massive and obvious in the paleobotanical and paleontological record that there would be little ambiguity in identifying and documenting this "nuclear catastrophe"? (2) Unless the supernova was so local as to be in low earth orbit (which is, of course, manifestly impossible), why was only a relatively small region of the earth affected? (3) The level of neutron interactions with nitrogen in charcoal was supposedly enough to "reset" ^{14}C in samples which were previously almost ^{14}C-dead, resulting in a radiocarbon age of 2880 RCYBP, i.e., a radiocarbon-specific activity of about 70 pMC (percent modern carbon). This activity was measured in the late 20th century some 12,500 years—more than two radiocarbon half-lives (2 x 5730 years)—after the purported event, so that the initial ^{14}C level following the irradiation must have been more than four times as high and thus in excess of 300 pMC. This large increase occurred in charcoal, with high carbon content (>40 percent) to dilute the newly produced ^{14}C but only traces of nitrogen (ca. 0.05 percent) from which to generate it. What then would have happened to the ^{14}C content of the atmosphere, which is 78 percent nitrogen?

In answer to the question posed above, if one adopts the 10^{17} neutrons/cm^2 value and assumes that an area 1000 km by 1000 km was irradiated, the excess ^{14}C production can be calculated based on the number of atmospheric nitrogen nuclei irradiated and the $^{14}N(n,p)$ cross-section. The result is astounding: the excess radiocarbon production diluted into the entire earth's atmosphere would have rapidly increased atmo-

production of excess ^{239}Pu, but in the absence of evidence for depleted uranium it is more reasonable to assume that modern ^{239}Pu must have invaded the chert and an anomaly is unlikely. There is no longer compelling evidence for a massive neutron event.

Another argument for a major cosmic event prior to 12,500 CALYBP exists in the radiocarbon record shown in Figure 1 of our paper[7,8]. ^{10}Be data[9] and numerous other results also indicate that cosmogenic isotope abundances were much higher before 12,500 CALYBP than now. Radiocarbon rapidly reaches equilibrium between the air, land, and ocean reservoirs, so elevated concentrations of 14C over many millennia must indicate higher cosmic ray rates before 12,500 CALYBP. There is ample evidence for sudden cosmic events at about 41,000, 33,000, and 12,500 CALYBP when radiocarbon suddenly increased by several tens of percent and then decayed with the ^{14}C half-life. Since radiocarbon is produced only in the atmosphere, where about 1.8 percent of global carbon resides[10], a 50 percent increase in global ^{14}C would correspond to a 3000 percent increase in atmospheric radiocarbon. Assuming this increase was due to a nearby supernova, it would take place over several decades as cosmic rays of different energies arrived. Plants and animals living then would absorb ^{14}C at an anomalous rate, leading to radiocarbon dates that are too young by many thousands of years. They would also experience a cosmic radiation dose ex-

Table 1 Summary of actinide data from Paleoindian sites.

Sample	Uranium (ppm)♦	USGS■ $^{235}U/^{238}U$(%)	McMaster▼ $^{235}U/^{238}U$(%)	$^{239}Pu/^{238}U$(ppb)
Uraninite standard	—	0.726 ± 0.007	0.73 ± 0.04	~ 0.003▲
Bayport chert	7.17 ± 0.13	0.724 ± 0.005	0.42 ± 0.06	32 ± 16
Gainey chert	0.7 ± 0.2	0.725 ± 0.009	< 0.4	~ 90
Gainey sediment	1.76 ± 0.09	—	0.94 ± 0.09	43 ± 4
Upper Mercer chert	3.58 ± 0.16	0.726 ± 0.019	0.17 ± 0.12	—
Chuska chert	45.8 ± 0.3	0.727 ± 0.005	0.60 ± 0.03	—
Fossil Hill chert	0.27 ± 0.02	0.732 ± 0.005	—	—
Onondaga chert	0.185 ± 0.006	—	—	—
Taylor chert	8.2 ± 0.5	—	0.59 ± 0.06	10 ± 1

♦A. R. Smith, Lawrence Berkeley National Laboratory Low Background Counting Facility, private communication, July 2001.

■Measured using Thermal Ionization Mass Spectroscopy (TIMS). The natural $^{235}U/^{238}U$(%) ratio is 0.7253.

▼Measured by Neutron Activation Analysis (NAA).

▲Value assuming no contamination from nuclear testing. Measured activities were ~0.2 Bq/kg. 239Pu from nuclear testing is typically ~5 Bq/kg in the top 10 cm of sediment at 40°–50° latitude.

spheric ^{14}C levels globally by a factor of about 1 million. Over the next 1000–2000 years, this ^{14}C "spike" would have mixed into the entire carbon cycle. Taking 40,000 gigatons (GT) for the total carbon inventory participating in the carbon cycle on thousand-year time scales and 600 GT for atmospheric carbon as CO_2 12,500 years ago, the overall increase in ^{14}C diluted throughout the entire carbon cycle (i.e., by a factor of 600/40,000) would have been a factor of 15,000. Today, more than two radiocarbon half-lives later, ^{14}C throughout the carbon cycle would still be about 3,300 times the pre-12,500 CALYBP level (and thousands of times the levels actually observed in contemporary materials).

This huge increase at 12,5000 CALYBP and subsequent decay is clearly not what is seen in radiocarbon calibration data. Late-Pleistocene data from laminated sediments and from corals dated by ^{14}C and uranium-series methods show a temporal offset in ^{14}C values at 12,500 CALYBP of about 2100 years (a ^{14}C increase of just 25 percent over recent levels), and the very precise dendrochronologically based calibration curve which covers the entire Holocene indicates that atmospheric ^{14}C level varied by no more than 10 percent over that period.[3] In contrast, under the Firestone and Topping scenario, the Holocene curve would be a pure exponential decay reflecting the 5730-year ^{14}C half-life, because the decay of the huge ^{14}C inventory produced in the 12,500 CALYBP event would grossly outweigh all subsequent natural ^{14}C production.

A bizarre consequence of such a radiocarbon cycle dominated by decay of an initial radiocarbon spike, as opposed to an approximate equilibrium between production and decay, is that the decay of the radiocarbon locked up within an organism or artifact at any time after the event would be matched by the decay of the global ^{14}C inventory. Thus, all carbonaceous materials synthesized after the event would contain the same ^{14}C concentrations today, regardless of when they actually formed. In other words, all post–12,500 CALYBP materials would yield identical radiocarbon ages!

Firestone and Topping assert that a "common problem at [Paleoindian] sites in the Great Lakes region of North America is the recovery of radiocarbon dates as much as 10,000 years younger than dates obtained with other methods." As far as we can determine, there are, in fact, data from only one other dating method available for comparison from one site—a single TL determination from the Gainey site. We have discussed the situation with two experienced archaeologists from the region and it is their general understanding that, in a number of sites in this region, the charcoal on which the ^{14}C dates had been obtained were derived from near surface contexts with significant site formation problems including

ceeding 100 rem for an extended period of time, placing great stress on many species and possibly leading to mutations and extinctions.

Recent astronomical evidence is compelling for the occurrence of many nearby, recent supernovae. The sun lies in the middle of a small local bubble of space swept clear of nearly all gas by these supernovae explosions[11]. Benítez et al. estimate that about 20 supernovae occurred within 40–130 parsecs (130–420 light years) of earth during the past 11 million years[12]. Sonnet has shown that each supernova gives rise to a series of forward, reverse, and reflected shockwaves arriving thousands of years apart[13]. It is inescapable that these nearby events would periodically produce cosmic rays that irradiate earth. Anomalous radiocarbon dates are thus expected to occur in concert with major ecological upheavals following each supernova event.

It is perplexing that Southon and Taylor don't acknowledge that there is a common problem with young radiocarbon dates at Paleoindian sites near the Great Lakes region. We reported seven dates from 160–3810 CALYBP for Paleoindian sites. Unless Paleoindians persisted into modern times, this is clear evidence of a problem, whatever the cause. A similar problem exists in the Northeast, where a fluted-point site (Munsungun Lake) yielded 20 dates from 35–3405 CALYBP; yet Debert (Nova Scotia) yielded 23 dates from 5033–11,120 CALYBP, of which only two were less than 10,000 years old. Bonnichsen and Will suggest[14] the young dates might be due to forest fires and tree throws, but they could also be the result of a nearby supernova. Southon and Taylor also reference unnamed archaeologists who allege that all the Great Lakes sites were compromised by the intrusion of surface material. Henry Wright has indicated that the Michigan sites were carefully excavated and material below the plow zone at Leavitt yielded unrealistically young dates[15]. Wright indicated there is a common problem with the Paleoindian dates that may be open to various explanations. Allegation of sample misidentification is hardly evidence that this occurred and is an rather unscientific method of eliminating data that fail to meet expectations.

There are two thermoluminescence measurements for the Gainey site reported by Rowlett that give comparable values of 12,360±1224 and 11,420±400 CALYBP. These results are consistent with expectations for a fluted-point site, so it is hard to comprehend why Southon and Taylor would have problems with them. However, there is other information about the age of the Great Lake sites that they ignore. As we have stated, the artifacts associated with these sites contained cemented sediments deposited before spodosols ceased in the area, indicating their association with the old C horizon and thus a very old date. This confirms that there is a clear discrepancy between the radiocarbon dates and the true dates for Great Lake Paleoindian sites. Southon and Taylor insist without proof that this is the result of misidentification of charcoal stratification. That might be true in some cases, but evidence for nearby supernovae suggests that there must also be anomalous dates associated with those events. This distinction might be difficult to resolve, except that we also provided evidence that the Paleoindian artifacts from seven sites were subject to a traumatic bombardment while still exposed on the ground. A high density of particle tracks, pits, and chondrules were found embedded on only one side of

features truncated by the plow zone. For example, a report on excavations at the Gainey site noted that the very shallow original deposit has been disturbed as the result of its being "located in a cultivated field."[4]

While we appreciate the ingenuity and creativity of the proposals developed by Firestone and Topping, we find their arguments, particularly those that pertain to radiocarbon geophysics, to be, at best, highly problematical and, at worst, difficult to take seriously. We suggest that the most likely explanation for the purportedly "young" ^{14}C age determinations from the seven archaeological sites noted in their Table 1 is what is most often the explanation for such perceived anomalies: the misidentification of charcoal samples with their expected statigraphic level or purported geological context by bioturbation, geoturbation, or other site formation process.[5] The history of ^{14}C applications in archaeology is replete with examples of such occurrences. This in no way should be construed as constituting a reflection on the care with which any of these sites were excavated. It is simply the recognition of unrecognized complexities of geomorphological processes in distributing and redistributing charcoal in a soil profile.

–John R. Southon, Center for Accelerator Mass Spectrometry, Lawrence Livermore Natl. Laboratory

–R. E. Taylor, Radiocarbon Laboratory, Dept. of Anthropology, Institute of Geophysics and Planetary Physics, Univ. of California–Riverside

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chert artifacts. Adjacent sediments were enriched in magnetic spherules. No terrestrial process could have produced these features, yet a supernova would provide the relativistic cosmic rays and dust that could have impacted the landscape.

Southon and Taylor correctly point out that the event as published was too extreme to be reasonable. New data presented here should place us in general agreement on this issue. However, I suggest that a gentler form of catastrophe occurred that is consistent with cosmogenic isotope record. This event would have had a profound impact on any Paleoindian who observed it, and similar occurrences must have occurred frequently over the past millennia with dire consequences. While mistakes in sampling can be important, as Southon and Taylor suggest, it is not prudent to assume this occurs whenever results defy expectations. Sites with anomalous radiocarbon dates may be especially interesting when they can be related to the times of cosmic events. We should all agree that more research is needed to unfold the events of the late Pleistocene.

–Richard B. Firestone, Lawrence Berkeley Natl. Laboratory

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Kennewick Man

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"two" in two groupings of languages that Dixon and Kroeber thought might be related, "pen" in Yokuts, Wintu, and Maidun, and "uti" in Coastanoan and Miwok. In 1929 Edward Sapir defined the Penutian languages as a super-stock that included languages in Oregon and Washington State as well as in Mexico. As Michael Silverstein points out, Sapir did not consider this an established grouping, but rather "a hypothesis to be tested by subsequent research." This, says Silverstein, was soon forgotten, and Sapir's "schema was accepted literally." Today just which languages within the supposed phylum are actually related to one another is the subject of debate. Some linguists doubt that one can speak of Penutian as a genetic category at all. William Shipley has searched for regular sound correspondences among the Penutian languages that would support this classification and has not found them. His conclusion is that they simply are not there and that "the term Penutian has no genetic definition at all." Campbell, who seconds Shipley's recommendation, says we should stop using the misleading label "Penutian." It is better, therefore, to speak of Penutian as a hypothesis rather than as a genetic unit of great structural differentiation and wide dispersal.

The general rule is, the greater the differentiation among related languages, the further back in time they diverged from their common ancestor. But if there is *not* a Penutian phylum, then the differences between its supposed language families do not justify the time depth that some linguists have claimed; instead, differences would be accounted for because they belong to separate genetic units and not because they are divergent members of the same genetic unit. And if there is *not* a Penutian phylum, then speakers of its supposed language families cannot claim to have emerged from an ancestral homeland and later dispersed over great areas, whether in the Pacific Northwest, including the Columbia Plateau, or anywhere else.

As for lexical evidence that supports the continuity theory, Hunn himself ad-

mits that "it is possible that the demonstrated close association between Sahaptin vocabulary and local environmental features may have developed in a matter of centuries rather than millennia, leaving open the possibility that Kennewick Man spoke some non-ancestral language."

Given all this, it is clear that the preponderance of linguistic evidence does not establish cultural affiliation between Kennewick Man and the tribes that today claim him as their remote ancestor. In fact, quite a different conclusion can be drawn from the evidence at hand.

An alternative scenario

A corollary to the general rule about language differentiation and divergence says the presence of languages with minimal differences over a broad territory suggests recent divergence and implies the process of language spreading. Salishan languages are more numerous than Sahaptian and are more sharply differentiated, with the greatest differentiation along the coast. Moreover, Salishan languages on the Plateau, called Interior Salishan, are sharply differentiated from those along the coast. This suggests an expansion of the Salishan languages from the coast into the interior long ago. Archaeology tends to support this hypothesis. On the other hand, the fact that the two Sahaptian languages, Sahaptin and Nez Perce, cover a wide area of the southern Plateau and differ little from one another indicates a recent expansion over the area and makes it unlikely that any modern Sahaptian dialect has ever been spoken on the same spot for very long.

There is both archaeological and historical evidence of the movement of peoples in the Plateau Culture Area. About 8,800 years ago the Pebble Tool Tradition completely replaced the earlier and far more elaborate Stemmed Point Tradition in the Columbia Basin. Since the break was sharp, as clearly revealed in the archaeological record, and since it involved a change from a more elaborate to a less elaborate technology, archaeologist James Chatters and others think it likely involved the migration of new people with a different culture into the area at a time of changing climatic conditions. Subsequent cultural discontinuities revealed in the archaeological record suggest additional migrations into the region. Kennewick Man died during the transition between two cultural traditions, leading Chatters to ask, "Was the change accompanied by a population replacement?" as suggested by physical differences between Kennewick Man and historical Indian populations. Or instead, "Were the local Stemmed Point makers acculturated by the newcomers?" Was the encounter hostile? If so, which tradition did Kennewick Man belong to? The hypothesis Chatters proposes is that Kennewick Man may have been a member of a population that became extinct in the encounter or he may have been "genetically swamped by newcomers in the region." In either case, the languages spoken in the area today cannot tell us anything about those remote events.

The historical record documents the "intrusion" into the Plateau Culture Area of peoples from different regions speaking different languages. Data from an Athapaskan language, now extinct, were recorded in the Nicola Valley in the northern part of the Plateau Culture Area in the 19th century. The movement of Paiutes and Shoshone, Numic-speaking peoples from the Mojave Desert some 800 to 1,000 miles to the south, was well underway before the first whites arrived in the region. In fact, Numic speakers were spreading out onto the Great Plains as well; known on the Texas Plains as Comanche, they raided white settlements in Texas and deep into Mexico. The intruding Numic collided violently with the native Sahaptian-speaking peoples. The Numic speakers pushed Sahaptian-speaking settlements northward.

How language spreads

Language spreads when a population in an area is replaced by another population that speaks a different language. Language also spreads through a process linguists call language shift, which occurs when a speech community replaces its original language with that of another over a period through what has been called the process of replacive bilingualism. This process is well known in the United States, where immigrant groups, even in concentrated enclaves, eventually replace their original language with English. Language shift has also been observed in the Plateau Culture Area and in archaic linguistic landscapes all over the world. The extinction of the Athapaskan Nicola language is one example. The ultimate extinction of the Cayuse language in the south is yet another.

Cayuse is a language isolate that jutted up into Sahaptian-speaking territory both north and south of the Umatilla River. The shift from Cayuse to the Sahaptian languages was underway before white contact; fortunately, it was recorded by observers in the 19th century. Substratum features from extinct languages are probably retained in the languages that replace them, but without historical records to document their existence, relic features might never be recognized and the existence of extinct languages might never be suspected. Given the linguistic diversity of archaic landscapes like that of Native America, and given prehistoric and historic movement of native people along the Columbia River and into the Plateau area from north and south, we will never know how many languages have appeared and disappeared in the Columbia River Basin over the millennia.

The point is that the linguistic history of the Plateau Culture Area, and especially of the Columbia River Basin, was likely far more dynamic than the static picture presented by Hunn in his argument for long-term continuity.

Language, race, and culture

In the 19th century, linguists realized that the histories of languages, cultures, and physical types (races) run different courses. Franz Boas made this point many times, especially in 1911 in the introduction to *Handbook of American Indian Languages*, the founding document of anthropological linguistics. A population characterized by a single physical type can change languages; witness the case of people of African descent in the Americas who speak English, Spanish, Portuguese, French, and French Creole. Ives Goddard observes that the population of English, according to DNA studies, has remained biologically stable for thousands of years, yet it underwent two major language changes in proto-historic times, one from an unknown language to Celtic in the first millennium B.C., and one from Celtic to the Germanic ancestor of English in the 5th century A.D. A population may merge with a speech community and spread its language to people of different physical types, as was the case of Magyar in the early Middle Ages. Boas cites Athapaskan as an example of this process in North America. There is also the kind of pattern seen on the Great Plains, where people of different languages and different physical types shared a common cultural adaptation.

Boas concludes that "anatomical type, language and culture have not necessarily the same fates." Without historical evidence, he insists, the pattern of "extended changes in language

COMING CONFERENCES

April 10–13 2002, 55th Annual Meeting, Northwest Anthropological Conference, Owyhee Plaza Hotel Conference Center, Boise, ID. Jointly hosted by the Idaho State Historical Society, Archaeological Survey of Idaho, Boise National Forest, Idaho Bureau of Land Management, and the Department of Anthropology of Boise State University. The conference theme is *Preserving the Spirit of Place*, adopted in coordination with the National Trust for Historic Preservation. The hosts encourage submission of papers, symposia, and poster sessions covering all geographical areas, subfields within anthropology, and related fields. David S. Whitley will speak at the closing banquet, followed by presentations honoring Max P. Pavesic.

Contact: Mary Anne Davis, Conference Chair, Northwest Anthropological Conference/ISHS, 210 Main St., Boise, ID 83702.

August 7–9 2002, International Symposium on *Early Humans in America: implications for the Peopling of the Basin of Mexico*, Museo Nacional de Antropología e Historia, Mexico City. Principal themes are new data on the antiquity of man in America and possible migration routes through the American continent; paleoenvironment during the Pleistocene/early Holocene in America—geology, fauna, flora, etc.; and the history of prehistoric research in Mexico and America. Papers presented will be simultaneously translated in Spanish and English. A guided tour is planned of prehistoric sites around the Basin of Mexico, including Peñon de los Baños (Paleoamerican site), Tepexpan (Paleoamerican and mammoth site), Tocuila (mammoth site), and the Teotihuacan Pyramids. Deadline for submitting title and abstract of talk is 31 March 2002. Registration fee ($110 U.S. or equivalent in Mexican pesos) must be paid before 15 May 2002.

Contact: Dr. Silvia Gonzalez, School of Biological and Earth Sciences, John Moores University, Byrom Street, Liverpool L3 3AF, England; bessgonz@livjm.ac.uk; or Jose Concepcion Jimenez Lopez, Instituto Nacional de Antropología e Historia, Direccion de Antropología Fisica, Reforma y Gandhi s/n Col., Chapultepec Polanco, Mexico D.F. 11560, Mexico; josejimenez_daf@hotmail.com

August 8–11 2002, 17th Biennial Meeting, American Quaternary Association (AMQUA), University of Alaska Anchorage, AK. Conference theme is *Peopling of the Americas in Its Paleoenvironmental Setting: Climate Change and Human Migration in the North Pacific Basin.* Program topics include Late Quaternary Paleoecology and the Peopling of the Pacific Coast. Field trips and workshops are planned. The AMQUA conference is preceded by the International Inuit Studies Conference and special Beringia Working Group (INQUA) symposium on Archaeology of Northeast Asia and Peopling of the Americas. Deadline for registration and poster submissions is 1 May 2002.

Contact: David R. Yesner, Local Arrangements Chair, Dept. of Anthropology, University of Alaska, 3211 Providence Drive, Anchorage, AK 99508; 907-786-6845; fax 907-786-6850; afdry@uaa.alaska.edu

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and culture without changes in blood," as opposed to the retention of an old language with the mixing of populations of different physical types, "cannot be proved."

What language did Kennewick Man speak?

Hunn speculates that Kennewick Man possibly spoke a Proto-Penutian dialect. Yet there is serious doubt such a language ever existed. Hunn admits he cannot rule out the possibility "that the group to which Kennewick Man belonged spoke . . . a language now extinct or ancestral to languages spoken outside the present region." Without a historical record, there is no way to describe with certainty a linguistic landscape in antiquity or its changes over time. Claims of cultural affiliation in the remote past that employ linguistic evidence are thus based on pure speculation.

If we are to admit linguistic evidence of the remote past to establish cultural affiliation, we must adopt a protocol, a set of rules that define what facts linguists can realistically recover from the past and what may be admitted as evidence. These rules might take the form of a scale of plausibility, similar to the scale suggested by Lyle Campbell for evaluating claims of genetic classification in historical linguistics. Until we adopt such a protocol, confusion and political interests will dominate the process.

–Glynn Custred

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The Baja Connection

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slopes under the overhang into an area not yet excavated. It is rich in shell fragments, flakes and lithic artifacts, and charcoal. It is worth noting that the shells include those of clams and giant chitons, species not found in the region today. Among the artifacts found are many large flakes, some retouched to form scrapers, along with flaked cobbles, cores, hammerstones, and abundant flaking debris. Charcoal samples from the lowest zone were dated between 8240 ± 160 and 8870 ± 60 RCYBP. Beneath the lowest stratigraphic zone is reddish brown loamy silt with coarse rubble, which overlies volcanic bedrock about 8 m (26¼ ft) below surface. Since charcoal fragments in this stratum had clearly intruded from above, no attempt was made to date this layer.

So far Abrigo de los Escorpiones has yielded about 800 artifacts (not counting unmodified lithic flakes still to be analyzed). Fine bifacially flaked specimens are rare; most artifacts are simple, chiefly large, heavy tools like flaked cobbles, hammerstones, and choppers.

Clearly the site has been used as a shellfish collecting station since at least the early Holocene, some 9000 years ago. The enormous shell deposits, however, obscure the vast interior of the rockshelter, where Gruhn and Bryan hope to find evidence of an even earlier occupation dating back to the Pleistocene.

They plan to resume excavating Abrigo de los Escorpiones this May and June. They're wiser now about the unique conditions at the coastal site. Fog commonly rolls in during the evening and lasts until mid morning, sometimes until noon. Condensation is a problem. "When we first went there," Gruhn remembers, "we stayed at a campground. In the morning tents, sleeping bags, everything would be soaked. You need a permanent shelter if you plan to be there for an extended period." Now they rent a house a few miles from the site.

They'll travel in a Chevy pickup. The Land Rover is in their garage, "not exactly in running condition," Gruhn admits. They just can't bring themselves to part with it. It's an old friend, like a faithful draft horse past its working years they won't send to the glue factory.

–JMC

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Suggested Readings

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