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 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, biologic, archaeological, linguistic, folklore, oral traditions, historical evidence, or other information or expert opinion—reasonably leads to such a conclusion (italics added).

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 of 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 the line that runs through the mid-re
ions 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 Kinscalle et al. report that two territorially extensive language families, Sahaptin and Sahaptian, characterize 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 other known language. For example, Basque is an isolate standing alone in Europe, where languages of the Romance Family are spoken in the south. Germotic languages in the north, Celtic languages along the northwestern fringe, and languages of the Iberic 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 Huns 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 Colville Confederation, which is predominantly 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.

Huns 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 founding population of the Americans 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 Columbia Plateau, says Huns, 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."

Huns 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-
gree of confidence expressed in the quotation from Hunt. His assertion is anchored in the belief that the general secular trends of linguistic expansion and replacement of languages spoken 10,000 years ago are unlikely to have any descendants still spoken today.

**Problems with the argument of continuity**

Hunt is careful to couch his conclusions in terms like 'possible,' but 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 the 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 Alu, an aboriginal pre-Mongolian population of Northeast Asia, than any other living population.

**Another problem**

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

**continued on page 17**
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 an ancient tool maker 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 flinknappers of Titmus’s expertise, archaeologists can interpret much about the stone artifacts they unearth.

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.”

Always popular with students, Titmus here is interviewed by Idaho State University student Earl Mitchell, ca. 1998.
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 1950s, 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 saddened 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 answer 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, Clevia 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 it 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.”

The object Gene is holding is a replica of a Maya eccentric. The original was manufactured of sheet stone during the late Classic Period, ca. A.D. 600-900. Gene’s replica is made of abalone. Maya eccentrics are generally found in burial offerings, or as dedicatory caches to commemorate new buildings. They are knapped using basic percussion techniques to make a large bifacial flake (the same basic technology used in North America to make bifaces), then using a combination of indirect percussion (using a punch) and pressure (using water 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 bird-crest and extended arms and legs, and all the deity’s hands are holding some unidentified object. Many of the simpler eccentrics are made in the form of serpents, aquatic, or bird forms. Anthropomorphistic 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 among the best. He has a forthcoming paper on this very topic in a volume being edited by Dr. Kenneth Martin of Pennsylvania State University. The largest Maya eccentrics are found at this site of Copan, Honduras. To see some spectacular examples, see National Geographic, September 1991, pp. 107-108.

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

—James C. Woods

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.
Points with Pedigrees

Classic Clovis (left) and Folsom points. This Folsom point, according to Titmus, "was an isolated surface find in 1923 on Lake Channel, a basaltic coulee located about 25 miles south of Falls, Idaho. The point exhibits basal and lateral marginal dullying 8 cm up from the base. It was taken from a site 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 repolished numerous times (perhaps because of dullying, or it may have broken at the distal and form impact and been repolished). It is interesting to note that, when repolished, the knapper maintained the small, sharp, pointed distal end and that helps initiate easier penetration into game."

Of the Clovis point, Titmus says, "It was part of an ethnographic collection assembled between 1866 and 1869 by Edward S. Drinker, a civilian surgeon stationed at Fort Laramie on the Nez Perce Indian Reservation, Idaho Territory. The collection was elegantly sent to the Army Medical Museum, and in 1869 the Smithsonian Institution acquired 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 Indian living on the reservation; since the Nez Perce made hunting and gathering up to the point, there may have been some contact. The Clovis point, made from chert, appears to have been produced, not repolished. It has the typical spaced collateral rake scars seen on the Weikamp Clovis, and the basal and lateral proximal margins have been dulled, evident in the light and smoother appearance of the first 3.5 cm of the proximal margin up from the base. Both faces have two blurred rake scars. Multiple channel rake scars appear to have been used to create a flat surface, wedge-shaped cross section at the proximal end and meet Clovis helting requirements. This is more like a braided flaking procedure than the fluting procedure found on Folsom points. The point is in overall good condition. Major differences to other Clovis points recovered in Clovis caves—Fern, Simon, Drake, and the like. (Titmus emphasizes that he hasn't personally examined this Clovis point, its comments are based on photos and drawings, and as on an article by B. R. Pulverd and D. D. Jones that appeared in 1924 in the field, 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 resume includes lengthy lists of archaeological debt projects, technical publications, presentations, workshops, consultations, and demonstrations. Because he is not a professor, he is not shackled to any authority 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 1981 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 possess a broad understanding of ancient technologies that probably is rare among flintknappers. He says he became involved with many differing technologies because his knowledge was acquired about them. "Their study entitled 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 flint-knapping expertise vested in a few skilled specialists, or did the majority of people practice such skills? "I could 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 to be self-sufficient in that unforgetting 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 prehistory of the Americas, others he made to perfect his skill at a particular technique, and some he made for the pure joy of it.

- 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 Evelyn point (below, right) as an exercise in removing a long channel flake, not to represent a type of point.
Our apology... The suggestion that we were Gene Titmus in an article came to us from Steve Kohlopg, who was himself featured in the December issue of Mammoth Trumpet ("Lithic Caches: The Puzzling Legacy from Early Knappers"). We express our gratitude poorly by misspelling his e-mail address. Steve's correct address is swzk@flitel.com, not swzk@filtertel.com.

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,"

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 hiluxes 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."

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 Harrett 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, and tested 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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known provenience—as proof of a known antiquity. Titmus has extensive knowledge of both.

What makes Clovis projectile points unique? "Clovis points were in almost all instances made from chert," 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 square, a break-resistant construction." As a result, he says, many sharpened 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 adhered 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.

Regarding those hallmark Clovis flutes, he says that the removal of the channel flutes 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 remains somewhat uncertain. "Many years ago Don Crabtree proposed a hafting method for the system for the Folsom point. Titmus recommends a paper by Stan Alder 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 much more beautiful 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

How to contact the principal of this article:
Gene Titmus
610 Twin View Road
Jerome, ID 83338
e-mail: titmus@magicjack.com

Suggested Readings
I T ISN'T AN EASY DRIVE from Alberta to Baja California, but Ruth Grunin and Alan Bryan, archaeologists from the University of Alberta (and husband and wife), have been making the journey since 1981. 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 9,000 B.C. (10,500 calendar years ago)—and they have hopes of pushing dates back even further.

The coastal connection

**Why walk when you can paddle?**

The theory they are testing is the coastal-entrance theory of migration, first proposed by anthropologist Knut Fladmark of Simon Fraser University nearly 30 years ago. Dr. Grunin 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-entrance model cuts through complicated theories contorted to explain how humans crossed the Bering land bridge on foot, how they found a route through the Cordilleras and Lawrence 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 needed to be an unrelentingly 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-entrance theory has an especially attractive advantage over models that have humans traversing the continents—and the entire hemispheres—on foot: speed. Paul Martin's prehistoric-overkill hypothesis, for example, contends that hu-
dates around 12,000 BCYBP, 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.

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 Danielle Lavalé, whom Gruhn met at a conference in Belgium. Although Lavalé 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 or obliterated by shifting and melting glaciers. (Archaeologists 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,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 Anshmann reported the discovery of fluted points in the northern part of Baja California Sur, and in 1957 Bringham Arnold found large boulders on old beach beaches that he suspected might date to the Pleistocene.

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 ‘generators’ who had long exploited the productive Pacific coastal ecosystems with a relatively simple, little technology.” The earliest colonizers would probably have supplemented their tool kits with imple-
mounds 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 Grunh 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 and dumped a load of gravel. About 10 or 20 minutes’ walk from the rockshelter are a series of felsite dikes that traverse the gravitic bedrock. Felsite is a very fine, tootastic, fine-grained and quite flaky. 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 points, then into projectile points and knives.

The First South Americans
The Peopling of a Continent from the Earliest Evidence to High Culture
Danielle Lavallée

"With this book, Lavallée brings to early South American prehistory a European perspective refreshing in its unbiased and explicitly empirical approach to the known archaeological record of early developments in the southern continent."
—Ruth Grunh, University of Alberta

"Lavallée has provided a nice introductory summary of the range of early South American complexes."—Latin American Antiquity

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(11½ ft) below. A small alcove could shelter one or two persons, "but it leaks." Grunh notes wryly. Its best feature is shade, a precious commodity in the desert. Campers using it today have made a stone-lined fireplace and swept 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, Grunh 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 gravitic bedrock. Felsite is a very fine, tootastic, fine-grained and quite flaky. 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 points, 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 follate (leaf-shaped) projectile points and preforms, and "touns" 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. Grunh 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 25–50 cm (8–20 inches) date between 800±580 and 900±5600 (2KBP). Sand and silt appearantly 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 plovel lakes in what is now desert in Baja," Grunh 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 Grunh 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," Grunh 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 (324 ft). Convinced that Abrigo Paredón had no more stories to tell them, Grunh 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
Escopiones didn’t get exciting until they reached 6 ft deep.

The sheltered area of Abrigo de los Escopiones, about 3-5 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 (17 ft). Although there was at least another 2 m (6 ft) of rock rubble overlaid bedrock, no more undisturbed cultural materials were found, and the excavations were filled with hay bales and covered with dirt in anticipation of resuming work in 2001.

Vanaha visited the site, however, and disturbed the upper 6 ft of the excavations. The mischievous archaeologist did Grohnan 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 Escopiones, 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 (9 ft), consists of assorted brown silt containing shells (masses), 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 sandy 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, Grohnan and Bryan also found several large lanceolate projectile points and substantial flaking debitage. Samples from the middle zone were dated at 8040 ± 300 BRY and 8040 ± 70 BRY at a depth of about 3.5 m (11½ ft). The lowest midden zone, brown sandy silt with rubble about 1 m (3½ ft) thick, continued on page 20.
Article Questioning Radiocarbon-dating Accuracy Draws Fire from Scientists

In "Terrestrial Evidence of a Nuclear Catastrophe in Paleolithic 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 supernovae 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. Soutophon, Curator 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 a printout below. Topping declined to respond, pending new experimental data.

Brief Comments on "Terrestrial Evidence of a Nuclear Catastrophe in Paleolithic Times" by Richard Firestone and William Topping

In a recent issue of Mammoth Trumpet, Richard B. Firestone and William Topping propose that about 13,500 CALBP (solar time, not 14C time), the earth was subjected to a sudden cosmic ray bombardment event due to a local supernova. 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 14C in charcoal samples contained in surface archaeological sites. According to Firestone and Topping, this in situ production of 14C had the effect of, in their words, "resetting" the 14C age of charcoal 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 BRYBP 14C determination and a 12.4 ka [CALBP] 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 14C was added to the 39,000-year-old Gainey site charcoal sample as the result of a thermal neutron flux of about 1011 neutrons/cm². They argue that the increased amount of 14C produced in situ in the charcoal by these neutrons caused the inferred age of the sample, based on the measured 14C content, to appear to be 2880 BRYBP rather than about 40,000 BRYBP.

We would like to make the following observations and pose several questions: (1) The thermal neutron flux of 1011 neutrons/cm² which supposedly affected a large region around the Gainey site, is equivalent to an hour's irradiation in a typical research reactor, but much longer for a reactor fuel core. Given the characteristics of the neutron flux, it is evident that the samples should remain at an 11,000-year-old age if the 14C flux is not affecting them. We would like to suggest that the authors have a more complete understanding of the nature of the flux. (2) The 14C age of the charcoal is in the 12,000-year range, not the 11,000-year range. The authors are aware that the TL age is a more accurate measure of the age of the charcoal, but the authors should also be aware that the TL age is not affected by the 14C flux.

Response to the Comments by J. B. Soutophon and R. E. Taylor

Soutophon and Taylor have commented on the immensity of the cosmic ray event reported in our Mammoth Trumpet paper and questioned whether a common problem with radiocarbon dates at Paleolithic sites even exists. The magnitude of the event that we reported is based on neutron fluxes derived directly from uranium and thorium 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 Paleolithic sites.

In our paper we reported measurements of depleted 230Th in charcoal and sediments using Neutron Activation Analysis (NAA) at the McMaster Institute. These results, summarized in Table 1, were consistent with a cosmic event that would have produced a terrestrial neutron flux of ca. 1010 neutrons/cm². We also measured 230Th concentrations with radiocarbon methods at Nuclear Technology Services that were consistent with a 1011 neutrons/cm² event. At the USGS in Menlo Park, additional 230Th/232U ratios were measured using Thermal Ionization Mass Spectrometry (TIMS). The TIMS results for representative blanks are shown in Table 1. In each case 230Th depletion was found to be less than 1 percent, consistent with a neutron flux of 1010 neutrons/cm². 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 loss of neutrons produced by fission within the samples may have been important. The TIMS results indicate that large uranium blanks can no longer be considered credible.

The measured 230Th/232U ages are also summarized in Table 1. An additional result for Rapport chert flakes from the Levant site has been added. Although these measurements are consistent with a large enhancement over expected background 230Th concentrations, no correction for contamination from fallout was applied. The measured 230Th activity was typically ca. 0.2 Bq/kg in both sediments and cherts. However, 230Th activity in surface sediment from nuclear testing is typically 5 Bq/kg or less at 40°-60° latitude. The 230Th activity is negligible at that latitude, although activity is expected to decline sharply with depth. Our original observation of 230Th depletion inferred the
and for material with the composition of animal soft tissue, corresponds to an absorbed radiation dose of about 2 Megarads. For acute (short-duration) exposures, this is roughly 3000 times the lethal whole-body dose for humans, 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 paleoanthropological and paleoneutron record that there would be little ambiguity in identifying and documenting this "nuclear catastrophe"? (2) Unlike 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 2580 B.C.YBP, 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 ago, 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 $^{90}$ neutron/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(p,$\alpha$)$^{11}$N cross-section. The result is astounding: the excess carbon production diluted into the entire earth's atmosphere would have rapidly increased atmo-

<table>
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<tr>
<th>Sample</th>
<th>$^{14}$C (pMC)</th>
<th>$^{12}$C (pMC)</th>
<th>$^{13}$C (pMC)</th>
<th>$^{15}$N (pMC)</th>
<th>$^{14}$N (pMC)</th>
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<td>Report chart</td>
<td>3.17 ± 0.13</td>
<td>0.724 ± 0.003</td>
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<tr>
<td>Campuchian</td>
<td>0.7 ± 0.2</td>
<td>0.725 ± 0.009</td>
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<tr>
<td>Campuchian</td>
<td>1.76 ± 0.09</td>
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<tr>
<td>Upper Mae chart</td>
<td>3.38 ± 0.16</td>
<td>0.726 ± 0.019</td>
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<td>Onnondy chart</td>
<td>0.184 ± 0.006</td>
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<tr>
<td>Taylor chart</td>
<td>8.2 ± 0.8</td>
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</table>

* A. K. Smith, Lawrence Berkeley National Laboratory's Lawrence Radiation Laboratory, private communication, July 2002.
* Measured using Thermal Ionisation Mass Spectrometry (TIMS). The expected $^{14}$N/$^{15}$N ratio is 0.735.
* Measured by Neutron Activation Analysis (NAA).
* Value assuming no contamination from nuclear testing. Measured activities were 0.2–2 Bq/kg. 259/259 from nuclear testing is typically ~5 Bq/kg in the top 10 cm of sediment at 40°–50° latitude.
spheric 14C levels globally by a factor of about 1 million. Over the next 1000–2000 years, this 14C "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 CO2, 12,500 years ago, the overall increase in 14C diluted throughout the entire carbon cycle (i.e., by a factor of 1000–4000) would have been a factor of 15,000. To-day, more than two radiocarbon half-lives later, 14C throughout the carbon cycle would still be about 3,300 times the pre-12,500 CALYP level (and thousands of times the levels actually observed in contemporary materials).

This huge increase at 12,500 CALYP and subsequent decay is clearly not what is seen in radiocarbon calibration data. Late-Pleistocene data from laminated sediments and from corals dated by 14C and uranium-series methods show a temporal offset in 14C values at 12,500 CALYP of about 2100 years (a 14C increase of just 25 percent over recent levels), and the very precise dendrochronologically based calibration curve which covers the entire Holocene indicates that atmospheric 14C level varied by no more than 10 percent over that period. In contrast, under the Firestone and Topping scenario, the Holocene curve would be a pure exponential decay reflecting the 5730-year 14C half-life, because the decay of the huge 14C inventory produced in the 12,500 CALYP event would grossly overweigh all subsequent natural 14C 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 14C inventory. Thus, all carbonaceous materials synthesized after the event would contain the same 14C concentrations today, regardless of when they actually formed. In other words, all post-12,500 CALYP materials would yield identical radiocarbon ages!

Firestone and Topping assert that a "common problem at [Paleindian] 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 14C dates had been obtained were derived from near surface contexts with significant site formation problems including

CALYP. These results are consistent with expectations for a dated-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 Lakes sites that they ignore. As we have stated, the artifacts associated with these sites contained cemented sediments deposited before sedge mounds ceased the area, indicating their association with the old C horizon and thus a very old date. This proves that there is a clear discrepancy between the radiocarbon dates and the true dates for Great Lakes Paleindian sites. Southon and Taylor fail without proof that this is the result of misidentification of charcoal stratification. That might be true in some cases, but evidence for nearly supersensitive suggests that there must also be unannounced dates associated with these events. This distinction might be difficult to resolve, except that we also provided evidence that the Paleindian artifacts from seven sites were subject to a traumatic bone-hardening while still exposed on the ground. A high density of particle tracks, pluts, and charcoal had been embalmed on only one side of
features truncated by the plow zone. For example, the exposure at the Gaisney site noted that the very shallow original deposit has been disturbed as the result of its being "located in a culti-
vated field." 14

While we appreciate the ingenuity and creativity of the proposals developed by Firestone and Topping, we find their ar-
guments, particularly those that pertain to radiocarbon geophysics, to be, at best, highly problematical and, at worst, diffi-
cult to take seriously. We suggest that the most likely explanation for the purportedly "young" 14C age determinations from the seven archaeological sites noted in their Table 1 is what must often the explanation for such perceived anomalies: the misidentification of charcoal samples with their expected stratigraphic level or purported geological context by bioturbation, geoturbation, or other site formation process. 15 The history of 14C ap-
lications in archaeology is replete with examples of such occurrences. This is no

certain that artifacts. Adjacent sediments were enriched in magnetic spherulites. No terrestrial process could have ges-
due to these features, yet a suggestion would provide the relativistic cosmic rays and dust that could have im-
pacted 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 gen-
eral agreement on this issue. However, I suggest that a gentler form of cata-
trophic occurred that is consistent with conspecific isolate record. This event
would have had a profound impact on any Paleolithic who observed it, and similar occurrences must have oc-
curred frequently even over the past millennia with dire consequences. While

miles/centuries in sampling can be important, as Southon and Taylor suggest, it is not prudent to assume this occurs when even 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 unveil the events of the last Pleistocene. 16

—Richard B. Firestone, Laurences Berkeley Natl. Laboratory

Kenneck Man

continued from Page 3

"two" in two groupings of languages that Dixon and Kroeger thought might be related, "pen" in Yokuts, Wintu, and Maidu, and "uri" in Costanoan 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 Shipleys's recom-
mendation, says we should stop using the misleading label "Penutian." It is bet-
ner, therefore, to speak of Penutian as a hypothesis rather than as a genetic unit of a 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 differ-
ences between its supposed language families do not justify the time depth that some linguists have claimed; instead, differ-
ces would be accounted for be-
cause 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 fami-
lies cannot claim to have emerged from an ancient homeland and later dis-
persed over great areas, whether in the Pacific Northwest, including the Colum-
bia Plateau, or anywhere else.

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

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8Veeh, B. L. et al., Radiocarbon 49, 1025 (1997).


15Kahn, E., Elginly of Michigan, pers.

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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 reproductive 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 Arawak languages in Cambodia is one example. The ultimate extinction of the Cayuse language in the south is yet another.

Cayuse is a language isolate that joined up into Sahaptian-speaking territory north and south and east 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. Sahaptan 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 disappeared 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 that the static picture presented by Hunt 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 his book, 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 Creole. Ives Goddard observes that the population of English, according to 1861 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 "anatonical type, language and culture do not necessarily have the same fates." Without historical evidence, he insists, the pattern of "extended changes in language..."
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.

-Glyn Cusack

How to contact the author of this article:
Glyn Cusack
Department of Anthropology
California State University
Hayward, CA 94542-3039
e-mail: glynco209@aol.com

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

How to contact the principal of this article:
Ruth Grunh and Alan Bryan
Department of Anthropology
University of Alberta
Edmonton, AB T6G 2H4, Canada
e-mail for both: rgruhn@maildropsrv.ualberta.ca

Suggested Readings
Arnold, Brighton 1957 Late Pleistocene and Recent Changes in Land Forms, Climate, and Archaeology in Central Baja California. University of California Publications in Geography 10(4):201-318.
Lavalier, Daniele 2000 The First South Americans. University of Utah Press, Salt Lake City, UT.