Reports

An Interview with Richard S. MacNeish

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[*Introduction*: Richard Stockton MacNeish, known to friends and colleagues as "Scotty," was born April 29, 1918. His name has become synonymous with research into the origins of agriculture. He was educated at the University of Chicago, from which he received his Ph.D. in 1949, and focused on the prehistory of Mesoamerica. Employed by the Canadian government from 1949 to 1962, he administered its archaeological division. He is honoured in Canada especially for his creation of a unique department of archaeology at the University of Calgary, which he ran from 1964 to 1968; it was designed to teach archaeology as archaeology, not as a branch of anthropology, and continues to produce fine scholars. His research quickly expanded to include Peru, the American Southwest, and, most recently, China. MacNeish served as the director of the Robert S. Peabody Foundation for Archaeology from 1969, a position he retired from in 1983, and taught at Boston University from 1982 to 1986. At the time of his death, following a car accident, on January 16, 2001, he was the director of research of the Andover Foundation for Archaeology. Most of his many archaeological publications are classics. The honours bestowed upon him have been many and varied, but in his curriculum vitae he proudly listed among them the Golden Gloves boxing championship he won in 1938 in Binghamton, N.Y. Because Alfred V. Kidder was MacNeish's mentor from his teenage years onward, he especially cherished the Alfred Vincent Kidder Award, which he received in 1971 for his groundbreaking discoveries into the process that led to the domestication of corn. MacNeish's unique personality is best described by his colleague and friend of many years, Kent Flannery, of the University of Michigan, who wrote on November 9, 1997: "Scotty is a fun-loving, overachieving archaeological legend who moves into a new region, works out the whole prehistoric sequence, and leaves when there is no more beer left in the area." This interview was conducted in September 1997.]

HF: How did you get into archaeology?

RSM: In my school in White Plains, New York, I had a teacher in eighth grade, Miss Ives, who conducted a very good class on art history, which dealt with the usual Greco-Roman art. At the end of this course she told us to make notebooks on some other art tradition. I picked "Maya" because my name starts with M. I did nothing about the notebook assignment until the night before it was due. I then enlisted my family to go through all the *Science Newsletter, National Geographic*, and *Scientific American* issues in the house and cut out pictures and help me make a notebook. Some of the articles were on Carnegie's work in the Yucatán and others were by Alfred Kidder. I handed in my notebook and went back to worrying about playing third base and other more important things. At the end of the semester, a prize was given for the best notebook, and lo and behold! I won the prize. Then I actually read some of those articles, because Miss Ives made me give a speech about the Maya. So, at the age of 12 I became involved in Maya archaeology. That was in 1930.

HF: What changed the course from the 12-year-old boy's fascination with Maya art to the adult's quest for the origins of agriculture?

RSM: My interest in Maya archaeology lasted throughout my high-school years. Then I had a choice between entering dull academia or doing archaeology my grandfather's way. He sold clocks for many years and became a millionaire and then did archaeology as a gentleman archaeologist.

HF: Was this the same grandfather who introduced you to Alfred Kidder?

RSM: Yes. He was selling Ingram's Roman clocks in Mexico and once visited the Yucatán to observe the archaeological dig in progress at Chichén Itzá. He talked to everybody there, including "Doc" Kidder. When he came back he showed me the photographs he had taken of the dig and of Doc Kidder. I was 13 years old and wrote Doc Kidder a letter asking him for a job as a water boy. And believe it or not, I got a letter back in which he informed me that he had lots of water boys, but he encouraged me to keep studying hard so I would become an archaeologist some day. But when it came time to go to college, I worked in a clock factory for a while. Then, secretly, my father got me a job out West as a water boy and mule packer in an archaeology camp. My father was a mathematics professor, and the idea that his son might not go to college was abhorrent to him. So he did anything at all to get me into college—even if it was to study archaeology. I was something of a "bad boy." My mother had died and we had a stepmother, so my adolescence

was a period of great turmoil. I was in revolt against everything: the capitalist system, the middle class of Scarsdale, New York, and Princeton, where I was supposed to go and in which I had been signed up when I was born because my mother's ancestors, the Stockton family of New Jersey, had not only signed the Declaration of Independence but also founded Princeton. Instead, I hobnobbed with Negroes and Italians. I did not like dancing school, where I got into fights all the time, so finally, in desperation, my parents took me out of dancing school and sent me to learn boxing instead.

HF: And did you like that?

RSM: You bet I did. That was much better. In fact, I paid my way through college for a while by fighting bootleg boxing.

HF: The shape of your nose confirms your story.

RSM: You're right—and so do my teeth. Later, at Colgate College I became a Golden Gloves boxing champion. I also fought in Chicago as a "white villain" in black athletic clubs, and I even coached boxing and wrestling. In Chicago I got $25 for a fight. That kept me going for a month at least. In my first fight against a black fellow I decided not to slug it out with him, but I jabbed and retreated. So I was booed roundly, but I won on points. When I went to collect my money from the little Italian racketeer, he told me proudly, "They hate you. From now on you get 50 bucks a fight." So I did. A meal was about 25 cents, so I had a lot of money. The turning point came when I went to the Rainbow Bridge (Monument Valley) excavation in Arizona. It was my father who had organized this for me. This was like a boys' camp, and people paid to go there. I was 17, I was away from the family, I didn't have to take a bath, and I hung around with Navajos and could visit all the ruins that Doc Kidder had dug. This was in Tsegi Canyon, and I visited every site he'd ever dug. I was paired with a Navajo boy, Jerry Little Salt, to pack mules. I had never seen a mule in my life. Jerry made me fall off the mule and told me to push it from behind, so I got kicked. But I could run. So when the Navajos and I came back from one of those trips on muleback, I decided we should run the last five miles to camp, and that made Jerry pant and suffer, and I got some respect after that and we achieved peaceful coexistence. He became my buddy, and we goofed off a lot—doing what Lewis Binford would make into ethnoarchaeology. Three days a week I was learning to dig, and three days he and I had to bring the week's supplies to the camp at Cobrahead Canyon. On the way we'd stop at his village, and I'd sit under a tree and watch people making rugs, women breast-feeding their babies, and I even saw a Navajo sand painting being done by a shaman. That world is now all gone. When I returned to this camp the second year, I was now a veteran and even had a Navajo name, which was Hotiazie. It means "Little Cottontail," to signify that somebody with a white rear end had won a Navajo race at their big annual gathering in Monument Valley. During the second season we dug a big burial site full of Pueblo III pots near Kayenta, a major trading post in northeastern Arizona where the Wetherhill family had been the first white people among the Navajos. They had found the cliff dwellings at Mesa Verde and been the first to invite archaeologists to investigate them.

HF: Was there any objection from the Navajos to burials' being dug?

RSM: Oh, yes, the Navajos were absolutely terrified, because they believed that we were letting the spirits out of the graves. They knew that these were not Navajo burials and that the bones were those of the earlier cliff-dweller people, but the fact remained, spirits were spirits. They would help us with the back dirt, but the moment we hit a bone they'd disappear. Each one of us kids had his own little burial to dig. In the middle of the summer an announcement was made that Doc Kidder would visit the site. He was coming to see the Wetherills, whom he had known for many years, and he was also going to make an inspection tour of our camp. Long before he arrived I, of course, had bragged that I knew Kidder and had even received a letter from him. So when he had inspected the burials we were digging, he was introduced to each one of us. When he got to the end of the line, he turned around and walked back and stopped in front of me and said, "Don't I know you?" He remembered my letter. Then he said, "Looks like you are moving in the right direction." That made my summer.

HF: Kidder played a very important part in your life.

RSM: He became very much of a father figure for me. I did not meet him again until the 1940s, when I started to work in Mexico. Later, when I wrote a monograph [MacNeish 1954] on an Early Formative site near Tampico in Mexico and was told that we needed someone to write the preface, Kidder said, "I know Scotty, I'll write the preface." And he did. I had begun to see him at professional meetings, and by mid-1950s, when I had a fellowship at Harvard, he had retired. Gordon Willey and I would go to his place every Friday afternoon to have cocktails. By then I was a full-fledged archaeologist.

HF: Did World War II interrupt your work?

RSM: Yes, of course, especially because while serving in the U.S. Army I contracted a virus which gets into your spinal cord and causes Landry's paralysis. It paralysed my arms and legs almost completely. One day a priest walked into my room wanting to pray for me because I was dying. I told him I was an atheist with no intention of dying. Actually, if it hadn't been for his coming in and making me so damned mad, I might actually have died. A little later I found out that he had once run a boxing club in Milwaukee and so we could talk boxing. Slowly, as I became a little better, we began to play blackjack. My hands were still paralysed, but he turned over the cards for me. And so he became my buddy, and religion

was never mentioned again. Eventually, I recovered fully; I guess the virus just gave up on me.

HF: How would you describe Kidder's significance for American archaeology?

RSM: He represents the transition period from antique collecting to doing scientific archaeology in the period between 1912 and 1930, when archaeology began to flourish in the American Southwest, the first pottery-type sequence was worked out, and multidisciplinary research began. He was the founder of good chronology. He had various ceramicists who worked with the native people in order to learn their pottery techniques, and this encouraged a positive relationship to the native cultures. At that time, the Jemez Pueblo people were still living at Pecos. He was digging up their history and had a very good relationship with them. He was especially interested in keeping good, solid records and writing careful site reports. This was the beginning of scientific archaeology. The next phase in American archaeology came under President Franklin D. Roosevelt's administration with the Works Progress Administration (WPA) during the Great Depression, which helped to train an entire generation of American archaeologists. James Ford, Gordon Willey, Jesse Jennings, Joseph Caldwell, Robert Braidwood—they were all on WPA. Alex Krieger's first archaeological work in Texas was also under WPA. He was a little like me, young and on the periphery of the big shots.

HF: How and when did academic training connect with dirt archaeology for you?

RSM: After working in the Southwest and going to Colgate, I transferred to the University of Chicago, which had a big training program, not only of the classroom variety—they also owned a large site in Illinois called Kincaid. It was sometimes referred to as "the black hole of Calcutta" because every graduate student went there to become a slave labourer on this set of mounds located in a steaming swamp. The most important of the teachers there was Roger Willis. This was my training period: there I moved in five years from digger to chief bottle washer to supervisor and wound up in charge of a lot of mounds and 80 WPA men digging them [Cole et al. 1951]. About this time, archaeologists in the Southeastern United States began to find things that looked very Mexican, suggesting that influence from the Mexican high culture reached the Moundbuilders in the Southeast. The mounds of Kincaid, with their Mississippian culture, were really truncated Mexican-style pyramids, but nobody had ever looked for the connection in the area between the Mexican high culture in the Southeast and that in northern Mexico and East Texas, where the mounds start. So I explored this area for Mexican influences coming up to the eastern United States. Alex Krieger had the comparative material from his work in East Texas on the Moundbuilders. This problem became the focus of my doctoral thesis [MacNeish 1949].

HF: Tamaulipas?

RSM: Tamaulipas was a very interesting place because half of it was not part of Mesoamerica: it had no pottery, no pyramids, but lots of spearpoints. So nobody was interested in this place. I came to realize that much of what I was finding was from before the time of the Mexican high cultures and their pottery, pyramids, and villages. Also, in the last and northernmost series of high-culture ruins there was a big canyon in which there were all sorts of caves. Initially, when I explored these a little, I found that beneath the pottery remains there were earlier artifacts. I realized that I was dealing with an unknown era of Mexican history, namely, the prehistory of the ruins. In the second year, 1949, when I received my Ph.D., I found La Perra Cave. There, underneath the pottery, which turned out to be 4,000 years old, I found tiny little corncobs. Later, in the early 1950s, the recalibrated carbon-14 dates put them at around 5,000 years.

HF: How did you find Tamaulipas?

RSM: Tamaulipas was rough, rough territory. First I was driving all over in an old car, asking people if they had ever seen any old ruins. I was told about ruins out in the wild where cars could not go, so truck drivers took me into the mountains to villages that had no electricity or plumbing but plenty of bandits. In all the bars people had pistols, and there were lots of fights. This was tough country. The son of the mayor of one of these towns, Los Angeles, had refused to be a farmer, much to his father's disgust, and spent most of his time exploring the mountains in search of wild honey which he sold to the truck drivers passing through the town. He knew the mountains, and he took me out to the ruins, mostly pyramids, which date to the Classic period from the time of Christ to about A.D. 1000. His name was Pedro Lerma.

HF: As in Lerma points?

RSM: Yes, he lent his name to that early culture. That first night out in the ruins, it was too late to return to Los Angeles, and we spent the night in a cave. The floor was littered with spear points, and by now I knew that these came from before the time of pottery and the high cultures. I asked Pedro if he knew of any more caves. (I speak a bandit-type Spanish and disgrace myself regularly before my colleagues who speak decent Spanish.) Pedro knew many more caves. Since there was no water anywhere and there were lots of jaguars and snakes, this trip required careful planning. Pedro Lerma showed me all the caves—and those were the caves of Tamaulipas. Eventually, we dug caves that had five and six different layers, all sorts of different cultures on top of each other, and prepottery sequences appeared which few people then were interested in. That was in 1945–46. When I returned in 1949, my assistant, Alberto Aguilar, found those tiny corncobs. La Perra Cave, which was way the hell up a steep mountain and a challenge to dig, had nice layers of pottery in it, and it was so dry that all the plant

remains were preserved. There were also chewed pieces of string and baskets. Below were spear points and more early plant remains. I was running out of money, and another dig was soon going to start for me up in Canada, so I told him to close the dig and meet me in town. We walked all night long to the next cave down the canyon, and he closed the dig there also, while I went down into town. He had also been told to pack the jeep, since we were going to leave this area. But when he arrived, the jeep was obviously not packed, and when I asked him why, he said, "I found what you said we were going to find." He handed me three tiny, tiny, primitive corncobs wrapped in string. They had come from a layer that I thought was about 4,000 years old. So we unpacked and went back to the canyon and dug La Perra Cave. At the end of the season, I took everything in to Mexico City to give to the authorities [MacNeish 1992:xii–xv].

HF: How did you get those tiny cobs dated?

RSM: Back at my old alma mater I had met W. F. Libby, who had been an instructor in physics in a class I had attended. He had developed carbon-14 dating in 1946, and he won the Nobel Prize for this in 1960. (Parenthetically, I should say that I have experienced the carbon-14 revolution from its inception in the 1940s to recent examples of its abuse on my own material, as I will tell you later. Apparently, many anthropologists fought its acceptance tooth and nail. It was most upsetting to many because its results didn't agree with prevailing estimates of chronology.) In Mexico City I was asked for some charcoal, and I gave them several bottles from each layer. They gave Libby the wrong bottle for the corncob-bearing layer, so the first date came in as 1,000 years old because it was derived from the charcoal taken from the *top* layer. In the meantime, the corncobs themselves went to Paul Mangelsdorf, a botanist at Harvard. There we got a second date from the cobs themselves, and this date made them 4,445 years old. Meanwhile, I was back at work in Canada, and when I got news of those dates I returned to Harvard to see Mangelsdorf. He asked me to lay out the corncobs in the sequence of the layers from which they had come, and when I did he exclaimed, "That's exactly right!" The sequence in which I had dug them up fitted with his theory about how corn had evolved through time. This was late 1950. Mangelsdorf was very excited by this and said, "Scotty, let's go together and solve this problem [of the domestication of corn]. You've got the energy to go into the field, you know how to find caves; I know the genetics, and I know how to get you the money." We knew that the solution lay in some cave in a valley between northern Chile and the St. Lawrence River—so all I had to do was find the right cave and the right valley!

HF: Where does Canada fit in here?

RSM: I had finished the Tamaulipas dig for the time being, earned my Ph.D., and also got married. Just before leaving for Tamaulipas the second time in 1949, I had had a fellowship at the University of Michigan for the purpose of studying early Iroquois pottery, and that had taken me to Ottawa in Canada. Douglas Leachman at the National Museum in Ottawa knew of my Iroquois work and asked me to look at the Iroquois collection there, which W. J. Wittenberg had originally dug up. While we were going down to the basement, he asked me why I was going to northeastern Mexico. I explained that Tamaulipas was one of those areas archaeologists always talk about but never go and actually look there.

HF: For very good reason—you have to haul your own water up every canyon and sleep with rattlesnakes.

RSM: Exactly. Not everybody's cup of tea. I was about to get myself into another situation just like it. Leachman didn't understand what I had said about Mexico, so I explained that the prehistory of Mexico was similar to the situation in the Mackenzie River area. Archaeologists always talk about the migrations of early peoples down that river, but nobody actually goes there either. I said, "That's the kind of place I am interested in, and that's what Tamaulipas is all about." Then he left me to study the Iroquois pottery in the basement. At lunch he said, "I've talked over your project with the head of the museum." I was dumbfounded. Why would the National Museum of Canada possibly be interested in Tamaulipas, Mexico? He said, "Dr. Alcock has worked in the northern part of Canada and is very interested in early migrations." So I asked, "Where?" and he replied, "Oh, you know—the Mackenzie River." I only vaguely knew the geography of that river. Then he said, "When we get back from lunch I have an appointment with you to talk about your project with Dr. Alcock." I was shocked. I excused myself to go to the bathroom, but actually I ran down to the museum library and asked the librarian for a map of northern Canada and told her I needed to know something about the Mackenzie River and the Indians there. She handed me a map and Diamond Jenness's [1932] book on the Indians of Canada. So I glanced at that map and saw Great Slave Lake, Great Bear Lake, and the names of the Indian tribes inscribed here and there. I ran back to meet this great old explorer, Alcock, who had been in the Geological Survey of Canada and now was the head of the National Museum of Canada. As soon as I started to talk about the Mackenzie River, he said, "Oh, yes, Yellowknife is a great place." He just loved the north. Then he said, "Fine. We'll send you the papers." As I later found out, these were civil service papers for the position to explore this river for the National Museum of Canada. My job was to look for the migration route of the American Indians down the Mackenzie River, and I was a Canadian civil servant. The Canadian government employed me from 1949 to 1963.

HF: How on earth do you begin to look for the prehistoric migration routes in such a vast area?

RSM: As Krieger [in MacNeish 1992:75] said, I had to

"learn to think like a [preceramic] Indian" first. So, in the Mackenzie I began to study the ecology of the area, reconstructing what locales would have been appealing to hunters and visiting such places in my exploration, and many sites turned up in just such places. I dug these sites every year. My job required completing a site report before I could apply for money for the next season's dig. That made me into a faithful writer of site reports.

HF: It is curious that you came to Canada because your ideas about the archaeology of prehistoric migration caught Alcock's imagination, yet your classic paper of 1952 [MacNeish 1952] rejected migration as an explanation for Iroquois origins.

RSM: Language research had determined that the Iroquois spoke a Sioux language and existed in an Algonquin-language sea; they were perceived as intruders into Ontario and New York. The theory was that the Iroquois had invaded this area around A.D. 1000 and pushed out the Algonquins and their culture. The Iroquois were thought to have come from the region around the mouth of the Ohio-Mississippi junction, which is where Kincaid was. Jimmy Griffin, who had originally given me a fellowship at Michigan, was involved in research on the Delaware River. He said, "Scotty, there is no archaeological material at the mouth of the Ohio that looks anything like Iroquois. This migration stuff is nonsense." Jimmy and I basically set out to destroy a theory that had stood untested for some hundred years. That's how I got into the study of Iroquois pottery in the first place and how I landed the job in Canada. As I examined the collections I began to find that there was early Iroquois, middle Iroquois, and early-early Iroquois, and then, without a break, there was a transition into Middle Woodland which nobody had ever thought of; some had called it Algonquin III. All of this was based on pottery seriation. Some 50 Ph.D. theses were written about this issue, some criticizing me on this or that detail, but they all backed me up on the basic claim that the Iroquois were an indigenous culture.

HF: What happened to your Mexican interests while you worked in Canada?

RSM: They never stopped. The Inuit used to say to me, "You are like a duck. You spend the summer in the north and fly south in the winter." If you worked for Canada's Geological Survey north of the 60th latitude, you got double pay because you worked Saturdays and Sundays, as the climate makes for a very short season. The National Museum didn't have any more money to give us, so they offered us extra vacation time. For every Saturday and Sunday that I worked north of 60°, I got an extra day added to my three-week vacation time and wound up with ten weeks for Mexico.

HF: How would you summarize your Canadian experience?

RSM: I did two things in Canadian archaeology. One was to get some sequences in the Mackenzie River and in the southeastern Yukon. These sequences told us mainly about local history and a little, but not very much, about early migrations because we didn't find a lot of very early material. Secondly, we took Canadian archaeology from about two people working on Canadian prehistory to the point where we had archaeologists working in every province, thanks in great part to Diamond Jenness, who became a very good friend of mine and was an adviser at the National Museum. The culmination of my efforts in Canada was founding the University of Calgary's department of archaeology, which has turned out some 40 Ph.D.s who basically still run Canadian archaeology. It has been said that I was Calgary's Dr. Frankenstein and that this department was my monster.

HF: In that classic 1952 paper you complained that anthropology and ethnology routinely imposed their ideas on archaeology. Did this determine your design of the department of archaeology at Calgary?

RSM: That's exactly right. This is why the Calgary department is turning out good archaeologists and most North American universities have a lot of crummy archaeologists instead! Even when I was still in Chicago, I felt that I was seriously lacking certain kinds of knowledge that I should have been taught, such as about soils, geology, palaeontology, botany, and so forth. Instead, I was forced to take courses in linguistics, phonemics, and other useless subjects such as Radcliffe-Brown's theory of kinship, all of which were rammed down my throat, none of which connected with anything I was interested in, and I wasn't at all sure that any of that stuff was even true. None of these theories can be dug up, you see.

HF: Did you feel that archaeology was being used to "prove" already formulated theories of cultural, social, and linguistic evolution?

RSM: Precisely. I consider what the anthropologists and ethnologists say as hypotheses to be tested by archaeological data. That was why in Chicago I was in revolt against the social anthropologists, but they eventually took over the department. Leslie White, who was at Chicago then and whom I got to know at the party level, was interested in evolutionary theories of culture, and these did pertain to archaeological sequences. Also, the notion introduced by Marxism that economic systems can change cultural systems made some sense to me.

HF: But your work makes no reference to Marxist notions of causation.

RSM: That is true, but White's ideas about cultural evolution and Marxist theory were at least compatible with archaeology, while Radcliffe-Brown had nothing to do with archaeology at all. White's ideas were materialistic, and archaeology is a science that focuses on material evidence.

HF: How do you see the difference between the approaches of Radcliffe-Brown and Leslie White?

RSM: First of all, the notion that an economic system can change the subsistence system was of key importance to White and Marxism. For the Radcliffe-Brown people social organization, kinship, and value systems were primary, while economic systems were of minor significance. These views were opposed to each other with regard to basic causation. But a compromise was made possible between them through Julian Steward's theories on cultural ecology and his ideas about the relationship between humans and their environment. This was quite different from Radcliffe-Brown as well as from the Marxists. Intellectually, I am a descendant of Julian Steward. I am an archaeologist and a cultural ecologist.

HF: How did you persuade the University of Calgary to begin training archaeologists your way?

RSM: In Ottawa I was rising in the system and becoming more and more of a bureaucrat and had less and less time for dirt archaeology. I didn't like that. Then we had a major scandal in the museum which made headlines on December 27, 1958, in *The Telegram* [later *The Toronto Star*] one-and-a-half inches tall—the same size as the World War II headlines—reading TOP BRASS DISSENSION ROCKS NATIONAL MUSEUM. That was when "Uncle Louis"—Louis Saint Laurent—was Canada's prime minister. When Alcock retired as head of the museum, the government decided it would get a French Canadian to replace him. It found a botanist from the Montreal Botanical Gardens for the job. The civil service commission didn't accept him, so I found myself in the middle of the French Canadian problem and all hell broke loose. Somehow, the press became involved, and the reporter assigned to the job was very clever. He waited until late Friday afternoon, when he knew that the deputy ministers, whose permission museum employees needed to talk to the press, would be gone for the weekend. But I saw this coming and told him I could not talk to him. So I wound up with a little picture on the second page and the caption "An American Who Won't Talk." Most of the others talked—and they were all fired. This mess made me really fed up with the museum. However, in the meantime I had made friends with a man named Eric Harvey, a Calgary billionaire and passionate collector of Indian relics and antiquities. He was a lawyer from London, Ontario, who went to Alberta in the 1930s. There he did wills and deeds for farmers who homesteaded the land out there, and as the wind blew away the soil they couldn't pay him and he had them sign over the mineral rights on their farms to him. And that's how he became a billionaire. By 1956 he was coming to Ottawa regularly to consult with people at the museum. He asked for my help in finding Indian antiquities. He took a suite at the Chateau Laurier Hotel and invited me to lunch. But before I met the grand old man himself, he sent two roughnecks to check me out. The boss wanted to know if this American could be trusted, so they took me downstairs to the bar to get me drunk and make me talk. Well—I put them both to bed! Next morning they told Harvey, "This guy Scotty is a good man," and I became Mr. Harvey's adviser. He purchased a lot of material which became the core of the Glenbow Foundation, a major museum in Calgary.

HF: Did you know Marie Wormington?

RSM: I was actually in love with Marie Wormington from the age of 18 onwards. The first archaeological meeting I went to, in 1936, was held at Yale. The entire Society of American Archaeology was in one room. She was there, about 22 years old, a Harvard graduate, wearing tight riding breeches, and she was beautiful. She talked about her archaeological work in southern Colorado. Marilyn Monroe couldn't touch her! Well, I never touched Marie Wormington either, nor did Bill Malloy or Dick Forbis, but we all remained in love with her from afar. Later, when I began to find early-man [now First Americans] material, I always discussed my finds with her. Her knowledge was encyclopedic. Her looks were great, but her brains were excellent. She knew more about the subject than anybody else, and every visit was a learning session. At about that time I was to give a speech on early migrations at the University of Calgary. Harvey came down to hear me. Afterwards there was a party, and the president of the university and others came along, too. Sometime after midnight and many drinks, somebody said to me, "So, you are fed up with Ottawa." Everybody knew I was on my way to Tehuacán, and they said, "Maybe you are not going to come back to Canada, so how about becoming the head of the anthropology department here in Calgary?" I said, "That's the last thing I want to do. Anthropology departments can't train good archaeologists." So they said, "OK, then how would *you* train them?" So Dick Forbis and I told them how. Well, the president of the university right away suggested one guy from the physics department and another from the geography department and various other departments to join my interdisciplinary program, and by 4 A.M. we had dreamed up a department. Then I said, "This is all very fine and good, but it is going to cost you at least $300,000 to do this, and anyway, I am leaving for Tehuacán and can't do it." By 11 A.M. the next morning I got a call from one of those two roughnecks I had put to bed in the bar of the Chateau Laurier Hotel. He told me that Mr. Harvey would like to meet me at the Calgary Curling Club "to discuss your department." So I thought I'd better put a stop to this before it was too late, and when I met Harvey for lunch I said, "You have to pay for a library; that'll be at least $50,000." Harvey replied, "No problem, you've got your library." Then I said, "If you want anybody to come out here, which is basically nowhere, you have to have fellowships, and that's another $10,000 each." Harvey was unmoved and said, "We'll get you six fellowships to start with. I'll get Imperial Oil to pay for three." So I said, "And then you need a summer field school; that's at least another $10,000." Harvey said, "Fine, you've got your summer

field school." Finally, I said, "But I am leaving for Tehuacán!" Harvey replied, "That's fine. That'll give us a year to get the library ready." And when I returned, everything happened just as he had promised. The first year we had 6 graduate students, and the following year we had 12. And I was busy—when we had 24 graduate students, 13 of them were writing their theses under me, and each had three hours a week of my time.

HF: How did you design the archaeology program at Calgary?

RSM: All the courses were designed specifically for archaeologists: soils for archaeologists, geography for archaeologists, physics for archaeologists, and so on, and six of these were the minimum requirement towards earning a Ph.D.

HF: Were you consciously correcting the wrongs you experienced at the University of Chicago?

RSM: You bet I was. When I was a student at Chicago you did not stray from anthropology. Nobody—and I mean nobody—took a course over here or over there. I did this, of course, and my professors would rein me in and inform me, "Scotty, you are neglecting anthropology. We don't want you to waste your time with [Alfred] Romer's palaeontology courses." He was leaving that year, and I took the course just before he left. Since I had taken every anthropology course Chicago offered, my supervisor, Fred Eggan, suggested I go and have a look at Romer's three-semester vertebrate palaeontology course and use it towards a minor. To my shock, I realized the first day that this was designed for the graduate students of the palaeontology department. I got a C the first semester and a B the second because by now we had moved out of those darn amphibians and were into reptiles and mammals, and at the end of it we had the most fabulous exam. I will never forget it. We came into the room, and here were all these tables lined with some 500 boxes and a fossil in each box. The task was to tell what part of which animal each item came from, what species it was, and what geological formation and period it had lived in. The exam started at two in the afternoon, and we were told that the building would be locked at midnight and that until then we could take our time. I got an A.

HF: This experience must have been like a rehearsal for the fieldwork of your entire life.

RSM: That is exactly what it was. Being able to identify those fossils and really knowing chronology stood me in very good stead for the development of my interdisciplinary approach. That is how later I was able to attract students like Kent Flannery [now at the University of Michigan] to my digs to identify bones, and it enabled me to have fruitful discussions about the sex and age of the animal bones, and so forth, all of which is crucial to reconstructing seasonality and length and frequency of occupation at a site.

HF: After all these years this issue is still a problem. Recently, an opinion piece in *Nature* lamented the fact that interdisciplinary communication is basically nonexistent and therefore knowledge is seriously impeded.

RSM: That is very true. I have lived through three stages of this problem of interdisciplinary communication. The first one was the most frustrating one, when I was a student at the University of Chicago and I wanted to learn such things as aerial photography interpretation and plane-table mapping. The second stage was most helpful, when I started work with Paul Mangelsdorf; he would look at the plant remains I spread out before him and say, "Ah, this is a bean, and that development goes so-and-so." I learned about all those experts in various other areas of research on whose knowledge one could call. So when I got my Guggenheim Fellowship in 1956 and went to Harvard, I finally sat in on such courses as Pleistocene geology—information I had wanted when I was doing my doctoral thesis.

The third stage was when I went to Tehuacán. I realized that we archaeologists treated scientists from other disciplines all wrong. Traditionally, we would pack all our bones from a dig into a box and send it over to some professor who had a busy career and was supervising some dozen doctoral students. We would ask him to identify the bones at short notice, and as a reward he would be offered an honourable mention in the appendix of our monograph. Well, for good reason these guys would often just not do it. I realized that getting information from other scientists had to be a situation of reciprocity. You had to make it sweet for them. They had to have freedom to write a whole chapter in the final publication on the dig, and they had to be free to interpret their data as well as mine in that context. Their trip had to be paid for, and their reputation had to be enhanced by this entire exercise. So I invited them to Tehuacán, gave them a guide and a jeep, and said, "Work on what you want to work on." Then, at about 5 P.M. every day we'd all go and have a bath and meet at the hotel for a drink and I'd ask them, "What are you guys finding?" And they'd be bubbling with enthusiasm and information because they'd be finding stuff that they had never seen before, and they would already be planning various papers for their own journals as well. Best of all, we archaeologists provided the temporal dimension (we could give them the oldest pumpkin, the oldest squash or gourd) and provided them with a context none of their colleagues had ever had before. Finally, we could all sit down together and discuss the process that might have led to the domestication of these plants. And, of course, we treated the bee man the same way when he arrived two weeks later. Then we'd put the bee man in touch with the pumpkin man and everybody with the amaranth man, the corn man, the wild plant man, the geologist, the zoologist, and so on. That became very stimulating for me and tremendously educational for us all. I love listening to other scientists talking about their discoveries.

HF: But how did you pay for it all?

RSM: Ah! Mangelsdorf taught me how to write grant proposals. I learned from him that there were other sources of money besides the National Science Foundation. There were, for example, the Luce Foundation, the Rockefeller Foundation, and the Dekalb Seed Company Foundation. This last one was interested in hybrid corn because it had got the idea from Mangelsdorf that teosinte could help to make new sturdy hybrids; it was translating the information it got from plant geneticists into actual money. Then there were stockbrokers and other businesspeople who were interested in science and archaeology and willing to pay for it. Today, we have an interested public as well willing to back archaeology. All of this gave me a lot more freedom for my research projects, and I didn't have to bother with all the politics that interferes when government money is involved. In fact, the new administration at the National Science Foundation wouldn't give me a grant if I walked on water and they saw me do it. You see, I believe in establishing detailed chronology first and formulating theory second, which does not seem to be what they approve of.

HF: What was your relationship to Walter Taylor [Taylor 1948], whose chief criticism was of too much focus on chronology and typology?

RSM: I found him very interesting. His criticism was often compatible with my own thinking. He took on the greats of the day—Alfred Kidder, Jimmy Griffin, Joe Brew, Ben Rouse, Bill Ritchie, and others—and wrote criticisms of them. He said that they were *only* interested in chronology and typology and that they were not doing anthropology, namely, reconstructions of the ancient cultures. He also criticized them for ignoring a lot of very basic data, including food and plant remains such as those he had dug up in the caves in Coahuila, the state to the west of Tamaulipas. His caves were very similar to mine, and he had the same kinds of bugs and local bandits to fight off in that very rough country. (I might add, Taylor never wrote up the finds of his dig.) It was true what he wrote, namely, that archaeologists rarely tried to interpret the information found in a specific layer. We had ignored all this reconstruction of the contextual data. Taylor was right in saying that we could not do any theoretical work if we hadn't done the contextual work, such as ethnographic analogy. He also wrote about all the things he was going to do with his basketry finds, and so on, but most of this was really impossible to do within a lifetime. He was suggesting what *could* be done if one had the resources. He did impress and inspire the up-and-coming generation. Much of his youth was spent in France, and he spoke French fluently, as well as German. He had worked in the American Southwest and become very frustrated with the emphasis on chronology and typology, so when World War II started he joined military intelligence and got dropped into France. There the Nazis literally captured him as he came down with the parachute. First they debated whether they should shoot him as a spy or not, but the Americans weren't in the war yet, so they decided not to shoot him. They put him into a prison camp in some godforsaken part of Poland, and there he began to write his Ph.D. thesis from memory.

HF: That famous Ph.D. thesis was written in a Nazi prison?

RSM: Yes, that's right. The conditions under which he wrote the bulk of his thesis were psychologically appalling. In the first camp he was joined by Australians and British prisoners who did not believe that he was an American but because of his fluent French and German remained convinced he was actually a Nazi planted to spy on them. He was ostracized for a couple of years. Towards the end of the war he was made into a prison guard watching over older prisoners. When the Russians invaded Poland, he told the old men that he wasn't going to wait around for the Russians to arrive but was going to escape at dawn and try to walk across Germany and join General Patton, who was by then at the Rhine River. This he did—and wound up with the British army in Belgium. That thesis, which was finally completed at Harvard, was the product of a heroic endurance test.

HF: How do *you* believe cultural change takes place?

RSM: I follow Julian Steward [Steward 1955], who long ago identified two kinds of theories—big, general theories which attempt to explain all culture change in terms of a particular variable such as population growth or environmental change and theories based on small generalizations, such as changes in a matrilineal band which might bring about specific little shifts with a cumulative impact in the long run. Hypotheses about such little changes can be tested with comparative data. What I am asserting is that we do not have a large explanatory theory that is capable of explaining all change.

HF: Where do you part company with the New Archaeology?

RSM: My criticism of the New Archaeology is that it ignores all these many complex strands of explanation in the interest of formulating big theories of culture change which do not, however, lend themselves to being tested. But even worse, the New Archaeology is working hand-in-glove with cultural resource management archaeology, which is a disaster for American archaeology. Business has invaded archaeology, and theory is out the window, as is good methodology.

HF: Textbooks still mostly prefer discussing single-trigger explanations. But you and Kent Flannery [Flannery 1986] use the more complex notion of *positive feedback*.

RSM: Flannery and I are blood brothers. We have many resemblances to his "Old Timer" [Flannery 1982]. By the way, Kent would violently object to being called a New

Archaeologist, although he does make some generalizations about agriculture, religion, and empire building which, if tested, could yield some larger generalizations. The concept of positive feedback allows us to get as close as one can get to a real generalization about culture change. My irritation with large generalizations was immortalized in a funny way by Kent in his allegorical paper entitled "The Golden Marshall Town" [Flannery 1982]. There he has a theorizing archaeologist say that he does not "need to break the soil periodically in order to reaffirm my status as archaeologist"; to this the Old Timer says, "I think I just heard 10,000 archaeological sites breathe a sigh of relief." That is actually verbatim an observation I once made to Kent Flannery about a colleague who shall remain unidentified.

HF: If God is in the details, as the architect Mies van der Rohe used to say, the Devil may be in the generalizations, which are mostly rather boring, wouldn't you agree?

RSM: Yes, very much so. That's why I am rather specific in my interests. I am focusing in all my work, for example, on the *lowland* cultures of China, the *lowland* culture of the Tigris-Euphrates region, the *lowland* culture of the Maya, having earlier focused on the highlands of each of these areas. I am interested in testing hypotheses with examples that all have ecological features in common. The New Archaeology tends to ignore this specificity and homogenizes it out.

HF: How would you describe your ecological approach?

RSM: There have to be, in my observation, certain kinds of necessary conditions to bring about agriculture, and these are very definitely ecological. You have to have certain kinds of seasonality and certain types of microenvironments in which potential domesticates—plants and animals—are at least available. So we are talking about very simple levels of culture in very harsh environments. There also have to be certain kinds of eco-zones next to each other but far enough apart that they can't be exploited from a single base. Furthermore, the climate has to have very definite seasons. There are lots of places around the world where these conditions can be found. In contrast to the harsh marginal environments you find lusher areas which can be exploited in just one day from a single base. Such eco-zones present no problems for obtaining food. The two types of eco-zones show sedentism developing along quite different paths. The lush ones get sedentism well before full-blown agriculture develops. People there don't need agriculture, but they are sedentary. This is the route to agriculture through foraging affluence. The other route shows that sedentism develops only after agriculture has developed sufficiently to allow remaining in one place throughout the various seasons.

HF: So, in the first case people are drawn together in a Garden of Eden, and in the second case—

RSM: —you build the Garden yourself.

HF: What a neat way of uniting the idealistic viewpoint with Marxist ideology!

RSM: Yes, that is true. But then there is a *third* pathway which is like neither of these two, and we find it in Europe, Africa and the Eastern United States, and Japan. There people were fairly affluent foragers who had domesticates in their environments but didn't have to depend on them—until they had totally wrecked their environment or their environment got ruined by some sudden climatic change. But of course, these are ultimately also all feedback situations. For example, as people moved down the Danube River valley, they cut down more and more of the forest, which meant that their hunting became an increasingly less reliable source of food. They inadvertently eliminated valuable plants through deforestation, which made collecting less and less profitable. So, suddenly they were faced with a crisis, and they took up agriculture from their neighbours or moved out of the territory completely.

HF: The "necessity" trigger does have its archaeological examples?

RSM: Oh, yes, it certainly does. I just refuse to universalize it for all the world. I maintain that we find in our studies of the origins of agriculture three different triggers and three different systems in three different types of environments. Furthermore, the population-pressure model also has its archaeological examples, such as when we see camps getting bigger and we see a single valley being exploited with increasing regularity and then, suddenly, we see an agricultural system such as that of the Basketmaker II people. But the situation may be even more complex. I am not sure if the affluent path to the Neolithic doesn't itself also have some significant differences in Africa, Japan, and the American Southwest that we need to understand in more detail. I don't believe the population-pressure model can be understood as a linear event; rather, it's a result of a disturbed equilibrium in which climate and population growth interact. Again, we have here the concept of positive feedback at work. But, you must realize, the ecologists have been saying this for a very long time. Anthropologists just haven't read them. There is a real resistance to explanations that are not based on a prime mover. The unlinear models need their *deus ex machina*, whereas the multilinear approach doesn't.

HF: How do you feel about David Rindos's [Rindos 1980, 1984] model of plant and human symbioses?

RSM: I see it as a model in which agriculture begins without human beings' even being involved in the process. These plants somehow evolve towards domesticated status without Mama's planting them. Rindos does not take into account the seasonality of plants and the human beings who plant and select them. Domestication

is not a natural phenomenon; it is the result of human selection. I totally disagree with Rindos because he leaves no room for any kind of cultural involvement.

HF: Doesn't Rindos have a point when he sees the innate tendency in animal and human behaviour towards manifesting pattern and order as determining continuum between them?

RSM: Of course, but Rindos ignores the animal's *involvement*, including that of the human animal. He's got a problem with conscious interaction.

HF: In Flannery's whimsical "A Visit to the Master" [Flannery 1986:516] the Master says, "I gather that for you the word intentionality may still have a place in human adaptation."

RSM: Exactly my view also, but Rindos did convince me that plant genetics was important to the interpenetration between intentionality and the domestication potential in the plant. From then on I paid much more attention to it. I also warned him that there would be Young Turks coming along who would try to take him apart just as he tried to take me apart. He died recently, rather tragically, at a young age.

HF: How did you develop your multilinear model of agricultural origins?

RSM: When I began work at Tehuacán [MacNeish 1967–75] I became more and more interested in *why* agriculture had developed there. By the mid-1960s I had a set of hypotheses, and the next thing was to test them. So I went to South America to find a valley that was ecologically similar to Tehuacán and see if the same thing might have happened there. I had a buddy in South America, Tom Patterson, who was working on the coast. He became a good Marxist. As I began finding things in the highlands, he would say, "Scotty, you're all wrong. At the coast it happened like this, and for different reasons." I would go to Lima to meet him, get a bath and a rest, and argue and drink with him in the good restaurants his wife would find for us. Then, one day in 1970 he invited me to Yale to a class he was teaching. He put down the sequence of events he had found at the coast on the blackboard, and I put down the sequence I had found in the highlands on the other side. And then I said, "Good God, we're both right." I realized that agriculture happens one way in the lush coastal area and it happens another way in the marginal environment of the highlands. There was no *one* theory to explain it all. That was the beginning of my multilinear approach to agricultural origins. Actually, I should have thought of it much earlier, when reading Julian Steward's ideas on multilinear developments, but it really hit me only in 1970.

HF: How did you find Tehuacán?

RSM: We were trying to bracket Central Mexico, where corn was first domesticated. Somewhere south of Tamaulipas but north of Chiapas seemed to be the right area. In Chiapas we actually found some corn pollen that was about 7,000 years old. By this time Mangelsdorf had decided that the most primitive corn plants were all highland desert plants, so we were looking for highland desert valleys between Tamaulipas and Chiapas. That meant Oaxaca or Tehuacán, because the Valley of Mexico had corn material that was also about 7,000 years old. I went to Oaxaca first and didn't find any interesting caves, but since then, of course, Kent Flannery has done fabulous work at the famous site Guila Naquitz [Flannery 1986]. I had found the cave originally, but at the time I was looking for earlier material. By the process of elimination Tehuacán became the most likely area, and it did have lots of caves. After a brief survey, the very first test was done one weekend at the back of Coxcatlán Cave. We dug through some surface pottery and then, underneath this big rock, we found these tiny little corncobs. Eventually, we dug about seven or eight caves and a number of village sites there. The interesting thing was that we found caves at Tehuacán that were in different ecological zones, so we managed to get sequences with data on annual calendar rounds.

HF: Your corncobs from Tehuacán were recently dated [Benz and Iltis 1990]. There seems to have been some controversy about them [Fritz 1994, Hardy 1996]. What is your view on this?

RSM: We [Flannery and MacNeish 1997] wrote a detailed reply to the latest version [Hardy 1996] of that criticism. There is a story here about the dating of the corncobs from Tehuacán. When we found those corncobs, J. L. Lorenzo (who recently died) was the head of the prehistory department of INAH [Instituto Nacional de Antropología e Historia]. He demanded that we send all the corn samples we had dug up back to him. When he got them, he sprayed them with metacreal, a plastic which is used to preserve perishable materials. After they were sprayed, he sent them for dating to Arizona, so the dating process included rubber from the previous week. (I believe this was intentional.) All the sequences were totally crazy and thousands of years off the mark. Fortunately, we also had uncontaminated materials which established the correct chronology. We now have about 80 really good dates from Tehuacán and Oaxaca. I did not know that the first batch of corn which I sent to Lorenzo had been sprayed until CURRENT ANTHROPOLOGY published the criticism by Fritz [1994]. They never bothered to contact me first and just published the critique. I then went and asked people at the museum in Mexico City about these corncobs, and they told me exactly what had happened. It has been known since the beginning of the carbon-14 revolution that you have to be terribly careful with the handling of such samples. Our corncobs were picked up with tweezers, never touched by hand, put on aluminum foil, and placed in a sealed bottle for heating to remove all the moisture, after which the bottle was

vacuum-sealed for transport to the carbon-14 lab. In fact, even without preservatives' being sprayed on the samples, if you think you can take a corncob that's been kicking around in a lab for 20 years and get an accurate date, forget it.

HF: What do you think was going on there when those ancient people switched the heads of their dead?

RSM: In the El Riego phase in Coxcatlán Cave there is a burial some 8,000 years old. In it are the skeletons of a decapitated child and two adults, one male and one female. Sitting on top of one of those adults' chest we found that child's head. It was an undisturbed grave, and the dead were inside baskets. I suspect that when the male head of this band died, the tribe decided to get rid of his wife and child also. Perhaps they even thought that those dead bodies would help bring rain and make the corn green. We have some very fancy burial rituals in the Archaic period of South America going back thousands of years, unlike the Archaic periods in North America. In Tamaulipas we also found something fancy when we came upon a 4,000-year-old burial containing a male and a female, both wrapped in mats, with their legs intertwined. One of them had died, but the other one had definitely been killed to go along with the dead person. Ceremonialism starts very early in Mesoamerica. We find a long preparatory phase in the Formative period where people were making figurines, practicing human sacrifice, and building pyramids.

HF: This seems to be true also for the evolution of agriculture. Your work shows that dependence on agriculture also had long preparatory phases.

RSM: Yes, the evolution of high culture and the development of plant and animal domestication both had great time depth. The interest in those early wild plants stemmed from what appears to have been the supplementation of a diet primarily based on collecting food in the wet season. The interest was in individual plants, not fields being sown. The animals of the Pleistocene had all become extinct by now, and the El Riego–phase people began collecting in a seasonal-round system which followed the annual calendar. When you work with native peoples, say, those in the Mackenzie River valley, you find that they know exactly when the acorns are available and when the skunk cabbage is ready. The same seasonal round and the knowledge of the environment that this implies is what we found in Tehuacán. The interesting thing is that by the very fact that people camped in areas the ecosystem was disturbed. The charcoal from their fires sank into the soil. Cactus plants were moved because they were in the way, plants that were not edible were thrown around, human faeces changed the nitrogen level in the soil, and so on.

HF: So who is domesticating whom?

RSM: That's what Rindos was getting at, and he was right there, but he wasn't taking people disturbing the ecosystem into account. Humans are hell-bent on maintaining their way of life, whatever it may be at any given time, and therefore they are a major factor in the positive feedback system. They are what keeps screwing it all up—the loose cannon in nature. This is especially true if a group of people decides it doesn't want to move around one year, and so it accumulates food to last through the winter season. The moment this decision is made, the old women and men who couldn't have made it and would have dropped dead and the mamas who were likely to have had miscarriages due to all that moving around don't drop dead and don't miscarry. Then the population increases, and more mouths have to be fed.

HF: What is your view on Cohen's [1977] idea of the "food crisis in prehistory"?

RSM: His original data were based on the lush areas of coastal Peru. My own findings are very close to his. This is what I think also happened in the Tigris-Euphrates area of the Near East, the Levant, lowland Mesoamerica, and southern China. Yes, this is pretty much a universal phenomenon. Cohen's observations fit well into what I call the secondary developmental phase [MacNeish 1992]. His approach is Neo-Marxist, namely, population changing the means of production. For me the process is much more complex. What I object to most in Marxist thinking and also in the ideas of V. Gordon Childe [1942] is that they are too simplistic. The Marxist scheme would probably work well in a lush area, but in a marginal area it doesn't work that well, and in a very harsh environment it wouldn't work at all. The environmental niches within a general cultural area complicate this system. In addition, you have to take into account factors such as trade with other areas and contact with other domesticates. Even my own feedback diagrams are most likely too simple. This complexity applies to the potential domesticates among the plants also. For example, the notion that corn descended from its closest wild relative, teosinte [Doebley 1990, Doebley et al. 1997], is still being defended, but it is far too simple and linear.

HF: How complex do you think the domestication process of corn really was?

RSM: Prior to Mangelsdorf's work the idea was that teosinte was the answer to the question of the origin of domesticated corn. Much earlier, some people [e.g., Collins 1912] did, however, think that domesticated corn had to have had a more complicated history. Mangelsdorf studied the three plants that are relevant to the history of domesticated corn (*Zea mays*), namely, *Tripsacum*, teosinte, and wild corn, and came to think that teosinte could very well be the *result* of corn domestication rather than the cause of it. He felt sure that somewhere in the ancestry of wild corn was most likely *Tripsacum* and that this involvement happened long before humans entered into the process. The debate then raged for a long time over whether there was such a thing as wild corn

and whether teosinte was the ancestor of domesticated corn. But then perennial wild teosinte was found [Iltis et al. 1979], and it looked like a perfect ancestor for annual teosinte and subsequently for corn. Shortly before his death, however, Mangelsdorf did something interesting: he took perennial teosinte and crossed it with primitive corn, and what did he get? Perennial corn. That indicated that one was not the ancestor of the other. Subsequently, he got annual teosinte out of a cross between corn and wild teosinte. So, annual teosinte came *after* corn and was the *result* of hybridization of corn and perennial teosinte.

HF: Does this mean that we haven't yet found the ancestor of corn?

RSM: That's right. It hasn't been dug up yet. What Mangelsdorf said, in fact, was that once people began to grow corn in the Tehuacán Valley, the pollen of that early domesticated corn was all over the valley and it systematically hybridized the wild corn out of existence. Mangelsdorf thought that the original corn must have grown only in small ecological niches where all the other grasses weren't there to outcompete it. Now, most recently, Mary Eubanks [1997], a botanist at Duke University, wondered what would happen if she crossed this perennial teosinte with various kinds of *Tripsacum*, because both have some of the characteristics of domesticated corn but neither has all the characteristics of ancestral corn. So she tried it in the greenhouse, and what came out was *Zea indiana*. She has now explained how *wild* corn came into being, a process that could have happened without humans' being involved in any way. From it, then, domesticated corn could later have evolved through human selection. Eubanks's experiment is crucial because she has shown that you cannot get corn out of teosinte alone no matter what you do, and so prehistoric people couldn't have done it either. There are two dominant genes in teosinte that would have to become recessive in domesticated corn. Her experiments show that for this to happen you need something else first. Her approach takes the real complexity of the process into account.

HF: So what justifies the Establishment approach?

RSM: Doebley [1990; Doebley et al. 1997] did some genetic work on some of the perennial teosinte and thought that this kind of perennial would give rise to annual corn, and since perennials come historically before annuals this conclusion seemed justified. Eubanks takes a different tack—she takes two perennials [teosinte and *Tripsacum*] and gets an annual. So humans were confronted with a plant that had the potential for odd behaviour. It might be possible to do some experiments with pollen in order to take all of this research farther and show what plant those prehistoric people might have been confronted with. However, right now it is almost impossible to tell corn pollen from teosinte pollen—except for its size—because they are very close genetic cousins. Our present techniques are not sophisticated enough.

HF: Looking back on Tehuacán, how would you evaluate its importance to your work?

RSM: Tehuacán set up a problem and allowed us to develop a methodology to solve the problem and then solve it. Now that we have even more new data, we can play around with them and see how they match the greenhouse results. Tehuacán showed that the evolution of domesticated plants was complex before and after human involvement. The natural world is far more complex than we think, and no one path or linear influence will explain everything. Interestingly enough, when we look at the map of Mexico, we find that in ancient times the perennial teosinte was in the west. In the east there was perennial *Tripsacum*, and Tehuacán happens to be in the overlap region. That's where those two plants must have accidentally produced the ancestral wild corn. People entered into the picture later, and then yet another evolutionary path began for that plant.

HF: What does all this complexity say about human beings and their evolution?

RSM: If we use the metaphor of a ship, we find that one group of loose cannons bounces around on the deck of the ship, and they do this in a different way from another group of loose cannons on another ship. There is nothing random about any of this; there is a pattern to each of them.

HF: Like a play being enacted with the actors improvising as they go—

RSM: —and the stage itself is moving as well. This is true for prehistoric developments as well as for archaeologists. When we deal with the archaeological record, at least 80% of it has rotted away long ago. To reconstruct the whole story from what little we have means that many intepretations are possible. Just moving from, say, 2% of available data to 22% means that the whole ball game changes. DNA studies suddenly give us another 2%. It would be nice to get the archaeological evidence for corn domestication—for example, to find *Tripsacum*, teosinte, and wild corn in the same layer and then compare their DNA with what we already know. Then we'd have as close an evolutionary sequence as you could probably ever get. In some cave, somebody is going to dig up better samples than we now have of these plants, and when that happens it will probably be not too far from Tehuacán.

HF: How did you wade into the problem of the First Americans?

RSM: Sometimes I back into something, protesting as I go. Early in my career, back in the days of the Tamaulipas project, I came upon very early material, the Lerma and

the Diablo lithic complexes. Lerma goes back to about 10,000 and Diablo to about 20,000 years ago. Those dates still stand. This happened again in Tehuacán. In some of those layers in Coxcatlán Cave, zones 23/28, there were bones of extinct animals. In fact, there is a lot more very early material to be dug up. We got good cross-dates on those animal bones between Tehuacán and Kent's [Flannery] material in Oaxaca and with Cynthia Irwin-Williams's stuff in Valsequillo. We still need some direct dates on that bone material, though.

HF: How were your early Lerma and Diablo finds received?

RSM: Not very well. People like Alex Krieger and the Mexican archaeologists accepted my early stuff from Tamaulipas without difficulty, but Americans didn't read the Mexican literature and dismissed them as crazy anyway. I actually wrote a short history of the problem concerning the First Americans [MacNeish 1978], starting with Thomas Jefferson and moving up to last week.

HF: What side would Jefferson have been on?

RSM: Ours, absolutely. He is the father of American archaeology. He was the first to dig up an Indian mound and identify the shovel-shaped incisors of the skeletons in there, and through this fact and his excellent interpretation of the stratigraphy—long before its time in archaeology—he clinched the matter on who built those mounds. He also cut down a big tree that was on top of the mound, counted the rings, and showed that the mounds were at least 200 years old and therefore predated the arrival of the Europeans. What a great beginning for archaeology! This dig made it impossible to say, as his contemporaries had done, that the Indians were savages and could be killed off mercilessly because they had supposedly killed off the high civilization responsible for those mounds. However, my colleagues managed to ignore my First Americans material from Tamaulipas. In the early 1950s it was Alex Krieger who was already talking about pre-projectile-point and pre-Clovis horizons. Krieger was on the side of God, as far as I am concerned. He brought a geologist and a paleontologist to Tamaulipas to examine our material. They were loud and clear in their support of my interpretation. Helmut de Terra of the Viking Fund [later the Wenner-Gren Foundation] also came to see the dig.

HF: Tell me about Pendejo Cave—scandals, archaeology, and all.

RSM: Of all the early sites in the Americas, Pendejo Cave has the best and totally indisputable evidence for pre-Clovis people in the New World. (Monte Verde and Meadowcroft are close seconds.) I had dug in this area before, because I had been interested in how early agriculture came into the Southwest. I had found various open-air sites, but none of them had plant remains. Then I heard about a survey that had been done in the area and was told that possibly I might be able to identify some primitive corncobs. Fort Bliss was setting up a firing range on its manoeuvre area in New Mexico and had asked J. Betancourt [1977] to do an archaeological survey first. He had found a few caves and felt they were good, so we went out and had a look. One of them was Pendejo Cave, and there, lying on the surface, were little corncobs. So I said, "OK, this one is a good one to dig." We expected all sorts of early plant remains and basketry which would help me round out the picture of how agriculture came into the Southwest. We started with a little slit trench, and I put Geoffrey Cunnar in charge of digging it. By the second week we had very nice stratigraphy: plant remains up there and six or seven floors underneath. I assumed they were Archaic and didn't expect much in the way of plant remains and artifacts. But then, one afternoon while I was busy in a neighbouring cave, Geoff came bounding across from his dig and yelled, "Look what we just found in zone G!" He was holding up a big horse toe bone, and it was a fossilized toe bone. I said, "Damn it, here we are again, embroiled in early-man stuff." With this find we changed our archaeological techniques and started looking in earnest for evidence of early occupation. The layer with the toe bone turned out to be 25,000 years old. Eventually we had 22 beautifully stratified major layers. The toe bone was found in direct association with choppers and scrapers.

I assembled an interdisciplinary team par excellence to study the ecology and all the basic environmental aspects of this site, and I also invited all the detractors and naysayers and even gave them the chance to dig at Pendejo. Dina Dincauze dug a square and found material in zone G dated to about 25,000 years; she still argued, of course. She had to leave for a meeting just before she reached zones H and I, which is where the first human fingerprints were found on a piece of clay. Vance Haynes was there also and collected samples for dating. We had divided up the various samples as the dig progressed and sent them to various labs. We have 72 radiocarbon dates. The last 13 of these were done by archaeologists for the Army at Fort Bliss, and 11 of them backed up my sequence perfectly. For example, they got a date of 43,000 years on a layer of which I already had a date of 51,000—perfect.

HF: What about those fingerprints?

RSM: There were several. The layer where the first print was found is roughly 33,000 years old; another turned up below that at 36,000 years. Eventually, we found 16 fingerprints, 8 of which are pre-Clovis [Chrisman et al. 1996]. One of them was found on a little bird effigy made of clay into which somebody had pushed a stylus to indicate eyes and made scratches for feet. The hardest part was getting our finds published in *American Antiquity*. In fact, the editor sat on it for four years. I found this amazing, especially since I have been a member of this journal's society since it was formed. He sent the article back twice, and we kept answering his criticisms, but in the end he waited another 16 months and finally,

when he did publish it, refused to include the photo of the effigy.

HF: Does Pendejo Cave meet the criteria for breaking the Clovis Barrier [Griffin 1965]?

RSM: Pendejo Cave meets all of them perfectly. These criteria were good stratigraphy, good dates, evidence of extinct fauna, good faunal material to facilitate environmental reconstruction, human remains of some kind associated with extinct animals, and clearly identifiable cultural material. The Pendejo Cave report, written by the top people in each category, is now ready in manuscript form. The first part deals with environmental issues. The geology, for example, is done by the grand old man of Pleistocene geology, John W. Hawley. He shows that Pendejo Cave is above a glacial lake. The general background on lake sequences can also be associated with Tehuacán. The geological setting for Pendejo and the formation of that cliff are described by an excellent mineralogist, the late Russell Clemons, and an outstanding young geologist, H. Curtin Monger. Clemons also identified the rocks from which the lithic tools were made, showing that much of the lithic material was foreign to the cave. Arthur Harris has written a classic chapter in which he identifies 41,000 fossil animal bones. They come from 22 layers starting at about 60,000 years ago. He has identified more than 100 species, including 60 now-extinct ones. We also have hair from extinct sloth and bear. On the botanical side, 50,000 plant remains and abundant fragments of wood were recovered, and Harold Hiles has identified some 27 species that go back 30,000 years ago, indicating climate change. An excellent study of packrat middens is informative on climate change. This method was originally pioneered by Paul Martin. Packrat middens are like little outhouse libraries which can be dated. Janet MacVickers took all of these and the data from other packrat middens and pulled it all together. The soil studies, done by Mike McFaul, tell us that there were five cold, wet periods, which represent glacial times, and four dry, warm periods, which are the interstadials. We have dates on these, of course. Bruno Marino, who was working at Harvard in climate studies, did isotopic studies on some of these materials, specifically on their hydrogen atoms. One type of hydrogen is found in fresh water, the other in sea water, and the fluctuations between the two tell us about changes in rainfall. He also did carbon and oxygen studies indicating how much sunlight and cloud cover existed at different times. His chapter is one of the best paleoclimatic studies in the United States to date. All of this means that we know what plants were around, what the climate was like, what people were coming into that cave and when, the seasons involved, and what animals they hunted in the canyon below. For example, they hunted giant rabbits the size of modern coyotes. Gigantism was all around then. These giant rabbits died out around 30,000 years ago. Our information from Pendejo Cave, by the way, does not confirm Paul Martin's [1973] theory of extinction caused by human overkill. We had invited him, too, of course. He remarked that this was the best stratigraphy he had ever seen, but when he saw that there were artifacts in the early layers he was sure that humans were causing even those much earlier extinctions. Actually, the extinction rates we did find do not back up his theory.

HF: How did the doubters of a pre-Clovis occupation of the New World deal with this material?

RSM: Vance Haynes wrote us a letter giving us all his objections, such as that the rocks used for tools must actually have fallen off the cave roof. The mineralogist's analysis showed, however, that 55% of the artifact material came from outside the cave. That picture above my desk depicts a short-faced bear knapping pebbles; it was drawn for me in response to the suggestion that animals carried rocks into the cave. Then Haynes alleged that the charcoal fires must have been caused by lightning hitting the cave. So we did a thermoluminescence analysis and found that the fires had in fact started inside the cave and then burned towards the outside. Then he objected that the fireplaces were really rodent burrows that had been burnt by lightning. So we photographed them very carefully to show how some of them were surrounded in neat circles by pebbles brought up from the river 300 ft. below; others are also clay-lined and have human fingerprints in them from patting the clay into place. The fire had turned the clay into brick.

The second part of the report contains the specifics of the stratigraphy and the 75 carbon dates by R. E. Taylor, who originally was against the notion of a pre-Clovis occupation but became convinced otherwise by the evidence. In my opinion, he runs the best radiocarbon lab in the world. Chapter 1 describes all sorts of interesting human-made features, among them grass bedding and a pebble-lined hearth dated to 55,000 years ago with some 50 stone artifacts in it. We also have the analysis of all the stone and bone tools. These reveal that there is a progression of the lithic complexes through time: the earliest complex is composed mainly of pebble choppers, the second has unifacial material, the third has blades and burins, and the last has Clovis material.

HF: No breaks?

RSM: No breaks. The frosting on our cake is the chapter by Rob Bonnichsen. In zone M, dated to 51,000 years ago, he found a bison humerus broken in half by two percussion blows with a sharp instrument and then scraped in the interior. The edge of that bone was used as a scraper. Vance Haynes suggested that these bones had been broken by other means, so we filmed a replication experiment. Bruno Marino brought me six freshly killed cow humeri, and my son picked up a boulder weighing about 49 lb. and dropped it from a height of about 10 ft. onto those freshly killed bones. The resulting shatter was totally unlike the ancient humerus Bonnichsen analyzed. We were, of course, copying William Irving [Irving, Jopling, and Kritsch-Armstrong 1989], who

had done similar demonstrations some years back at Old Crow Flat in Alaska. To further satisfy the detractors, some of whom had suggested that possibly wild horses had stomped on the ancient humeri, my 250-lb. son put on his hobnailed boots and stomped on the fresh cow bones. Again, the results were not even comparable. However, when we took a ball-peen hammer and used the technique which the people of 51,000 years ago must have used, we did get identical results.

HF: Did your critics concede after these experiments had been done?

RSM: Recently quite a few people have changed their minds about the Clovis-only doctrine. But I must tell you about yet another very interesting bone from Pendejo Cave. We found the heel bone of an extinct horse and sent it to be examined by police experts, who X-rayed it. Inside they found a projectile point made of a bird bone which had originally been hardened by burning. That horse bone is about 35,000 years old. They also reconstructed that ancient projectile point in just the way they do modern bullets in a murder case. Another extinct horse bone was found with a flint chip stuck in it.

HF: And what about the human evidence?

RSM: Part of our analysis of this has, of course, been published in *American Antiquity* [Chrisman et al. 1996]. For example, we have human hair from Pendejo, and its DNA confirmed that it is human and that, interestingly, it seems to match Central Siberian DNA. It does not match the four Native American groups.

HF: This could become a political hot potato.

RSM: Indeed. Add this to the ongoing controversy about Kennewick Man, a skeleton found in Kennewick, Washington, in 1996. That skeleton's DNA is almost identical to the DNA found in the human hair at Pendejo Cave, though our hair sample is about 10,000 years older. This suggests that the early Americans were coming out of central Siberia. That is why I have stopped talking and writing about the Paleo-Indian period and instead write about the Paleo-American period. *The New Yorker* article [Preston 1996] correctly pointed out that the date of the Kennewick Man skeleton is in conflict with the prevailing views held by Native Americans. Perhaps all of this will bring about a rebirth of American archaeology. The cherished myths of both the archaeologists and the Native Americans are being destroyed.

HF: Did you find any other pre-Clovis cultural material at Pendejo Cave?

RSM: Yes, in fact we found that pre-Clovis people were weaving textiles. The textiles known so far mostly came from the Archaic or the more recent Ceramic periods, but at Pendejo we found part of a net, some knots, and some string from the Clovis level, as well as seven pieces which included what appears to be twine basket from a pre-Clovis level dated to 14,000 years ago. Gary Jessup plotted activity areas and found that there was nothing random about these materials. Mama was working skin here, Papa was flint-knapping over there. Such information allows us to reconstruct their way of life and to compare Pendejo with many other early sites from Tierra del Fuego all the way up to the Bering Strait. We have about 2,000 pre-Clovis artifacts in South America, of which 7 come from stratified sites, and there are more than 200 carbon-14 dates from the time prior to 11,500 years ago. The South American evidence of pre-Clovis humans in the New World is in my view indisputable. In fact, as you know, Dillehay [1997] finally took all those doubters down to Monte Verde, and they were forced to agree with him. In South America we have the El Bosque mammoth-kill site, and Bill Irving, who studied this site, was convinced that the date of 32,000 years ago was right. The Valley of Mexico has several good pre-Clovis sites, including Tlapacoya, dug by Lorenzo [Mirambell 1973]. Moving north, we have Pendejo Cave and Meadowcroft, in Pennsylvania, but equally important are some clay-deposit sites near Lake Calgary, dated to about 25,000 years ago, and then there are the bones from Old Crow Flats in Alaska. I don't see how there can be any basis for the Clovis-only doctrine. The evidence to the contrary is absolutely overwhelming, and every year the evidence increases.

HF: This mental block does not exist in Europe, where pre-Clovis sites are taught in the universities and included in textbooks.

RSM: It is certainly an American phenomenon. Part of this resistance to a much longer prehistory in the New World has to do with deeply rooted prejudice against accepting the fact that the Native Americans also had a long and honourable past, as did the Europeans. This prejudice goes back all the way to finding excuses for killing off the "savages." I suspect, from my reading of history, that the U.S. army was far more savage than the native peoples ever were. Many American archaeologists seem to share those traditional prejudices or at least have not attempted to examine them consciously.

HF: Would it be true to say that Pendejo Cave, Meadowcroft, and Monte Verde are causing a revolution in New World archaeology?

RSM: Yes, what we have here is indeed another revolution in American archaeology such as we had when Clovis was discovered and Folsom ceased to be the earliest known culture. There is no doubt that humans were here at least 70,000 years ago. Instead of arguing about this indisputable fact, we should be worrying about making sense out of the existing material and realizing that nice simple answers are simply an illusion. There is nothing simple about the migration routes of early humans. At best we have some guesses about that process.

The Clovis-only people are kidding themselves if they think they have all the answers. For example, the idea American archaeologists have about the Mackenzie Corridor is simply wishful thinking. Nat Rutter from the Geological Survey of Canada [now at the University of Alberta, Edmonton] showed that this corridor was never closed by an ice sheet. We should also be asking why people ever left the rich northern hunting grounds at all. Moving south through the Americas, they had to continually adapt their tool kit as they came into all those diverse environments and met all those different kinds of plants and animals. Another nice archaeological myth is the migration-overkill theory [Martin 1973]. The process of adapting technologically to new environments, when moving with children, women, and old people, is extremely slow, and nothing radical happens very fast. Pendejo Cave suggests that the peopling of the New World must have been a very, very complex matter and a very slow process with lots of individual variation in tool kits and behaviour. The archaeology of North American prehistory is in its beginnings, and I would predict that the best is yet to come. Meanwhile, I am washing my hands of the dirty business of the search for the First Americans and am concentrating on China, where things are so much more peaceful.

HF: What a huge jump around the world!

RSM: But here we have an interesting connection: the first prominent archaeologist to provide some really good evidence for pre-Clovis occupation in the New World was Frank Hibbin [1941]. The resistance was fierce, and everything was done to ride Frank out of the profession. Half a century later, just recently, he offered me some money from the Hibbin Foundation for my work in China. He is still active in archaeology and in the past year has been visiting sites in Africa and Alaska.

HF: Living well is the best revenge.

RSM: No doubt about that. By the way, our Pendejo Cave publications includes a reanalysis of Hibben's material from Sandia Cave. I was told by Linda Cordell that the original bags from Sandia Cave were still in the basement of the Maxwell Museum in Albuquerque, New Mexico, and that those bags had not been opened since 1943. I was permitted to open those bags, and it turned out that the lithic technology is radically different from both Folsom and Clovis. I offered to pay for a formal lithic analysis of this material through the Andover Foundation. There is no evidence at all—as had been claimed by Hibben's opponents—that there was mixing of materials from the top of Sandia Cave. Only six pieces have a vague resemblance to the late, upper-level materials. The lower-level material from Sandia Cave is at least 20,000 years old. We included this comparison in our report.

HF: How did China become the focus of your research?

RSM: It grew out of my interest in the development of agriculture. There are different routes to that development, but these different pathways also stimulate each other. There is an interpenetration between the highland and lowland cultures as well. I began to worry about whether I had missed the boat on how it happened in Mexico. In the 1980s I checked my hypothesis on the lowlands in Belize. By this time I was familiar with Kent Flannery, Phil Smith, and Cuyler Young and knew the work of all three in the hilly flanks of the Near East since they were students of Robert Braidwood [Young, Smith, and Mortensen 1982]. I came to realize that the hilly flanks followed one path to agriculture and the Levant another way—a situation very similar to that in Peru and Mexico. And then I asked myself, what about China?

HF: At that time it must have been difficult to get into China. How did you do it?

RSM: I had known K. C. Chang since his days as a graduate student at Harvard. When he went to Yale to teach we met again. His son played with my son, and we were very friendly. During that time [former U.S. President] George Bush was the U.S. ambassador to the United Nations; he had also been on the board of trustees of the Peabody Museum. His executive secretary called me up one day and said, "The Chinese are trying to get into the UN, and they are therefore offering to have some cultural exchange. They have given us a list of what kinds of foreign research they will permit in China. On the top of the list is agricultural archaeology. You are the only agricultural archaeologist George [Bush] has ever heard of, so would you like to go to China?" That was in 1974. I said, "Sure, but I'm not the guy for that job. K. C. Chang is the guy you want." So Bush's secretary said, "OK. Call up Professor Chang, and we'll send you both." I called up K. C., and he said, "I'm from Taiwan. They'll never let me back into China. But if you want to go, I'll tell you what books and papers to read." He handed me his own book first, that classic *The Archaeology of Ancient China* [Chang 1986(1963)]. I researched everything available under K. C.'s guidance and left for China, but I had a heart attack in Seattle in 1975, so I never made it then. But K. C. did. In fact, it was Marie Wormington who went in my place with his delegation [Howells and Jones-Tsuchitani 1977]. It turned out to be just as well, because the plan to which the Chinese had agreed when we planned it with them at the National Science Foundation was scuttled once this American delegation actually reached China. The Chinese wound up showing them happy little weavers one day and happy factory workers the next, and they saw nothing much at all of archaeology.

HF: When did you eventually get to China?

RSM: In the mid-1980s K. C. was approached by Enzheme Toang, then head of anthropology and the museum in Chengdu, Sichuan. He told K. C. that he wanted to have a team of American advisers for the museum

there. (Incidentally, Enzheme fled just ahead of the secret police at the time of Tienanmen Square in 1989 and escaped to the United States, where he taught in Connecticut. Tragically, he died [in 1997] just when things were going well for him in his new teaching post.) K. C. made up a team which included me, and the National Science Foundation gave him the money, and again, at the last minute, the Chinese government canceled the trip for political reasons. I finally made it in 1991, when the International Conference on Agricultural Archaeology took place in Nangchang, Jiangxi Province.

HF: And how do the Chinese regard you now?

RSM: I have become a sort of patron saint.

HF: Maybe you have to be the right age before the Chinese even talk to you?

RSM: That's exactly right. I grew this Confucian beard for just that reason. I am a venerable scholar, and they have great respect for those. I even have my very own god [a small carved statue]. Here he is, Shou-lao, one of the Eight Immortals, carrying the peach of immortality in one hand and the lotus of longevity in the other. The crane beside him has to do with happiness. The bulge on his forehead supposedly connotes wisdom. When my students in China gave me this little statue they said, "Here, this is you." They presented me with it at a banquet celebrating the end of a digging season, saying they wanted me to come back.

HF: So communism hasn't been able to kill the old ideas?

RSM: The ancient religions and cults of China are very much alive in the villages and the country. The peasants have been dealing with warlords and bureaucrats for millennia, and they bow down before them but continue doing what they always did. For example, in Li Ping, where we found a wonderful, undisturbed cave, there is a Protestant church led by a Chinese Presbyterian. On top of the cave is a Taoist monastery whose monks worship some local spirit gods, and everybody respects them, even the secret police.

HF: Were you able to test your hypothesis on the multilinear development of agriculture?

RSM: Yes. My hypothesis was that the domestication of millet in the north of China was like the situation in the South American highlands and that the south of China provided the parallel for the lush South American lowlands. K. C. Chang was sure of the developmental evidence for the millet picture, but the rice picture was still up for grabs. Theories about the beginnings of rice agriculture were based on places like Spirit Cave [in Thailand; see Gorman 1970], but nothing much was known about the role China might have played. I had decided that this conference would give me the excuse to take a tour through China. The other American scholar with me was Stan Olsen, a zoologist and the father of John Olsen [of Tucson, Arizona], another old China hand. We were introduced to many Chinese archaeologists, and, frankly speaking, I was very unimpressed with what I saw. They took us on a three-day tour of Jiangxi Province, and I saw caves, lots of caves, and more caves right in the area where the conference was being held, in Pengtoushan and Hemedu. There was one cave, Xian Ren Dong, which had been dug in the 1960s and had yielded material dating back 10,000 years. But the Chinese and even K. C. had totally ignored it.

HF: Why?

RSM: The prevailing theory was that rice cultivation came from Thailand and could not have been an indigenous development, so evidence to the contrary was simply ignored. Southern China had the caves and dates from good materials, and then I found out that they also have wild rice in that area even today. I spent most of 1992 writing letters. I also found Zhijun Zhao, who is the most important person in this research project because he studies phytoliths and is fluent in English and Chinese. He studied with Deborah Pearsall at the University of Missouri at Columbia. The significance of the cave material in Jiangxi Province became clear to me when I visited Xian, where the Neolithic village of P'an Po (about 6,000 B.P.) has been reconstructed as an exhibit. It had yielded the earliest painted pottery. When I looked at that, I said, "Gosh! We have material in Jiangxi Province which is much older than this village. That stuff is almost 11,000 years old." Back in the United States Zhijun Zhao wrote to the Chinese and handled the intricacies of their hierarchical structure until they finally suggested that we go to China and visit for a few weeks and work out the details for a permit in person there. Jimmy (as I called him) and I went to Beijing, where we were, to our surprise, met with open arms. We arrived Sunday night, and on Monday night they gave a banquet in our honour. What I didn't know was that my theory of indigenous origins of rice cultivation happened to agree with that of their top man, Yan Wenming, who was head of the department of archaeology at Beijing University and believed that rice was first cultivated south of the Yangtze River. We drank a few toasts together and Jimmy prophesied that we would be great partners. Sure enough, two days later we went to Yan Wenming's office and met a high-school buddy of Jimmy's, Yang Lin, who had studied in Texas and spoke fluent English. And so we worked out our joint research effort. The terms were clear and simple: Chinese do fieldwork, Americans do fancy analysis and bring the money!

HF: And so you have been stuck with raising the money ever since?

RSM: Anybody working in China is stuck with that—that's China. In return for this, we got to fill in the blanks on the archaeological permit as we chose, and a car was waiting for us at the airport to take us on a

tour of the caves around Nanchang in Jiangxi Province. If that plane had gone down, Chinese archaeology would have been set back by a hundred years—everybody was on it! My team consisted of myself and Jimmy and a young pollen expert, Luo Erhu, who had been a student of Enzheme Toang. In 21 days we found 28 caves. Some had been looted for "dragon bones" [fossils, traditionally used for medicinal purposes; see von Koenigswald 1981], and one had been used as an ammunition factory and for storage of secret documents. The department of defense had once needed a bomb shelter, and this cave had been considered perfect for that. We found 8 meters of refuse in it. But we also found six absolutely fabulous and apparently undisturbed caves. We have so far dug four of them.

HF: How do you know when a cave is "fabulous"?

RSM: The first clue is the kind of surface you find on the talus slope at the mouth of the cave. If you find some pottery, some microblades, some Palaeolithic material, you can be pretty sure that they came out of the strata inside. Then, as you investigate the walls of the cave you can roughly estimate how deep the deposit might be. The roof of the cave may have evidence of smoke. It's also important to find out if water ever ran through it and disturbed everything.

HF: How did the authorities like your plan to dig six caves?

RSM: They said, "That's too many caves for 14 weeks." We bargained for three and permission to test a fourth, and they agreed to that.

HF: I got the impression in talking to K. C. Chang [Ferrie 1995] that archaeology has always been an integral part of Chinese culture and continues to be so today. Did you get the same impression?

RSM: Absolutely. I also found that we very much enhanced the local clan pride because this archaeological backwater was now being made famous in the whole country through foreign archaeologists. But, of course, we had a lot of conflicts to work out. The Chinese operate on closer to a five-hour working day. They were impatient with us because they thought we were too exact and took too many notes. Besides that, we had tourists breathing down our necks all the time. Geoff Cunnar, my supervisor, was trained to use a trowel, as we all do, because we want to see exactly where that person had dropped an item thousands of years ago, and we want to understand the relationship between a specific spear point and everything else on that floor. We see artifacts in their relationship to each other as an information pool. We had difficulty convincing the Chinese to use only trowels; they were used to digging through the hard-packed soil with a tool like a large garden weeder. After one clash over tools, we had a big truth-speaking session. We told them we needed to work longer hours if we wanted to meet our objectives. They insisted they needed a rest period after lunch. We compromised accordingly. And so we dug Xian Ren Dong [Benevolent Spirit Cave] and Diaotonghuan [Bucket Handle Cave], and everybody felt part of the whole thing. The Chinese began to understand the enormous importance of every detail—that one can't just throw away all the little bones and only keep the big ones. I explained to them that I wanted to know who occupied this cave in each season and only the bones and plant remains would tell me this. They were interested in objects; I was interested in *relationships* between objects. As we proceeded with the dig, floor plans began to emerge, and the peasant workmen could now see these relationships themselves and became experts. Like workmen all over the world throughout the history of archaeology, as they began to understand, they and everybody else were having fun. Then visitors came—every politician and bigwig turned up. The students, who had now been taught what I called the "American La Perra Method" (after La Perra Cave in Tamaulipas), became very proud of themselves. Not only were they certified by Beijing but they had also mastered this fancy American method, and they started to teach others. These guys were a new elite.

HF: How did your patron, Yan Wenming, take to all these developments?

RSM: The first season all went well, and the second season we did mostly interdisciplinary work. By the third season we ran into problems because our rice-bearing levels were too early even for Yan Wenming. He had published papers in which he had given estimates which were now contradicted by this much earlier material. That created tension. Meanwhile, something of a palace revolution had occurred at the university, and the hierarchies had all changed. So in the 1997 season we had to renegotiate a lot of things.

HF: What are your preliminary conclusions about the origins of rice cultivation?

RSM: To begin with, the whole pottery picture has been revolutionized in the past three years. For example, our pottery at Xian Ren Dong is about 13,000 years old and is similar to the pottery recently discovered by a Japanese-Russian expedition in the Amur River area, which is *made* in the same way. Pamela Vandiver of the Smithsonian Institution has examined this pottery for the manufacturing techniques involved. This unique manufacturing process establishes a connection between the Amur River area and South China. A third group of pottery, slab-built, is made with fibre temper rather than grit temper and is the earliest in Japan. The next stage in pottery development is just as interesting. The pottery we find is now paddled. We also found net-impressed material in our Chinese caves. All this pottery is fired in a simple bonfire, not in an artificial kiln. By about 8,000 B.P. we have painted pottery and sophisticated ves-

sel forms fired in kilns. This is more than messing around with clay.

HF: Now we are in the time of P'an Po village?

RSM: Exactly, and K. C. Chang was mighty pleased with all of this. The Chinese will probably argue for the next 100 years about the names we've been making up for these pre-P'an-Po layers. Together with this sophisticated pottery we now have rice. P'an Po has millet, and here in the South we have rice. Later, P'an Po level 2 also has rice. We have not yet dug big village sites in open environments; that's the next project. We now have the outlines of the chronology, we have good stratigraphy, and we know quite a bit about the subsistence system but not yet very much about the culture. We have chipped adzes, some digging sticks and weights. Bone tools are gone now, and people might be getting ready for bronze tools. We also have Kent Flannery's student Richard Redding doing the bone identification for us. Most important, we have been able to establish that South China is indeed part of the Asian Palaeolithic, because we found microblades at the lowest levels here. This is the first solid evidence for microblades in South China.

HF: So much for the infamous "hiatus" in Chinese archaeology!

RSM: Right. But then I am the old kind of archaeologist who gets a chronology first before shooting off his mouth about cultural developments. I have solid data and layers with clear information. That "hiatus" is out the window now. I must add that with those microblades we found some very nice bone tools which establish the transition from the earlier Palaeolithic very nicely. Of course, the Chinese found these before, but they never saved them until now. The preliminary reports [the Sino-American Jiangxi Origin-of-Rice Reports] anybody can obtain from the Andover Foundation.

The only thing I am not happy with yet is the dates. Of the 34 carbon-14 dates, 7 are acceptable. However, we have well-dated correlations with places from Siberia which help us out for the time being. The earliest microlithic material in Asia goes back to that period, but in our dig the earliest goes back to about 20,000 years ago, and we still have six layers underneath that to explore. Our reports cover roughly a 35,000-year period. Jimmy's phytolith studies show us that around 20,000 years ago wild rice began to appear in South China and the people in our cave began to pick it, though, of course, they were still microblade-using hunters. Around 17,000 to 13,000 years ago the climate was too cold and dry to sustain rice very well, but by 13,000 years ago rice is found everywhere again, and there is a hint that the proportions between wild plants and rice were changing in the caves. The rice finds now include mutation freaks, and people are using rice a lot more. The shift to domesticates shows up around 11,000 to 9,000 years ago, and between 9,000 and 7,000 years ago paddy fields appear.

HF: Any hints about the people themselves?

RSM: Indeed! We have some human bone from these later layers, and we have analyzed some already for carbon-12, carbon-13, and nitrogen-15 isotopes, which tell us what these guys ate. These are just stray bones kicking around in some of those layers. The proper burials we found in the later periods; the Chinese do not allow any human skeletal material to be taken out of the country, fearing another disaster such as the one which caused all of the Zhoukoudian *Homo erectus* material to be lost during World War II, so all we have is casts. Today the Chinese authorities will only let us have stray bones. Rice happens to leave a neat signature in the bones of people who eat it. The proportions of carbon-12 and carbon-13 are diagnostic; carbon-12 is stable and carbon-13 is unstable. The ratio between the two gives us the information we want: −12 indicates corn, −22 rice consumption. If you get a +5 in nitrogen isotopes and a −20 in carbon, you are almost assuredly dealing with rice consumption. We have one near-complete skeleton at roughly 16,000 years ago, and it shows a nice −20 carbon value, and the nitrogen value tells us that the rice was a semiaquatic plant already then. At 12,000 years ago the carbon is still the same, but the nitrogen has shifted and rice is no longer growing in its wild habitat—we are getting hybrids—and this continues into 11,000 years ago. We did find two most interesting skeletons. One is 8,000 years old, and the other one died just a few years ago and was buried on the hill below our cave.

HF: You were messing with ancestral bones?

RSM: There are tombs all over the sides of these hills, and this was just luck. These happened to be the bones of a man who had committed suicide because of an unhappy love affair. In the Confucian tradition, suicide disgraces the family, so we were allowed to have his remains. We know his name, but, more important, we know exactly what he ate. He spent his whole life working in rice paddy fields. For our purposes the important fact is that the analysis of the two skeletons showed that they carry the identical signature. Both were dependent on rice paddy agriculture. All of this information also agrees with the tools we found, which are rice planting and harvesting tools.

HF: Do you have any information on the nature of the rice plants themselves?

RSM: Yes, here is the report by a most interesting Japanese scholar, Y.-I. Sato [Sato 1996], whom I met at a conference in Japan. In his greenhouse at the Rice Institute in Shizuokar he has figured out the DNA sequence of rice. There are 19 kinds of wild rice, and 4 of these grow in our Chinese province. Two of these are related to domesticated types. Some are annual, some

are perennial. Sato played around with the annual variety, *Oryza sativa nivara*, and found that it was genetically very unstable. If you changed the soil it grew in or the available water it would begin to throw off mutants. Its big root allows it to thrive wild without human help. In a wild form it has a very narrow leaf and a few grains; we know that it was eaten then, and Sato has identified its DNA. We think that the next stage was a mutant, still with a big root and a narrow leaf but now occasionally beginning to have more than one stalk with seeds that were bigger and round. Sato thinks these were selected by people as desirable and in the process the root changed to a smaller one that requires human intervention because now it can no longer grow wild. In time the rice plant developed five stalks and very big seeds, producing 25 times as much food as before.

HF: Is this archaeologically visible?

RSM: Oh, yes, but it gets even better. Sato took domesticated *Oryza sativa nivara* and hybridized it with the wild progenitors *Oryza sativa japonica* and *Oryza rufipogon* and wound up getting *Oryza sativa*. The archaeological evidence we have right there in our dig shows that those people double-cropped the two types in the same field. One variety requires a growing season of six months, and other of nine months. They still grow the *indica* and *japonica* varieties today. That young man who had recently committed suicide—whose bones we found below our cave—had eaten both types, just as his ancestors had 4,000 years before him, namely, *Oryza indica* and *Oryza japonica*. This is our hypothetical model. We hope Sato can do further research on samples of the various wild types.

HF: Do Sato's experiments inform us about what must have happened in rice domestication worldwide?

RSM: Most definitely. In the sites in Thailand you never get the *japonica* variety, only *indica*, and the later dates for Thailand indicate that the people of Spirit Cave got rice from the Chinese. It might have been an independent development there also. The question now is, what kind of life did people lead once they came out of the caves?

HF: This reminds me of the Near Eastern development, where people came out of Shanidar Cave and built Zawi Chemi Shanidar village.

RSM: Exactly the same. That was also an indigenous development. An exactly parallel development might be confirmed once we get the chance to dig this tell we found only 80 miles away from our caves. In fact, samples of pottery from this tell are identical to the pottery we found in the caves earlier. A road runs right through the middle of the tell, so we just pulled out a few pottery pieces without trouble. We suspect that the top of the tell is about 5,000 years old and the gutter level just below the road is about 9,000 years old. All of this is pre-P'an Po, and we expect to find posthole houses which we can eventually reconstruct. We have some interesting leads with caves in the next province in Hunan, and then—

HF: I have great faith in the powers of the Taoist Immortal Shou-lao that your Chinese students gave you, but how long is all this going to take?

RSM: Beware—I will still be digging at 103!

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The Initial Upper Paleolithic in Northeast Asia[1]

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The period between roughly 45,000 and 30,000 years ago witnessed several critical events in human evolutionary history, among them the appearance and elaboration of Upper Paleolithic technologies, the disappearance of archaic hominid species, and the apparent ascendance of anatomically modern humans. Among the many novel features of the Upper Paleolithic, it is the sudden ubiquity of blade technologies beginning approximately 45,000 years ago that appears to signal significant behavioral change (Bar-Yosef and Kuhn 1999:333). This increasing reliance on blade technologies is now commonly referred to as the Initial Upper Paleolithic (Bar-Yosef and Kuhn 1999, Kuhn, Stiner, and Güleç 1999). Throughout western Eurasia there appear to be common technological trends defining this phase, including (1) blade production from cores combining elements of both Middle and Upper Paleolithic technologies, (2) high frequencies of retouched blade tools, (3) blade blanks with faceted platforms, and (4) elongate Levallois points (Kuhn, Stiner, and Güleç 1999:506). Assemblages are dominated by tool forms traditionally considered characteristic of the Upper Paleolithic, namely, end scrapers, burins, and truncations. Other tool forms, including side scrapers, denticulates, and occasionally points, may also occur in high frequencies.

There is ample evidence to suggest that genuine Initial

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Upper Paleolithic variants are found in certain areas of Northeast Asia. Like its Western counterparts, the Northeast Asian Initial Upper Paleolithic is characterized by the elaboration of blade technologies showing both Middle and Upper Paleolithic characteristics. The best-known of the Northeast Asian Initial Upper Paleolithic sites, Kara Bom, in the Altai region of southern Siberia, has been dated as early as 43,000 years ago (Derevianko, Petrin, and Rybin 2000, Goebel, Derevianko, and Petrin 1993). This paper presents detailed comparisons between the Middle and Upper Paleolithic assemblages from Kara Bom and Late Pleistocene blade-based assemblages from the Mongolian Gobi and Northwest China in an attempt to outline the characteristics of the Initial Upper Paleolithic in Northeast Asia and assess its coherence as a technological phenomenon. In addition, we provide geochronological background for the primary sites discussed, much of which has not appeared in the English-language literature. The sample of sites examined here, though not geographically exhaustive, was analyzed in sufficient detail to allow rigorous quantitative comparisons between sites.

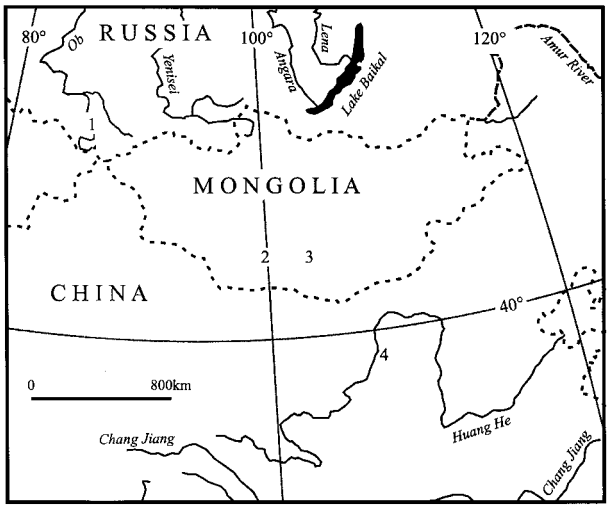

FIG. 1. *Northeast Asia, showing the location of the sites compared in this study.* 1, *Kara Bom;* 2, *Chikhen Agui;* 3, *Tsagaan Agui; and* 4, *Shuidonggou.*

SITES AND SAMPLES

Kara Bom is an open-air site in the Siberian Altai (fig. 1) (50°43′N, 85°42′E; 1,120 m above sea level). First excavated by Okladnikov, it consists of 11 lithological units divided into three main depositional phases (fig. 2) (Derevianko, Petrin, and Rybin 2000; Derevianko, Shimkin, and Powers 1998:103–4; Goebel, Derevianko, and Petrin 1993; Okladnikov 1983). At the base of the section, strata 11 and 10 are thought to correlate with the Zyr'ansk glaciation ($\delta^{18}O$ stage 4). Stratum 11 yielded a single ESR age of 72,200 years B.P. (calendric, uptake model unspecified). Strata 9–5 are correlated with the early part of the Karginsk interstadial ($\delta^{18}O$ stage 3). An ESR age of 62,200 years B.P. (calendric, uptake model unspecified) was obtained from stratum 9, while stratum 6 yielded AMS radiocarbon ages of 43,200 ± 1,500 B.P. and 43,300 ± 1,500 B.P. (Goebel, Derevianko, and Petrin 1993). The overlying units are correlated with the later part of the Karginsk interstadial and have produced ages of 34,180 ± 640 B.P. and 33,780 ± 510 B.P. (stratum 5b), 30,990 ± 460 B.P. (stratum 5a), and 38,080 ± 910 B.P. (stratum 4) (Goebel, Derevianko, and Petrin 1993:456). The Middle Paleolithic collections derive from strata 9–7 and thus have an expected age of approximately 62,200 years B.P. (calendric). The Upper Paleolithic collections derive from stratum 6 and have a corresponding radiocarbon age of 43,000 B.P. Additional Upper Paleolithic assemblages were excavated from strata 5–3 but are not discussed here. The analyses presented below are based on a study of the Kara Bom collections (n specimens = 2,085) undertaken in 1998.

The dominant stone raw material used at Kara Bom is a fine-grained gray-black chert found in abundance in the channel of the Altairy River, 1–2 km from the site. More than 98% of the combined Middle and Upper Paleolithic collections is based on this one raw-material type. Levallois-like flat-faced cores are the dominant core form represented in the Middle Paleolithic collections (n = 20) (fig. 3, table 1). These cores are typically plano-convex in lateral cross section, restricting reduction to a single "face" of the core (see Boëda 1995). The striking platforms are commonly faceted and approach right angles with the primary reduction face. Additional core forms make up only a small part of the assemblage. Not surprisingly, Levallois end products constitute more than 20% (n = 78) of all of the recovered blanks from the Middle Paleolithic. Characteristically, such blanks are flat in both lateral and longitudinal profile and have steep (> 70°), faceted striking platforms. Only generalized flakes (n = 220, 59.9%) surpass Levallois end products in relative frequency. Perhaps more surprising, given Goebel, Derevianko, and Petrin's (1993:452) conclusion that "true blade cores and their removals" were absent from the Middle Paleolithic assemblage, is that blade end products, including Levallois and subprismatic blades, pointed blades, crested blades, and bladelets, make up nearly 15% (n = 52) of the Middle Paleolithic collections. Levallois blades are flat in cross section and have length-width ratios not exceeding 4:1, faceted platforms, parallel or subparallel dorsal scars, and somewhat irregular edges. In contrast, subprismatic blades tend to have straight lateral edges, trapezoidal or triangular cross sections, and more acute striking platforms. The manufacture of these blade end products is consistent with the morphology and reduction trajectories of the recovered cores. Combination tools (n = 15), displaying various mixtures of notched, denticulated, and scraper elements, dominate the Middle Paleolithic tool assemblage (table 2).

Subprismatic blade cores are the most common prepared core form in the Upper Paleolithic collections (n

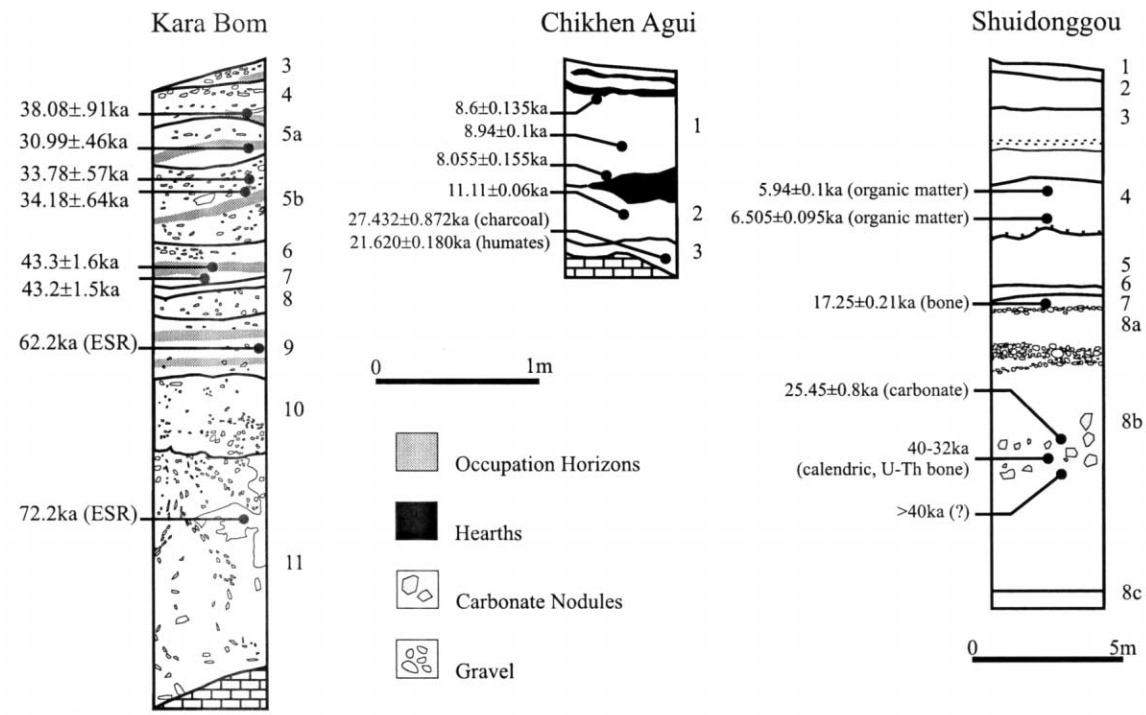

FIG. 2. *Cumulative stratigraphic profiles Kara Bom, Chikhen Agui, and Shuidonggou showing the positions of occupational horizons and hearths and principal radiometric dates.*

= 5), though cores with standard Levallois geometry occur in nearly equal frequencies (n = 4) (fig. 3, table 1). Subprismatic blade cores differ from Levallois-like flat-faced cores in extending reduction to as much as 200° of the core perimeter. Their striking platforms also tend to be more acute (< 70°), and platform faceting is less common. Blade end products are more than twice as common in the Upper Paleolithic horizons (42.4%, n = 252) as in the Middle Paleolithic. The majority of these are classified as subprismatic blades. Like their Levallois counterparts, subprismatic blades are very flat in longitudinal section and represent one end of a continuum of blade morphologies generated from both flat-faced and subprismatic cores. Flake-blades, which meet the metric definition of a blade but are unstandardized in one or more characteristics, represent the other end of this continuum. The small number of core tablets (n = 2) and the increased frequency of crested blades (n = 11) is consistent with a greater emphasis on blade technology in the Upper Paleolithic. Similarly, retouched tools on blades assume more importance (table 2).

Chikhen Agui (Ear Cave) is a small limestone rock shelter located in the central Gobi Desert of Mongolia (44°46′22.3″ N, 99°04′08.7″ E; 1,970 m above sea level) (fig. 1) (Derevianko et al. 2001*b*). Deposits reach a maximum thickness of about 75 cm, and the sequence is divided into three archaeological components (fig. 2). Strata 1 and 2 are exclusively microlithic and are not discussed here (Derevianko et al. 2000*b*). Stratum 3 contains a large blade industry resembling that from Kara Bom. A single AMS radiocarbon determination on hearth charcoal dates stratum 3 to 27,432 ± 872 B.P. (AA-26580), with the humate fraction dating to 21,620 ± 180 B.P. (AA-32207). A bone collagen date from an associated open-air component (locus 2) yielded an age of 30,550 ± 410 B.P. (AA-31870). The archaeological sample analyzed here derives from the 1996 excavations and consists of 167 specimens.

The raw-material environment at Chikhen Agui differs dramatically from that at Kara Bom. Approximately 94% of the assemblage is made of high-quality opaque cherts of several different types imported from at least 5 km away. Quartzite, one potential local material, makes up only 3.6% of the assemblage. The majority of the prepared cores from Chikhen Agui are small, Levallois-like bidirectional blade cores with opposed striking platforms (fig. 4, table 1). Two specimens are classified as Levallois flake or point cores on the basis of the character of the final removal before core discard. Generalized flakes (n = 41) form the single largest category of debitage at Chikhen Agui. However, all of the blade products combined (n = 42), including Levallois blades and bladelets, reach equal frequency. There is no evidence to suggest that blade and bladelet blanks were produced by different reduction strategies. They are morphologically similar in all respects, and the core

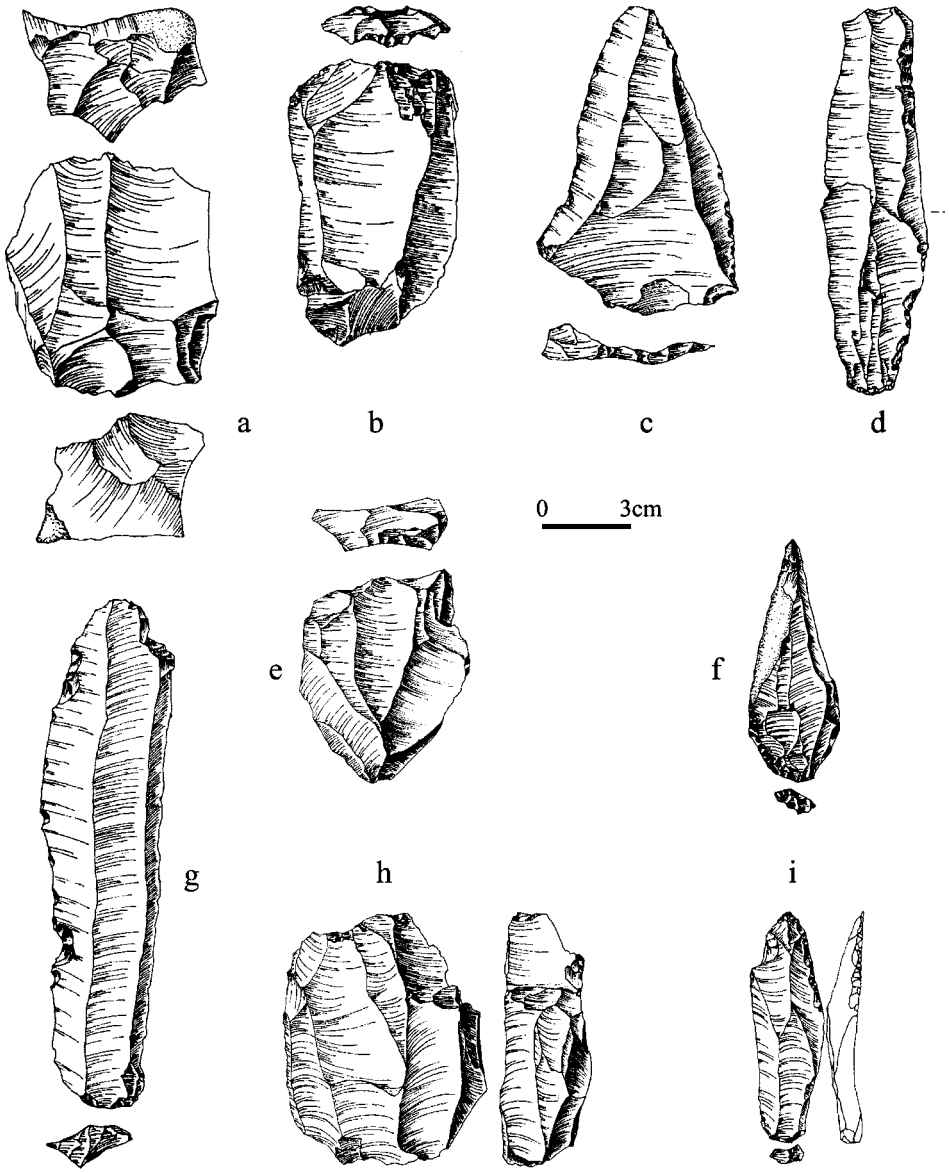

FIG. 3. *Cores, blanks, and tools from the Middle Paleolithic* (a–d) *and Initial Upper Paleolithic* (e–i) *levels at Kara Bom.* a, b, e, h, *flat-faced* (*"Levallois"*) *cores;* c, *Levallois-like point;* d, g, *Levallois blades;* f, i, *retouched pointed blades* (*redrawn after Derevianko, Shimkin, and Powers 1998*).

population is consistent metrically with the production of both blank types. A similar conclusion may also apply to the series of elements resembling Levantine Levallois points. Flat-faced blade cores display a tendency to evolve toward convergent reduction. Over its use-life, a flat-faced core may therefore generate products that are parallel, subparallel, and convergent in plan form, as well as metric blades and bladelets. The technical elements classified as "crested blades" are similar to classic Upper Paleolithic *lames à crêtes* (Inizan, Roche, and Tixier 1992). However, these preparations were apparently employed in shifting reduction from the primary face to the edge of the core in a manner somewhat consistent with *lames débordants*. Cores with lateral crests prepared late in the reduction sequence are common at other sites in Mongolia (Krivoshapkin 1998). Retouched tools constitute nearly 12% (n = 20) of the recovered artifacts from Chikhen Agui (table 2). No single tool form occurs with great frequency except for blades with one or two edges retouched.

Shuidonggou Locality 1 is located on the edge of the Ordos Desert in Ningxia Hui Autonomous Region,

TABLE 1
Raw Counts of Core and Debitage Types from Kara Bom, Chikhen Agui, and Shuidonggou

	Kara Bom Middle Paleolithic[a]	Kara Bom Upper Paleolithic[b]	Chikhen Agui	Shui-donggou
Tested pebble	3	2	1	7
Chopping tool	1	–	–	9
Chopper	–	–	–	10
Polyhedron	1	–	–	38
Discoid	–	–	–	11
Levallois flake core	19	2	1	5
Levallois point core	3	1	1	1
Levallois blade core	–	1	7	80
Subprismatic blade core	–	5	–	1
Pyramidal blade core	–	–	1	–
Change-of-orientation core	1	–	3	5
Pebble microblade core	–	–	1[c]	4[d]
Narrow-faced core	2	9	2	3
Broad-faced core	–	1	2	–
Other cores	–	–	2	2
Generalized flake	221	315	40	1,507
Levallois flake	32	5	–	11
Levallois point	22	8	4	15
Levallois blade	24	20	28	402
Subprismatic blade	14	156	–	–
Prismatic blade	–	–	–	7
Pointed blade	–	14	3	7
Bladelet	10	44	9	66
Pointed bladelet	2	7	1	–
Microblade	–	–	–	1
Core tab	–	2	1	7
Edge element	9	5	–	24
Other technical element	–	7	7	23
Bipolar flake	–	–	–	3
Kombewa	1	–	–	–
Crested blade	2	11	5	46
Flake blade	–	–	–	112
Total	367	615	119	2,407

[a]Mousterian horizons 1 and 2 combined.
[b]Upper Paleolithic levels 6 and 5 combined.
[c]Displaced from microlithic levels.
[d]Bipolar pebble cores.

China (38°17′55.0″ N, 106°30′6.2″ E; 1,220 m above sea level) (fig. 1). The site was excavated initially in 1923 by Emile Licent and Pierre Teilhard de Chardin and subsequently by Chinese teams in the early 1960s and again in 1980 (Boule et al. 1928, Jia, Gai, and Li 1964, Ningxia Museum 1987). Late Pleistocene sediments at Locality 1 occur within a fluvial cut-and-fill sequence (fig. 2). Stratum 4 is securely dated to the Holocene with radiocarbon assays on pond organic matter of 5,940 ± 100 and 6,505 ± 95 B.P. (Geng and Dan 1992:48; Ningxia Museum 1987). The underlying units have produced two finite radiocarbon dates of 17,250 ± 210 B.P. (bone collagen) and 25,450 ± 800 B.P. (pedogenic carbonate) from stratum 7 and 8b, respectively (CQRA 1987:37). Given the probable secondary context of the bone date from stratum 7, the depositional age of strata 7–5 is estimated to be less than 17,000 B.P. A third infinite radiocarbon date on unknown material underlying the archaeological horizons is difficult to evaluate (Geng and Dan 1992:49).

TABLE 2
Raw Counts of Retouched Tool Types from Kara Bom, Chikhen Agui, and Shuidonggou

	Kara Bom Middle Paleolithic[a]	Kara Bom Upper Paleolithic[b]	Chikhen Agui	Shui-donggou
Single side scraper	2	7	–	86
Double side scraper	3	2	2	22
Convergent scraper	1	–	–	16
Transverse scraper	–	2	–	28
Single end scraper	–	1	1	43
Double end scraper	–	–	–	1
End scraper on retouched blade	1	11	1	2
Fan-shaped end scraper	–	1	–	1
Circular scraper	–	–	–	5
Thumb-nail end scraper	–	–	1	2
Carinated end scraper	–	–	–	9
Nosed end scraper	1	2	–	1
Simple burin	–	1	2	5
Dihedral burin	–	–	–	2
Multiple burin	1	2	–	1
Borer	–	1	–	–
Backed knife	–	1	–	7
Backed fragment	–	1	–	–
Single notch	1	4	1	76
Multiple notches	–	3	–	18
Denticulate	3	8	–	18
Combination tool	15	29	1	59
Blade, one edge retouched	1	21	5	55
Blade, two edges retouched	2	16	3	17
Blade, retouched into point	–	1	–	3
Bladelet with abrupt retouch	–	–	1	–
Retouched flake	2	9	1	71
Flake retouched into point	–	1	–	–
Other	–	1	1	3
Total	33	125	20	551

[a]Mousterian horizons 1 and 2 combined.
[b]Upper Paleolithic levels 6 and 5 combined.

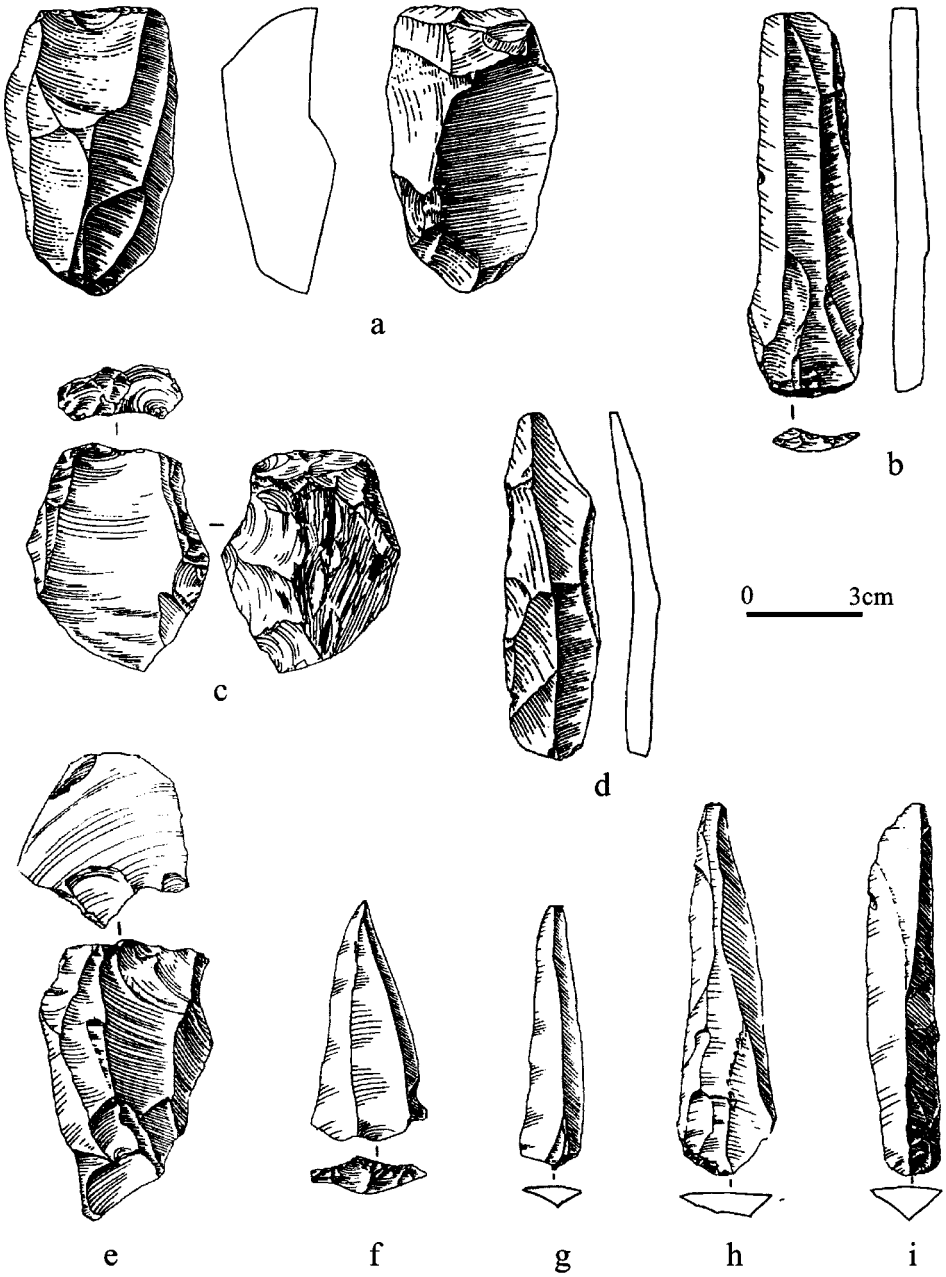

FIG. 4. *Cores and blanks from stratum 3 at Chikhen Agui.* a, c, e, *flat-faced* (*"Levallois"*) *cores;* f, *Levallois point;* b, d, h, *Levallois blades;* g, i, *subprismatic blades.*

Finally, Chen and Yuan (1988) report on bone-derived U/Th ages from the "Lower Cultural Level" at Shuidonggou ranging from 40,000 to 32,000 years B.P. (calendric). Though not unreasonable given the character of the Shuidonggou industry, U/Th dating of bone has to be treated with extreme caution (Bischoff et al. 1988). Recent AMS radiocarbon dates from Shuidonggou Locality 2 strongly support a model of increasing occupation intensities between 26,000 and 25,000 B.P. (Madsen et al. n.d.). A total of 3,806 specimens excavated in 1980 were analyzed in 1998. The materials recovered from strata 6, 7, and 8b are identical in composition and are combined in all of the following presentations.

Shuidonggou is located in an area of abundant alluvial gravels. Derived from these local sources, the two most common raw materials used in core and tool reduction

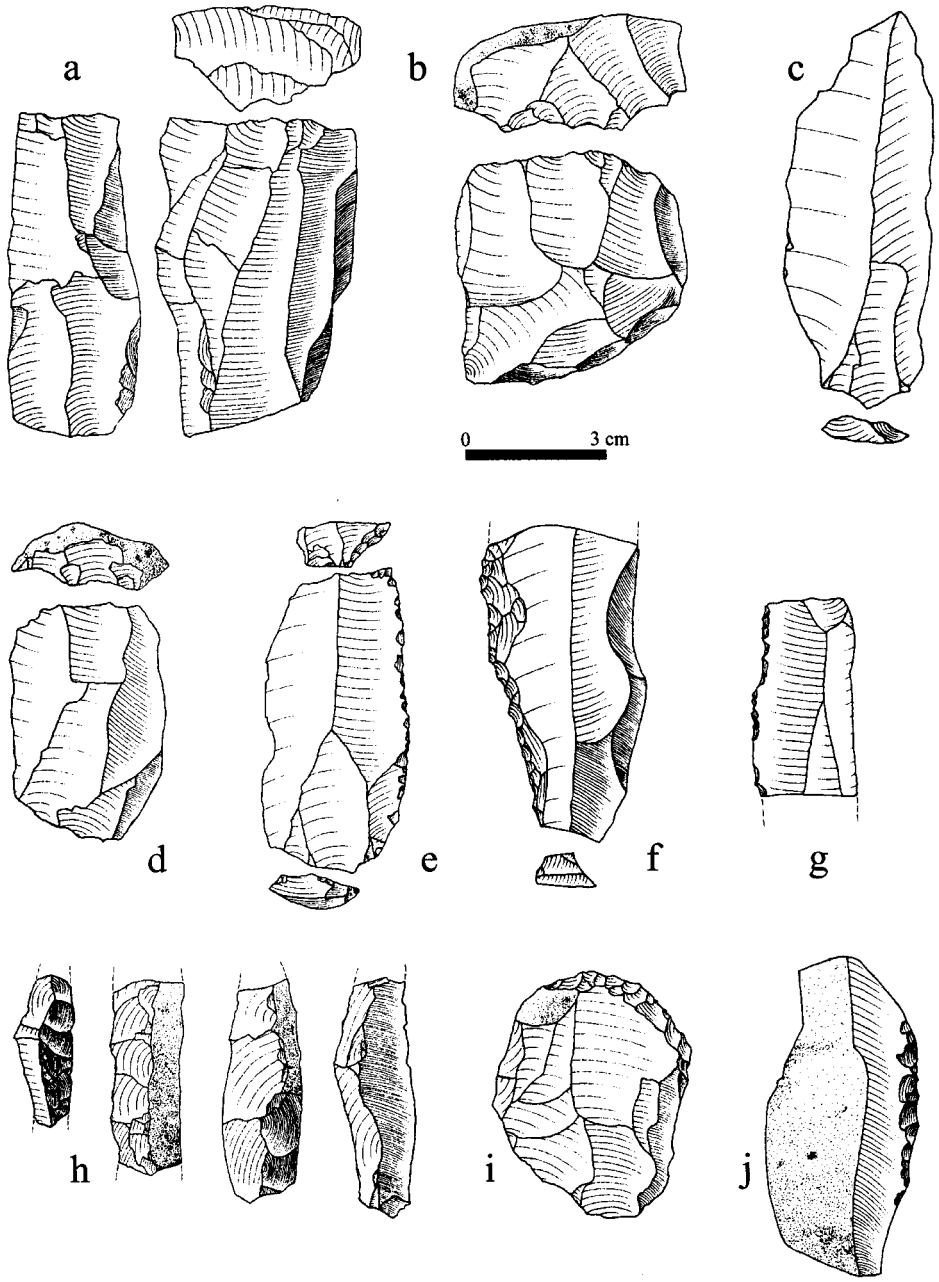

FIG. 5. *Cores, blanks, and tools from Shuidonggou.* a, b, d, *flat-faced* (*"Levallois"*) *cores;* c, e–g, *Levallois blades;* h, *crested blades,* i, *retouched flake end scraper;* j, *side scraper.*

are silicified limestone (n = 2,540, 66.7%) and quartzite (n = 698, 18.3%). Of the 176 cores recovered, formally prepared examples are numerically dominant (n = 94) and Levallois-like cores make up the majority (n = 86) (fig. 5, table 1). Six are classified as Levallois flake and point cores, while the remainder (n = 80) are Levallois-like unidirectional and bidirectional cores dedicated to the production of blade blanks. Other prepared core forms, including two unfinished pyramidal bladelet cores, are represented in low frequencies. The specimens classified as bipolar pebble cores superficially resemble microlithic technology, reflecting in part the constraints of using very small chalcedony pebbles (Madsen et al. n.d.). Generalized flakes constitute more than half (n = 1,507) of all the debitage at Shuidonggou. A portion of the generalized flakes are undoubtedly related to the initial working of prepared cores, though there are no clear attributes to distinguish these flakes from others devoted

TABLE 3
Prepared Core and Blank Reduction Patterns (Residuals in Parentheses)

	Kara Bom Middle Paleolithic[a]	Kara Bom Upper Paleolithic[b]	Chikhen Agui	Shuidonggou	Total
Prepared cores					
Unidirectional linear	16	5	2	36	59
Bidirectional linear	2	3	10	41	56
Unopposed/centripetal	5	3	3	15	26
Total	23	11	15	92	141
Standardized blanks					
Unidirectional linear	84 (1.0)	187 (0.2)	17 (−3.9)	461 (0.6)	749
Bidirectional linear	12 (−2.4)	59 (0.1)	32 (5.1)	135 (−0.6)	238
Unopposed/centripetal	8 (−1.4)	8 (−1.2)	9 (3.8)	24 (−1.0)	49
Total	104	254	58	620	1,036

[a]Mousterian horizons 1 and 2 combined.
[b]Upper Paleolithic levels 6 and 5 combined.

to a core-and-flake strategy. Blanks that are unequivocally related to prepared core reduction constitute 27.8% (n = 620) of the assemblage, and formal blades alone constitute 21.6% (n = 482). The majority of blades are classified as Levallois products (n = 402). Subprismatic blades are uncommon (n = 7) and flake-blades more abundant (n = 112) than in the Kara Bom Upper Paleolithic collections. The tool assemblage from Shuidonggou represents a substantial part of the excavated collections (n = 544, 15%) (table 2). Flake tools constitute 58.6% (n = 319) of the sample, while 26.1% (n = 142) are based on blades. Including flake-blades, nearly 37% (n = 200) of the retouched tool assemblage is based on elongate end products.

COHERENCE OF THE NORTHEAST ASIAN INITIAL UPPER PALEOLITHIC

As in western Eurasia, the Initial Upper Paleolithic emerges in Northeast Asia sometime after 45,000 years ago and is characterized by the elaboration of blade technologies showing a mixture of Middle and Upper Paleolithic characteristics. Beyond this general pattern, it is important to ask how coherent it is in terms of technology and typology. This question is addressed in a series of statistical comparisons of core, blank and tool populations.

Prepared core reduction patterns. Table 3 compares primary reduction patterns for prepared cores from the study assemblages. The category "unidirectional linear" includes cores with unidirectional convergent, subparallel, and parallel removal scars. The category "bidirectional linear" includes cores with opposed platforms and parallel-to-subparallel removal scars. The category "unopposed/centripetal" includes cores with bidirectional unopposed removals, usually from platforms located at right angles to one another, and those with centripetal removals. Linear reduction predominates in all of the assemblages. Unopposed/centripetal cores are exceedingly rare. Unidirectional cores are more abundant in the Kara Bom assemblages, while bidirectional cores are more abundant at Chikhen Agui and Shuidonggou. Sample sizes preclude a statistical assessment of these observations.

Blank reduction patterns. Table 3 conveys the same type of information for dorsal removal scars on standardized blanks. Generalized flakes and technical elements are not included in the analyses. The predominance of linear reduction patterns seen in the core populations is amplified in the blank populations. The general pattern is of an abundance of unidirectional blanks, with bidirectional and unopposed/centripetal blanks occurring in progressively lower frequencies. Standardized residuals provide a measure of the evenness of the reduction patterns across the four assemblages. Bidirectional blanks are apparently underrepresented in the Kara Bom Middle Paleolithic, with a greater than expected frequency of unopposed/centripetal blanks. Chikhen Agui appears to be an outlier in that bidirectional and unopposed/centripetal blanks are overrepresented. The Kara Bom Upper Paleolithic and Shuidonggou assemblages are strikingly similar to one another. Chikhen Agui is significantly different from the Kara Bom Middle Paleolithic (χ^2 = 44.088, d.f. = 2, $p \ll 0.001$), the Kara Bom Upper Paleolithic (χ^2 = 43.955, d.f. = 2, $p \ll 0.001$), and Shuidonggou (χ^2 = 54.063, d.f. = 2, $p \ll 0.001$). Shuidonggou is significantly different from the Kara Bom Middle Paleolithic (χ^2 = 8.025, d.f. = 2, $p < 0.02$) but is indistinguishable from the Kara Bom Upper Paleolithic (χ^2 = 0.441, d.f. = 2, p = .802).

Platform preparation and maintenance. A similar level of agreement characterizes the frequencies of platform types across the assemblages (table 4). Here cortical platforms include blanks retaining all or part of the cor-

TABLE 4
Blank Platform Types (Residuals in Parentheses)

	Kara Bom Middle Paleolithic[a]	Kara Bom Upper Paleolithic[b]	Chikhen Agui	Shuidonggou	Total
Cortical	0 (−2.2)	1 (−3.2)	4 (0.8)	44 (2.7)	49
Simple	7 (−4.2)	47 (−3.1)	19 (0.5)	228 (3.6)	301
Complex/faceted	97 (3.4)	205 (2.9)	35 (−0.5)	348 (−3.1)	685
Total	104	253	58	620	1,025

[a]Mousterian horizons 1 and 2 combined.
[b]Upper Paleolithic levels 6 and 5 combined.

tex on the striking platform. Simple platforms include plain and dihedral types. Complex/faceted platforms include those with multiple flake scars, small facets, and large transverse facets. Overall, there is a clear emphasis on complex/faceted platform types, which indicates special attention to platform preparation and maintenance. At a fine scale, complex/faceted platform types are slightly underrepresented at Chikhen Agui and Shuidonggou, while simple and cortical platforms are underrepresented at Kara Bom. Indeed, statistical comparisons of simple and complex/faceted platform types (excluding cortical types) indicate that Shuidonggou and Chikhen Agui are indistinguishable ($\chi^2 = 0.384$, d.f. = 1, $p = 0.536$) and both are significantly different from the collections from Kara Bom.

Technical (core-trimming) elements. The importance of platform faceting in Northeast Asian prepared core technologies is further supported by the low frequency of platform tablets in all of the assemblages (table 5). Platform tablets are recognized as one distinctive method of platform rejuvenation, especially for Upper Paleolithic prismatic blade technologies, whereby platform shaping problems and prominent flaking errors are corrected by removal of the entire platform (Inizan, Roche, and Tixier 1992). This rejuvenation strategy was infrequently employed at all of the sites. We suggest, moreover, that the tablets identified at Chikhen Agui and Shuidonggou are reduction errors rather than intentional rejuvenation spalls. At Shuidonggou, where a target blade length appears to have driven the intensity of core reduction (Brantingham 1999), the use of platform tablets would tend to reduce expected core use-life by quickly shortening the long axis of the cores.

The broader pattern of occurrence of technical elements is indicative of the similarities in core technologies between sites. Crested blades are consistently the dominant technical element represented except in the Kara Bom Middle Paleolithic. These are followed by "other" technical elements (primarily *outrepassé* blades) and edge elements, or *éclats débordants*. Statistical comparisons indicate that Shuidonggou is indistinguishable from both Chikhen Agui ($\chi^2 = 5.383$, d.f. = 3, $p = 0.146$) and the Kara Bom Upper Paleolithic ($\chi^2 = 0.411$, d.f. = 3, $p = 0.938$). This provides perhaps the strongest evidence that cores were prepared, reduced, and maintained in essentially the same ways in the Kara Bom Upper Paleolithic and at Chikhen Agui and Shuidonggou. The sample from the Kara Bom Middle Paleolithic is too small to evaluate statistically. However, the high frequency of edge elements relative to crested blades hints at some differences in core reduction strategies across the Middle-to-Upper Paleolithic transition.

Retouched tools. The strong similarities in core reduction strategies seen across the sites are not carried over to the retouched tool populations (table 6). The three most common retouched tool types at Shuidonggou are (1) side scrapers, (2) notched-denticulate tools, and (3) retouched blades. In the Kara Bom Upper Paleolithic the three most common tool types are (1) retouched blades, (2) combination tools, and (3) end scrapers and notched-denticulate tools, which occur in equal frequencies. Previous studies identified a much higher frequency of burins in the Kara Bom Upper Paleolithic assemblage, approaching 11% of all retouched tools ($n = 20$) (Derevianko and Markin 1997). Goebel (1994) also recorded a greater number of burins than the current study ($n = 13$). Yet in both of these studies burins still fall behind notched-denticulate tools, retouched blades, side scrapers, and irregularly retouched flakes in overall frequency. Our conservative estimate does not differ

TABLE 5
Occurrence of Technical (Core-trimming) Elements

	Kara Bom Middle Paleolithic[a]	Kara Bom Upper Paleolithic[b]	Chikhen Agui	Shuidonggou	Total
Core tablet	–	2	1	7	10
Edge element	7	5	–	17	29
Crested blade	2	21	8	54	85
Other technical element	–	9	7	22	38
Total	9	37	16	100	162

[a]Mousterian horizons 1 and 2 combined.
[b]Upper Paleolithic levels 6 and 5 combined.

TABLE 6
Occurrence of Retouched Tools

	Kara Bom Middle Paleolithic[a]	Kara Bom Upper Paleolithic[b]	Chikhen Agui	Shui-donggou	Total
Side scraper	6	9	2	124	141
Notched/ denticu-late	4	15	1	112	132
Retouched blade	3	38	9	75	125
Retouched flake	2	10	1	71	84
End scraper	2	15	3	64	84
Combination tool	15	29	1	59	104
Transverse scraper	–	2	–	28	30
Other	–	4	1	10	15
Burin	1	3	2	8	14
Total	33	125	20	551	729

[a]Mousterian horizons 1 and 2 combined.
[b]Upper Paleolithic levels 6 and 5 combined.

qualitatively from these earlier studies. Retouched blades are the most prominent tool type at Chikhen Agui, with other tool types occurring in roughly similar frequencies. In the Kara Bom Middle Paleolithic, combination tools are two to three times more frequent than any other tool type. Combining Shuidonggou, Chikhen Agui, and the Kara Bom Upper Paleolithic, the three most common tool types are (1) side scrapers, (2) notched-denticulate tools, and (3) and retouched blades. Clearly, none of these sites falls within traditional typological classifications of the Upper Paleolithic, which emphasize end scrapers, burins, and truncations. Formal end scrapers are present in low frequencies, burins are extremely rare, and truncations are absent.

To satisfy a measure of typological curiosity, it is instructive to count combination tools with classic Upper Paleolithic working edges as discrete types. Counting those with end-scraper edges strictly as end scrapers produces some changes in the rank-order frequencies of tool types. At Shuidonggou, end scrapers jump to the third-most-frequent tool type, behind side scrapers and notched-denticulate tools and ahead of retouched blades and retouched flakes. The rank-order position at Chikhen Agui does not change. In the Kara Bom Upper Paleolithic, end scrapers rise to the second-most-common tool type behind retouched blades. In the Kara Bom Middle Paleolithic, end scrapers surpass notched-denticulate tools, retouched blades, and retouched flakes to become the second-most-common tool type. The same counting procedure for burin combination tools has less impact. The burin category rises to position eight at Shuidonggou, position two at Chikhen Agui, position five in the Kara Bom Upper Paleolithic, and position four in the Kara Bom Middle Paleolithic.

The Chikhen Agui and Kara Bom Upper Paleolithic tool assemblages are the most consistent with a typological definition of the Initial Upper Paleolithic. However, the small sample size of the Chikhen Agui assemblage must be taken into account. In addition, the typological relevance of retouched blade tools must be questioned; blank size and shape are perhaps the most important morphological determinants among this class of tools. Regardless of the counting procedure, Shuidonggou has a strong Middle Paleolithic typological signature.

DISCUSSION

The Kara Bom Upper Paleolithic, Chikhen Agui, and Shuidonggou assemblages show evidence of the common technological trends accepted for the Initial Upper Paleolithic in western Eurasia. Core technologies generally fall within the Levallois definition and are specialized toward blade production. These generalizations may also hold for other assemblages in Siberia, such as Ust Karakol-1, Kara Tenesh, Byika II, Tolbaga, Varvarina Gora, Khotyk (Unit 2) and Kamenka A (Derevianko, Shimkin, and Powers 1998, Derevianko and Markin 1997, Rezanov et al. 1999), and in Mongolia, such as Tsagaan Agui (White Cave) and the Arts Bogd, Orog Nur 1-2, and Tuin Gol localities (Derevianko et al. 2000*a*, *b*; Derevianko and Petrin 1995; Kozlowski 1971; Krivoshapkin 1998; Okladnikov 1965, 1978). Shidonggou remains the only site in North China known to exhibit these characteristics (Brantingham 1999, Lin 1996). The most striking parallels with western Eurasian sites are found with the "flat cores" and "cores with lateral crests" of the Bohunician (Svoboda and Svoboda 1985:511), as well as recently excavated Initial Upper Paleolithic assemblages in Turkey (Kuhn, Stiner, and Güleç 1999), Syria (Boëda and Muhesen 1993), and the Levant (Bar-Yosef 2000, Marks 1990).

Despite these clear technological parallels, the Northeast Asian assemblages examined here do not conform to western Eurasian typological expectations of the Initial Upper Paleolithic. The high frequencies of side scrapers and notched-denticulate tools are more consistent with Middle Paleolithic typological definitions. End scrapers and burins are present but in relatively low frequencies. Such typological distinctions—including those emphasizing the presence or absence of *fossiles directeurs* such as Emireh points—may at best have regional chrono-stratigraphic relevance, and they probably have little to do with the behavioral and evolutionary processes underlying the origin and elaboration of the Initial Upper Paleolithic. The only substantive difference between the Middle and the Initial Upper Paleolithic in Northeast Asia is a shift in emphasis toward the production and use of blades. Stone tool typology appears to vary independently of this shift, and classic Upper Paleolithic traits such as formal bone and antler technologies do not in fact become prevalent until much later (< 30,000 B.P.) (Derevianko, Shimkin, and Powers 1998).

Why shift to a greater emphasis on blades but retain Levallois core designs? Levallois core geometry is one way to maximize core productivity in terms of number of end products and cutting-edge length while minimizing reduction waste (Brantingham and Kuhn 2001). Far from incompatible, blade and Levallois technologies together may actually extend the broad benefits of Levallois core geometries by allowing for the continuous production of usable blanks, uninterrupted by preparation and maintenance. The behavioral implications of recurrent blade production are twofold. First, the ability to generate more usable blanks per unit volume of raw material may have allowed foraging groups to move farther from specific sources of raw material, and this may have contributed measurably to more flexible activity scheduling on a variety of time scales. Second, the ability to produce standardized blades may have translated into greater predictability in technological performance and greater control over potential foraging risks, particularly if large Initial Upper Paleolithic blades were used as insets in complex composite armatures (Bar-Yosef and Kuhn 1999, Elston and Brantingham 2000).

Such behavioral changes may be reflected in the appearance and spread of the Initial Upper Paleolithic into extreme Northeast Asian environments. For example, the ability to forage away from immediate sources of stone raw material may have facilitated the initial occupation of Chikhen Agui 30,000 to 27,000 years B.P. In contrast, initial occupation of Tsagaan Agui occurred much earlier because of the abundance of chert at the site (Brantingham et al. 2000, Derevianko et al. 2000*a*). Consistent with the above model, the appearance of Initial Upper Paleolithic technologies in the Tsagaan Agui sequence at 33,000 years B.P. is coincident with the first use of a range of high-quality cherts and chalcedonies not available in the vicinity of the site. As at Chikhen Agui, Initial Upper Paleolithic populations occupying Tsagaan Agui were apparently foraging over much greater distances and transporting high-quality stone in the process. A similar shift from short- to long-distance raw-material transport characterizes the Middle-to-Upper Paleolithic transition in the Siberian Altai (Postnov, Anoykin, and Kulik 2000).

With regard to the reliability of Levallois blade technologies and the minimization of foraging risks, we can offer only a tentative conclusion. The heavy emphasis on blade production in the Northeast Asian Initial Upper Paleolithic may underscore the importance of complex composite tools used perhaps as projectiles. However, blades with clear hafting accommodations and bone armatures designed for stone insets are not known definitively in Northeast Asia until the Last Glacial Maximum (Derevianko, Shimkin, and Powers 1998:82, 152).

Turning to issues of chronology, Initial Upper Paleolithic sites in western Eurasia fall within the relatively restricted time range of 45,000–40,000 B.P. (Bar-Yosef 2000; Kuhn, Stiner, and Güleç 1999:507), although certain Bohunician sites may date as young as 36,000 B.P. (Svoboda, Lozek, and Vlcek 1996:107). Initial Upper Paleolithic assemblages are found stratigraphically between Middle and later Upper Paleolithic assemblages at only a handful of sites, among them Ksar Akil (Ohnuma and Bergman 1990). It remains to be determined, therefore, whether they coexisted with distinctive Middle and later Upper Paleolithic industries or consistently occupied an intermediate stratigraphic and chronological position. In Northeast Asia the chrono-stratigraphic situation is no less complex. Initial Upper Paleolithic assemblages occur stratigraphically above Middle Paleolithic industries at Kara Bom and at Tsagaan Agui (Derevianko et al. 2000*a*), but it appears that they do not replace those industries. At Kara Bom there is substantial continuity in core reduction strategies across the Middle-to-Upper Paleolithic boundary (contra Goebel, Derevianko, and Petrin 1993), and indeed the many similarities between Middle and Initial Upper Paleolithic core technologies preclude any simple notion of "replacement." In addition, a number of "classic Mousterian" industries from Siberia (e.g., Okladnikov Cave, Strashnaya Cave, and Ustkanskaya) date as young as 35,000–28,000 years, persisting alongside other Siberian Initial Upper Paleolithic assemblages (e.g., Kara Tenesh, Ust Karakol-1, Kamenka A) that date between 42,000 and 31,000 years (Derevianko, Shimkin, and Powers 1998). The implications of this chronological overlap require further investigation.

Current evidence suggests that Initial Upper Paleolithic industries first appeared in southern Siberia around 43,000 years ago, in the Mongolian Gobi (Tsagaan Agui and Chikhen Agui, respectively) between 33,000 and 27,000 years ago, and in northwestern China at Shuidonggou by 25,000 years ago. Taken together, it appears that the expansion of the Initial Upper Paleolithic was gradual, lasting more than 10,000 years. The Initial Upper Paleolithic may document a revolution in human ecology and behavior, though it arguably occurred on an evolutionary time scale. It is important to emphasize, moreover, that there is as yet *no* fossil evidence to link these assemblages to the spread of any one hominid pop-

ulation, and we lack a comprehensive theory integrating population dynamics, biogeography, and behavioral ecology in such a way as to permit untangling the complex relationships between archaic and modern human populations solely from archaeological data. Though it is tempting to speculate that anatomically modern humans were responsible for the Northeast Asian Initial Upper Paleolithic, any such conclusions must await further theoretical and empirical developments.

CONCLUSIONS

We include the Kara Bom Upper Paleolithic, Chikhen Agui, and Shuidonggou assemblages in the Initial Upper Paleolithic, emphasizing both the striking technological coherence between these assemblages and the technological parallels with accepted Initial Upper Paleolithic assemblages from western Eurasia. Moreover, we hold that there is strong continuity between the regional Middle and Initial Upper Paleolithic. The primary technological features of the Northeast Asian Initial Upper Paleolithic include (1) expanded patterns of raw-material exploitation and transport, (2) emphasis on blade production from Levallois-like prepared cores, (3) high frequencies of retouched blades, (4) occasional classic and elongate Levallois points, and (5) Middle Paleolithic retouched tool types, especially side scrapers, notches, and denticulates. The assemblages discussed here fit the general chronological profile for the origin and elaboration of the Initial Upper Paleolithic, but the ages for the Initial Upper Paleolithic in Mongolia and North China are apparently younger than those documented in western Eurasia.

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New Directions in Paleolithic Archaeology: Asia and the Middle Pleistocene in Global Perspective

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The conference "Asia and the Middle Pleistocene in Global Perspective" was held at the East-West Center in Honolulu March 14–17, 2001.[1] Organized by the Panxian Dadong Collaborative Project,[2] a Sino-American team excavating the late Middle Pleistocene cave of Dadong in China's Guizhou Province, the conference focused on East Asian human cultural innovation and environmental adaptations in comparison with other world regions. Twenty-seven scholars[3] from the United States, China, Korea, Israel, England, France, and Canada came together to present research results and exchange ideas about their work in East, Central, South, and Southwest Asia, Europe, and sub-Saharan Africa. The papers and discus-

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2. The conference organizers from the Panxian Dadong Collaborative Project team were Sari Miller-Antonio (California State University, Stanislaus), Lynne Schepartz (University of Cincinnati), Deborah Bakken (Field Museum of Natural History), and Huang Weiwen and Hou Yamei (Institute of Vertebrate Paleontology and Paleoanthropology [IVPP], Beijing).
3. Conference participants, in alphabetical order, were Deborah Bakken (Field Museum of Natural History), Alison Brooks (George Washington University), Mou-chang Choi (Kon-kuk University Museum, Seoul), Robin Dennell (University of Sheffield), John E. Dockall (Bishop Museum), Gao Xing (IVPP), Naama Goren-Inbar (Hebrew University), Hou Yamei (IVPP), Erella Hovers (Hebrew University), Huang Wanbo (IVPP), Huang Weiwen (IVPP), Fumiko Ikawa-Smith (McGill University), Susan G. Keates (Oxford University), Lee Yung-jo (Chungbuk National University, Cheonju), Leng Jian (Washington University), Liu Jun (Liupanshui Cultural Relics Bureau), Sari Miller-Antonio (California State University, Stanislaus), Anne-Marie Moigne (Centre Européen de Recherches Préhistoriques de Tautavel), Lewis K. Napton (Californian State University, Stanislaus), John W. Olsen (University of Arizona), Richard Potts (Smithsonian Institution), Nicolas Rolland (Prehistoric Anthropology Research Canada), Lynne A. Schepartz (University of Cincinnati), Shen Chen (Royal Ontario Museum), Si Xinqiang (Liupanshui Cultural Relics Bureau), Paola Villa (University of Colorado Museum), Wang Wei (Nanning Natural History Museum).

sions examined the development of behavioral complexity through studies of lithic technology, raw-material usage, site formation processes, and paleoenvironment.

Research in China and northern Asia was the topic for the first day of presentations. Members of the Dadong team presented the results of four excavation seasons, emphasizing site formation processes (synthesizing taphonomic studies, stratigraphic analysis, and geochemical work), paleoenvironment, lithic technology, and raw-material usage. The Dadong faunal assemblage contains predominantly large-bodied mammal elements. Cranial and axial skeletal elements are less common than limb elements, suggesting that selective transport of skeletal portions occurred. The idea of selective resource use is also supported by the lithic analysis, which focused on the differential use of limestone, chert, and basalt.

The diverse nature of Chinese Middle Pleistocene lithic technologies was evident from several presentations on important northern and southern localities including the Bose Basin (Guangxi Province), with its large cutting-tool assemblage showing affinities to Mode 2 (Acheulean) technology. Tektites associated with these implements date to 803,000 years ago, establishing the antiquity of this technology in East Asia. Use-wear analyses and refitting studies done on stone tools from Nihewan Basin (Hebei Province) sites have yielded evidence for bone working, meat processing, and a lithic reduction strategy involving core rotation. Further technological insight came from a comparative study of Zhoukoudian Locality 15 and Locality 1 lithics. This study demonstrated that the core reduction strategies at Locality 15 were sophisticated multidirectional and alternate flaking techniques producing disc cores, in contrast to the predominant bipolar flaking strategy apparent at Locality 1.

Other papers addressed the influence of global climatic change in East Asia and provided a critical evaluation of the idea that it was a region of climatic stability during the Pleistocene. As a result of recent research on Chinese loess deposits, sea-level changes along the margins of the Western Pacific, and the extent of glaciation on the Tibetan Plateau, East Asia is now seen as having undergone significant environmental fluctuations. Discussion of the Late Middle Pleistocene formation of the Three Gorges area of the Yangtze River region highlighted the urgent need for protection of archaeological resources in this fragile and threatened environment.

The next day and a half of presentations was devoted to research in the broader geographic context of Eurasia and Africa. Participants presented interpretations and syntheses of data on population expansion and human migrations as responses to climatic change, the chronology of the earliest Europeans and associated technological hallmarks, the use of ochre, and social networks. A report on the fauna from the Turubong Cave Complex documented the expanding Early Paleolithic and paleoclimatic record of Korea. This rich faunal assemblage with cut-marked bone shows variability in butchering techniques by animal body part. Following the recent scandal involving the planting of artifacts in Japanese Early Paleolithic localities, which shocked the global archaeological community, Japanese scientists are now beginning to assess the extent of the damage to their collective knowledge of the Early Paleolithic. This will eventually entail reexamination of the archaeological materials and excavation histories of several hundred sites in the archipelago.

At European localities the occurrence of contemporaneous biface and nonbiface assemblages (interstratified at some localities) is interpreted as indicating activity differences rather than cultural distinctions, and the low frequency of bifaces in cave localities supports this functional explanation. The use of nonlithic tools such as elephant-bone bifaces, wooden spears, and bone harpoons is a behavioral adaptation that accompanies increased complexity and planning strategies in lithic technologies such as Levallois and other prepared core techniques. Presentation of a systematic study of carnivore bone "piercing" and hole production demonstrated the need for careful taphonomic analysis based on the observation that carnivore signatures and human behavior patterns can produce strikingly similar assemblages.

In comparison with Asia and Europe, the African Middle Stone Age documents human exploitation of a wide range of large and small taxa as well as fish and shellfish and less dependence on large mammals. It is significant that in both Africa and western Asia after 350,000 years ago, the behavioral adaptations recognized as "Middle" Paleolithic occurred exclusively in already large-brained *Homo* during times of climatic shifts. In Asia, drier periods in the Middle Pleistocene could have produced more favorable environments for grassland-adapted hominids, suggesting an increase in colonization of Asian environments during this time. Recovery and analyses of paleobotanical specimens at Gesher Benot Ya'aqov are adding extraordinary paleoenvironmental information to our knowledge of the Levantine corridor, a critical thoroughfare for humans and other animal populations.

The conference concluded with a series of three roundtable discussions. The first focused on lithic technology, where the theme of selective raw-material use was continued in discussion of the constraints imposed by raw materials and the recognition and quantification of technological characteristics of artifact assemblages. Reports on the application of low-power use-wear studies to Middle Pleistocene tool assemblages raised the question of the analytical ability to distinguish impact damage on points (i.e., projectiles) from utilization of the same implements as cutting tools.

Discussants stressed the need for studies that clarify raw-material sources for Middle Pleistocene localities, especially in China, in order to establish whether long-distance (>35 km) transport of materials was a feature of hominid adaptation. The question of presence or absence of Levallois technology (*sensu stricto*) in China stimulated a conversation on the idea that there are different types of core preparation and systematic flaking techniques. Participants agreed that assemblage analyses should focus on identifying the technological correlates

of cognition and recognizing them as specific adaptations to different environments.

The second round-table session, on paleoenvironment, began by examining the cyclic nature of global environmental change during the Middle Pleistocene. The extent of glaciation on the Tibetan Plateau was discussed in the context of the barrier it might have posed to human migrations and population expansion. It was suggested that between 1 million and 600,000 years ago a heightened amplitude of environmental variability was established. Discussants recognized that Middle Pleistocene humans already had significant behavioral flexibility to adjust to habitat changes and to colonize marginal areas such as mountainous regions. One distinction between Middle Pleistocene hominid adaptations and later adaptive strategies is the multifunctional/multitask (less standardized, less discrete) artifact morphologies that characterize these tool assemblages (i.e., large cutting tools as butchering implements or chopping implements and as a source of simple sharp-edged flakes).

A third round table addressed the study of behavioral complexity. The discussion became a synthesis for this recurrent theme of the conference. Several participants presented evidence for changing levels of behavioral complexity. These included the early evidence for intricate bone tools (barbed harpoons) in Central Africa; expansion into new and vertically diverse environments in South Asia; the use of ochre burial treatment in Qafzeh; development of species-specific exploitation systems; selective transport and organization of resources, with economical use of raw material at Dadong; the increasingly systematic extraction of bone marrow at Orgnac 3; spatial distinctions to create an organized habitat in Lazaret Cave; and the role of fire production and home bases in Middle Pleistocene population expansions. Other important data, such as the human figurine from Berekhat Ram in Israel, were discussed. Participants also considered the role of adaptationist thinking in behavioral complexity studies by responding to the question: Do we ever find evidence that Middle Pleistocene humans didn't behave intelligently? The numerous affirmative examples suggest that there is a range of hominid abilities but the ones illustrating "intelligence" as we recognize it today are much more likely to be investigated and reported.

The conference was productive and successful as a forum for exchange between parts of the world that have long been studied in relative isolation. Most important, it gave a collegial group of scholars the opportunity to meet and discuss and expand upon ideas that are common to paleoanthropological investigations of the Middle Pleistocene.

Selected papers from the conference will be edited by Miller-Antonio, Schepartz, and Bakken and published as a special issue of *Asian Perspectives*.

The Origin of the Canary Island Aborigines and Their Contribution to the Modern Population: A Molecular Genetics Perspective[1]

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The Canarian Archipelago is located between latitudes 27°37′ and 29°24′ North. Fuerteventura, only 100 km from Cape Juby, is the island closest to the Northwest African shore (fig. 1). Although ancient knowledge of the islands may be deduced from the classical literature, the effective incorporation of the archipelago into the European world was delayed until the 14th century. In contrast to other Northwest African archipelagos such as Madeira, the Azores, and Cape Verde, the Canary Islands were inhabited by white people with their roots in the Neolithic (Onrubia-Pintado 1987). Europeans conquered the Canary Islands in the 15th century, beginning with Lanzarote in 1402 and finishing with Tenerife in 1496. The Normans conquered Lanzarote, Fuerteventura, and El Hierro without resistance because these islands had been partially depopulated by earlier slave raids. The occupation of the other islands by the Spaniards was rather violent. Many aboriginal inhabitants were killed, and others were enslaved in large numbers to defray the cost of these military expeditions. After the conquest, Europeans settled in increasing numbers on all seven islands. In addition, the need for a farm labor force gave rise to the importation of slaves. Initially, people were brought from neighboring Northwest Africa mainly to the depopulated islands of Lanzarote, Fuerteventura, and El Hierro. Later, the Portuguese transported sub-Saharan African slaves to the archipelago as labor for sugarcane production. In time, the slaves were freed and integrated into the islands population. From this preamble it is easy to deduce that the modern population of the Canary Islands is an amalgam of different ethnic groups which had distinctive settlement patterns on each island (Ladero-Quesada 1979). This helps us to understand the difficulty that scientists since the 19th century have had in trying to answer some crucial questions about the past and the future of the Canarian aborigines: When did the first settlers arrive on the islands? Where did they come

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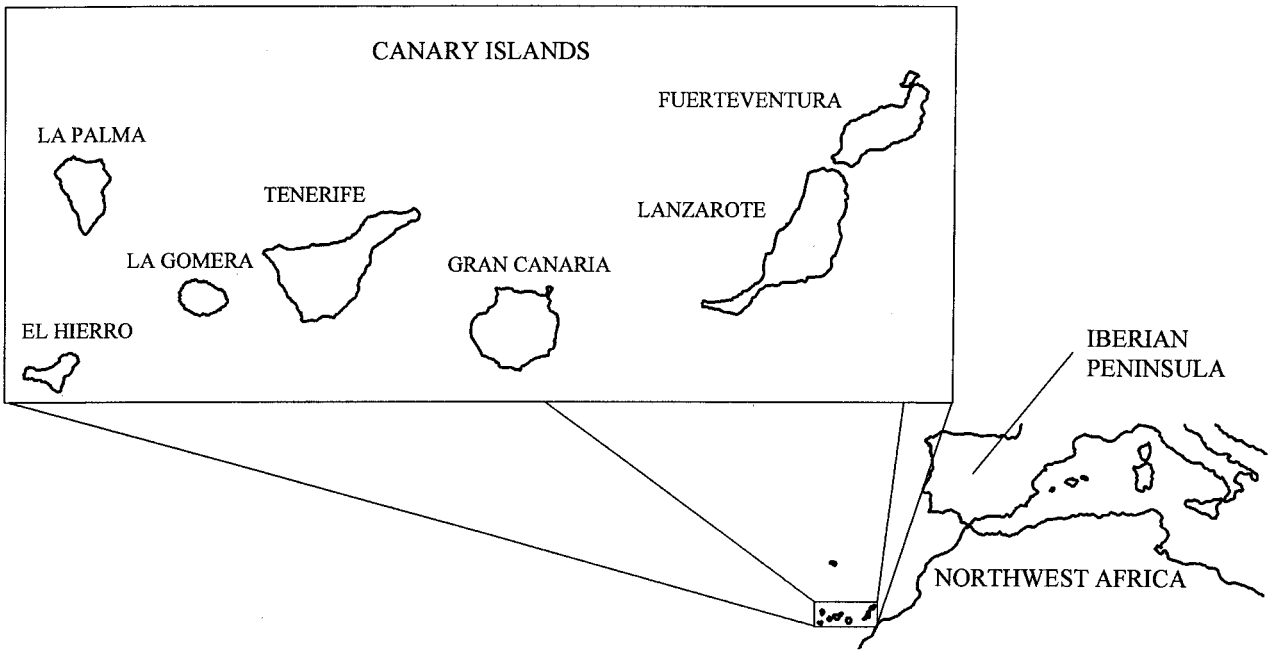

FIG. 1. *Geographic location of the Canary Islands.*

from? How many of their descendants persist in the present-day population? According to direct and indirect ^{14}C dating, the most ancient human settlement seems to be no earlier than 2,500 B.P. (Onrubia-Pintado 1987). Anthropology and archaeology provide important data for tackling the remaining questions.

ANTHROPOLOGICAL AND ARCHAEOLOGICAL CONTRIBUTIONS

From the beginning (Verneau 1887) anthropological studies have revealed at least two human types in the Canarian prehistoric remains: the Cro-Magnon and the proto-Mediterranean. Negroid characters were originally detected but later dismissed (Schwidetsky 1956). The main affinities of these types are with the Mektta-Afalou and the robust proto-Mediterranean types associated in the Maghreb with Paleolithic and Neolithic cultures respectively (Camps-Fabrer 1989). Comparative osteological studies of prehistoric collections and modern rural populations from Gran Canaria (Fusté 1958) and Tenerife (Schwidetzky 1975) testify to a significant persistence of prehistoric traits in current populations. It is estimated that at the end of the 16th century approximately two-thirds of the Canarian population were descendants of the pre-Hispanic aborigines (Wölfel 1930). Among the modern islanders, those of La Gomera display the most aboriginal physical characteristics (Schwidetzky 1975). The archaeological record also suggests the existence of superposed pre-Hispanic cultures with strong insular features (Onrubia-Pintado 1992). Pottery and rock engravings from Tenerife and La Gomera present the most archaic traits. In contrast, the eastern islands show various series of ceramics and inscriptions that are unquestionably products of successive contacts with the cultures of the Maghreb and the Western Sahara (González and Tejera 1990). Although these data seem to point to more than one arrival on the islands, they are compatible with a single wave if the anthropological heterogeneity detected in the aborigines was already present in the parental population and if the probable secondary cultural contacts had little demic impact on the primitive settlers. In this respect, it must be mentioned that there was important physical variability in the human remains exhumed from Neolithic settlements in the Maghreb (Brett and Fentress 1996).

THE CONTRIBUTION OF MOLECULAR GENETICS

Genetic studies have brought new perspectives to the history of the Canary Islands. Sampling methods are extremely important when there are high levels of gene flow between the populations to be compared. Demographic expansion in the Canaries has been impressive. The total aboriginal population in the 15th century was estimated at around 60,000 (Macías-Hernández 1988). Currently, the population is approximately 1,600,000, and an important part of this demographic increase is due not to births but to adult immigration. To avoid this recent input we took our blood samples from unrelated individuals all of whose known ancestors were born on the same islands, deferring the immigration impact about three generations. Therefore our samples reflect the Canarian population at the beginning of the 20th

TABLE 1
MtDNA Haplogroup Frequencies in Different Populations

	Populations[a]										
Haplogroups	Lan[b]	Fue[b]	GC[b]	Tfe[b]	Gom[b]	Pal[b]	Hie[b]	CanT[b]	I.Pen[c]	NWA[d]	WSSA[e]
Sample size	33	36	45	73	33	50	30	300	839	345	240
H/HV*/U* /R*	0.424	0.472	0.422	0.438	0.303	0.38	0.333	0.403	0.545	0.325	0.013
pre-*HV	–	–	–	–	–	–	–	–	0.007	0.009	–
pre-V	–	0.028	0.067	0.041	–	–	–	0.023	0.050	0.049	–
U1	–	–	–	–	–	–	–	–	–	0.003	–
U2	–	–	–	0.014	–	0.020	–	0.007	0.014	–	–
U3	–	–	–	–	–	–	–	–	0.006	0.032	–
U4	–	–	–	0.014	–	–	–	0.003	0.012	–	
U5*	0.061	–	–	–	–	–	–	0.007	0.002	0.006	–
U5a*	0.030	0.139	–	–	–	0.040	0.033	0.030	0.036	0.012	0.008
U5a1*	–	–	–	0.027	–	0.020	–	0.010	0.010	0.009	–
U5a1a	0.030	–	–	0.014	–	–	–	0.007	0.007	–	–
U5b	–	–	–	0.041	0.030	–	–	0.013	0.005	0.012	–
U6	0.152	0.111	0.133	0.055	0.364	0.160	0.100	0.140	0.019	0.148	0.013
U7	–	–	–	–	–	–	–	–	0.001	–	–
K	0.030	–	0.089	0.055	–	–	0.100	0.040	0.054	0.032	–
T*	0.091	0.139	0.067	0.082	–	0.160	0.067	0.090	0.052	0.026	–
T1	–	0.056	–	0.041	0.091	0.020	0.033	0.033	0.020	0.020	–
J*	0.091	–	0.067	0.014	0.061	0.080	0.067	0.050	0.042	0.032	–
J1*	–	–	–	0.027	–	–	–	0.007	0.001	0.003	–
J1a	0.030	–	–	–	–	0.020	0.033	0.010	0.004	0.006	–
J1b*	–	–	–	–	–	–	–	–	0.004	0.003	–
J1b1	–	–	–	–	–	–	–	–	0.004	–	–
J2	–	–	–	0.014	–	–	–	0.003	0.012	–	–
IWX*	–	–	–	–	–	–	–	–	0.001	0.003	–
I	–	–	–	0.014	–	–	0.067	0.010	0.008	–	–
W	–	0.028	–	–	–	0.040	–	0.010	0.021	–	0.013
X	–	–	0.044	0.027	0.030	0.060	–	0.027	0.013	0.012	0.004
L3*	0.030	–	–	0.027	–	–	0.033	0.013	0.011	0.020	0.054
L3a*	–	–	–	–	–	0.033	0.003	–	–	–	
L3b	–	–	0.022	0.014	–	–	–	0.007	0.006	0.023	0.104
L3d	–	–	–	–	0.091	–	–	0.010	–	0.009	0.075
L3e	–	–	–	–	–	–	–	–	0.006	0.014	0.017
M1	–	0.028	–	–	–	–	–	0.003	0.005	0.020	–
A	–	–	–	–	–	–	0.033	0.003	–	–	–
C	–	–	–	0.014	–	–	–	0.003	–	–	–
L1a	–	–	–	–	–	–	–	–	–	0.006	0.008
L1b	0.030	–	0.044	–	0.030	–	0.067	0.020	0.008	0.064	0.188
L1c	–	–	–	–	–	–	–	–	0.001	0.006	0.038
L2	–	–	0.044	0.027	–	–	–	0.013	0.013	0.099	0.467

[a]Abbreviations are Lan, Lanzarote; Fue, Fuerteventura; GC, Gran Canaria; Tfe, Tenerife; Gom, La Gomera; Pal, La Palma; Hie, El Hierro; CanT, Canaries; I.Pen; Iberian Peninsula; NWA, Northwest Africa; WSSA, West Sub-Saharan Africa.
[b]From Rando et al. (1999).
[c]From Bertranpetit et al. (1995), Côrte-Real et al. (1996), Torroni et al. (1998), Salas et al. (1998), and unpublished results.
[d]From Rando et al. (1998) and unpublished results.
[e]From Graven et al. (1995) and Rando et al. (1998).

century, when immigration was not so strong. These samples were compared with those from their most probable parental populations: Spaniards, Northwest Africans, and Sub-Saharan Africans. This population analysis began in the eighties and included classical markers, mitochondrial DNA (mtDNA) sequences, and Y-chromosome polymorphisms.

We analyzed 13 classical markers—5 blood groups (A_1A_2Bo, CDE, MNSs, Duffy, and P1) and 8 red-blood-cell enzyme loci (ACP-1, EsD, G6PD, GLO1, GPT, PGD, PGM-1, PGP). Sample sizes, allele frequencies, and methodology are reported elsewhere (Martell et al. 1986; Morilla et al. 1988; Afonso et al. 1989; Pérez et al. 1991; Larruga et al. 1992; Pinto et al. 1994, 1996*a*; Flores et al. 2000). The mtDNA analysis was based on 403 base-pair sequences from the noncoding control region. In addition, restriction-fragment-length polymorphisms (RFLP) were used to sort these sequences into haplogroups whose assigned geographic origins were known (Macaulay et al. 1999). The methodology and the samples from the Canary Islands, the Iberian Peninsula, and Northwest and Sub-Saharan Africa are in Pinto et al. (1996*c*) and Rando et al. (1998, 1999). Additional samples were taken from other sources (see table 1). The Y chromosome was

studied using the following nine biallelic markers: DYS287, DYS271, M9, SRY1532, SRY2627, 92R7, 12f2 (Bosch et al. 1999), M13 (Underhill et al. 1997), and PN2 (Hammer et al. 1997). Combining the allelic states of each marker could give a total of 11 haplotypes. Haplotype relatedness and nomenclature are in Bouzekri et al. (1998) and Bosch et al. (1999) and sample sizes in Flores et al. (2001).

The results of these analyses are as follows:

Classical markers. The first genetic analysis carried out on the modern Canarian population was on the Rh blood group (Guasch et al. 1952), and its results were those of a typical European population with some African admixture—the prevailing result in all subsequent studies. Compared with those from their most probable parental populations, our data on Canarian blood-group allelic frequencies are within the range of Europeans but with a small African contribution (Pinto et al. 1996*a*). Red-blood-cell enzyme polymorphisms also confirmed the existence of this African component in the Canarian population. For example, the R variant of acid phosphatase (ACP-1), present in Gran Canaria (0.8%) and Lanzarote (0.6%), was not detected in the European sample but had frequencies of 1.1% and 6.8% in Northwest and Sub-Saharan African samples respectively (Martell et al. 1986, Morilla et al. 1988, Pérez et al. 1991, Pinto et al. 1994). The glucose-6-phosphate dehydrogenase Negroid allele G6PD A+ was also consistently detected on all islands; frequencies ranged from 0.5% on La Gomera to 3% on Lanzarote, compared with 2% in the Northwest African and 27% in the Sub-Saharan African samples. Although a Negroid component of the Canarian indigenous population cannot be excluded, these alleles probably came to the islands as a result of the sub-Saharan slave trade (Lobo-Cabrera 1982). This hypothesis is supported by the fact that the highest G6PD A+ allele frequencies were found in localities near former slave harbors such as La Aldea (7%) in Gran Canaria or Adeje (2.4%) in Tenerife. G6PD A+ allele sequences (Pinto et al. 1996*b*) showing more sharing between Canarians and Sub-Saharan Africans (45%) than between Canarians and Northwest Africans (4%) reinforce this interpretation.

MtDNA variation. MtDNA analysis is used extensively to study the female contribution to the origin and migration of human populations. A close relationship between the maternal lineages of both Canarian and Northwest African populations was found by Pinto et al. (1996*c*). Following Di Rienzo and Wilson (1991), we tested the degree of sharing of lineages between Canarians and their putative ancestors. Pairwise comparisons of Canarians with Spaniards ($\chi^2 = 9.19$; d.f. = 1; $p < 0.01$) and with Sub-Saharan Africans ($\chi^2 = 8.67$; d.f. = 1; $p < 0.01$) were highly significant but comparison with Berbers ($\chi^2 = 0.10$; d.f. = 1; p = n.s.) was not significant. Following this pilot study, we conducted a more thorough analysis of data from all seven islands (Rando et al. 1999). MtDNA phylogeographic analysis (Macaulay et al. 1999) allowed us to sort most European and African mtDNA sequences into basal haplogroups with geographical assignation (table 1). As a rough approximation we can consider all sequences grouped in L haplogroups as of primarily Sub-Saharan African origin. These sequences would represent 6–7% of the Sub-Saharan input, El Hierro (13%), La Gomera (12%), and Gran Canaria (11%) being the three islands showing the most influence; at the other end is La Palma, lacking any L sequence. The sub-haplogroup U6 is considered a specific Northwest African marker (Côrte-Real et al. 1996, Rando et al. 1998). Its mean frequency for the Canary Islands (14%) is similar to that for Northwest Africa (14.8%). If we employed the relative frequency of U6 in the two areas to extrapolate the North African maternal contribution to the Canaries it would be 100%, which is clearly inconsistent with the historical record. Severe founder effects have to be invoked to explain the high U6 frequencies in the Canary Islands, and at the same time they would explain the oscillation in frequency among the islands, ranging from 5.5% in Tenerife to 36.4% in La Gomera. The geographical distribution of U6 in the archipelago provides additional information concerning the way the islands were colonized. The diversity and number of sequences are greatest in Lanzarote and Fuerteventura, the islands closest to Africa, and gradually decrease to the west, suggesting an initial settlement process concordant with the stepping-stone model (Rando et al. 1999).

Y-chromosome polymorphisms. Y-specific polymorphisms represent the male counterpart of mtDNA polymorphisms. Their paternal transmission and lack of recombination offer the same advantages for the study of male lineages as mitochondrial sequences do for female ones. We have some preliminary results for males of the same Canarian, Iberian Peninsular, and Northwest and Sub-Saharan African samples used in the mtDNA analysis. The total Canarian sample shows twice as many similar haplotypes with the Iberian Peninsular (31%) as with the Northwest African (13%) sample and very few (3%) with the Sub-Saharan African sample. Haplotypes mostly restricted to geographical areas or to ethnic groups can be used as reliable indicators of gene flow between populations. Haplotype 8 is a good indicator of sub-Saharan African influence. It is present at 71% in the Sub-Saharan African, 8% in the Northwest African, and only 0.4% in the Iberian Peninsular sample. Values range from 0% in La Gomera, Tenerife, and Fuerteventura to 4% in El Hierro. Haplotype 25 has its highest frequency in the Northwest African sample (66%), though it is also found in the Sub-Saharan African (11%). Regrettably, it is not a good indicator of male Northwest African influence in the Canary Islands because, as it is in the rest of Europe (Hammer et al. 1997), it is also present in the Iberian Peninsular population (12%). However, the higher frequencies on some islands such as Gran Canaria (18%), Tenerife (18%), and Fuerteventura (16%) than in the Iberian Peninsular sample, if not due to founder effects, could be attributable to Northwest African influence. Haplotype 21 is widespread in sub-Saharan Africa (Scozzari et al. 1999), in our sample representing 11%. It is practically absent in Europe, including the Iberian Peninsula. Although it was not found

TABLE 2
Estimates of Admixture in the Canarian Population and Contribution of Sexual Asymmetry (Percentages)

	Populations		
	Iberian Peninsular	Northwest African	West Sub-Saharan African
Estimate of admixture			
Blood groups[a]	72	22	6
Enzymes[b]	70	20	10
MtDNA[c]	36	43	21
MtDNA[d]	62	33	5
Y-chromosome	91	8	1
Contribution of sexual asymmetry			
MtDNA and nuclear			
Males	46.5 (65)	2.0 (10)	1.5 (19)
Females	24.5 (35)	19.0 (90)	6.5 (81)
Y-chromosome and nuclear			
Males	45.5 (64)	4.0 (19)	0.5 (6)
Females	25.5 (36)	17.0 (81)	7.5 (94)

NOTE: In parentheses, percentage contributed by each parental population.
[a]Roberts et al. (1966).
[b]Pinto et al. (1994).
[c]Pinto et al. (1996*c*).
[d]Rando et al. (1999).

in our Moroccan sample or in any Northern African one analyzed Scozzari et al. (1999), it reaches 12% in our Saharan samples. In the Canaries, haplotype 21 is present only on the easternmost islands of Lanzarote (1%) and Fuerteventura (3%). Finally, haplotype 22 has a supposed origin in Iberia (Hurles et al. 1999). In our Iberian sample it has a low frequency (4%) compared with those found for Catalans (28%) or Basques (12%) but similar to that for Galicians (Hurles et al. 1999). This haplotype can be used as a relative measure of the male Iberian input into the Canary Islands. Its values are comparatively high on some islands, such as El Hierro (13%) and Fuerteventura (8%), but it has not been detected on La Gomera and Lanzarote, pointing to a heterogeneous Iberian colonization and/or strong founder effects.

Genetic contribution of the parental populations. The present Canarian genetic structure has been considered a result of admixture/gene flow from other populations, North Africans, Europeans (mainly from the Iberian Peninsula), and Sub-Saharan Africans being their most probable ancestors (Roberts et al. 1966; Pinto et al. 1994, 1996*c*). Using least-square methods of admixture (Elston 1971), estimates of the relative genetic contribution of the parental populations have been obtained for classical, mtDNA, and Y-chromosome polymorphisms (table 2). Classical marker analyses agree upon admixture proportions of about 65–75% for European, 20–30% for Northwest African, and 5–10% for Sub-Saharan African samples. Quite different percentages were obtained with mtDNA. Pinto et al. (1996*c*) estimated the female contributions from Northwest Africa (43%) and sub-Saharan Africa (21%) as adding up to an African contribution of around 60%. Rando et al. (1999), however, employing a larger sample and using the same least-squares method but considering all mtDNA haplotypes as alleles of a gene, obtained significantly different estimates, with a preponderant (62%) Iberian component, 33% due to Northwest Africa, and negligible sub-Saharan African influence. This sharp discrepancy seems to be due to the small sample size used in the Pinto et al. approximation. However, when mtDNA haplogroup frequencies instead of haplotype frequencies were used, the main contribution was again from Northwest Africa (57%) compared with the Iberian Peninsula (43%), evidencing a surprising lack of matches between Canarian and North African haplotypes belonging to the U6 subgroup. The two estimates seem to measure different aspects of a more complex phenomenon. In view of this, we adopted a third approach based on the continental affinities of several Canarian mtDNA lineages that met several criteria (Rando et al. 1999) for being considered potential founders. A conservative 38% contribution of female lineages from Africa was obtained. Finally, the African male contribution was less than 10%, leaving 91% to paternal Iberian input on the Canary Islands—a value even higher than that obtained with autosomal markers (70%).

Asymmetry in the male and female contributions. The differences in the relative contributions of African and Iberian populations to the Canary Islands depending on whether autosomal, mtDNA, or Y-chromosome data are used can be explained by parental immigration that is asymmetrical by sex. Using formulae detailed elsewhere (Hsieh and Sutton 1992), we inferred the expected male contributions from autosomal and mtDNA real data (table 2). The values are in agreement with those obtained from the Y-chromosome experimental results.

Concordant results are also obtained when the female contribution is estimated from the Y-chromosome and autosomal information (table 2). Whereas the Iberian gene flow to the Canarian population consisted mainly of male lineages, Africans contributed more with female ones. This sexual disequilibrium has a plausible historical explanation based on the asymmetrical migration of the Iberian conquerors to the Canary Islands and by their different behavior depending on the sex of the aborigines. In occupation times, the Iberian migration was mainly a male migration, so they displaced autochthonous males by death or deportation but mixed with aboriginal females. Whereas autosomal genes of Iberian and aborigines mixed and recombined, the Iberian Y-chromosome lineages have practically replaced the aboriginal male background, while the mtDNA lineages continue to be mainly aboriginal.

Molecular information from ancient aboriginal remains. Even admitting that the relative African contributions to the Canary Islands were real, uncertainty remains about whether this is due to the first settlers, to the historic slave trade, or to a mixture of the two. Molecular information from the relatively abundant remains of Canarian aborigines might confirm whether their genes are present in the modern Canarian pool, but data of this type are still very preliminary. In the early sixties the frequencies of ABO alleles in the primitive inhabitants were obtained by analyzing the soft-tissue remains of 370 mummified bodies (Schwarzfischer and Liebrich 1963). A very high frequency (84%) of the O allele, clearly different from that of the present-day population, was observed. If this is not due to technical or sampling difficulties, it may reflect the strong effect of the continuous migration of people of different origins on the original aboriginal gene pool after the conquest. The modern population of La Gomera, with the highest estimated value for this allele (78%), has the frequency closest to the aboriginal estimate. Curiously, La Gomera also has by far the highest percentage (36%) of the Northwest African mtDNA U6 cluster (Rando et al. 1999). First results on aboriginal mtDNA sequences obtained from DNA extracted from ancient aboriginal teeth have recently been reported (Rando 1999). Though the study was based on only 20 PCR partial amplifications, the results are very informative. Two of the founder mtDNA sequences proposed by Rando et al. (1999) were revealed by this analysis. Both have been detected in Northwest Africa but are absent or very rare in Europe. One, with a transition in 16260, belongs to haplogroup H. The other, with transitions 16163, 16172, 16219, is currently the most abundant U6 haplotype in the Canary Islands. Finally, and most surprisingly, there is a high percentage (30%) of sequences with sub-Saharan African motifs carrying transition 16223, which is more abundant in West Saharans and Mauritanians than in Berbers and Moroccans from Northwest Africa (Rando et al. 1998). The latter result suggests the possibility that the first settlers came from Saharan populations.

CONCLUSIONS

With regard to the probable origin of the first settlers of the Canary Islands and to their possible contribution to the modern genetic pool of the present-day islanders, molecular genetics contributes new information that can be summarized as follows: The first colonizers came to the Canary Islands from Northwest Africa, probably from Saharan populations. It seems that a single main prehistoric settlement colonized all the islands from east to west, following a stepping-stone model. Later Northwest African and sub-Saharan African input due to the slave trade had a limited genetic influence. However, in spite of their relative geographic distance, genetic affinities are detectable between El Hierro, Fuerteventura, and Lanzarote, the islands conquered by the Normans at the beginning of the 15th century, pointing to an important homogenization of their populations by human interchange. The aboriginal genetic contribution to the modern Canarian population has a strong sexual bias. Whereas the Y-chromosome-specific male presence is less than 10%, aboriginal mtDNA lineages represent around 45% of the population. The aboriginal component differs, however, on the different islands. La Gomera has the highest percentage and the main central islands the least because of the greater impact of immigration. Furthermore, if we take into account the fact that these aboriginal contributions are measured with a sample from the beginning of the past century and immigration is constantly increasing, it seems that the future of these male and female aboriginal lineages is extinction.

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Male Contribution to Diet and Female Reproductive Success among Foragers[1]

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Male-female bonds in humans have long been attributed to the need for male assistance with provisioning of offspring (Westermarck 1929, Lovejoy 1981, Lancaster and Lancaster 1983). Recently, however, attention has been drawn to several features of modern foragers (and presumably our ancestors) that challenge the paternal-investment theory of human pair bonding: (1) Men in some

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foraging societies contribute less to the diet than women do (Hiatt 1974, Kelly 1995) yet marriage still exists. (2) Forager women are less dependent on husbands when they can gather food and when they live with their own kin, and postmarital residence is much more flexible among foragers than in traditional agricultural societies (van den Berghe 1979, Marlowe 2000*a*). (3) Hunting provides such unpredictable returns that it should be a poor strategy for provisioning children (Hawkes, O'Connell, and Blurton Jones 1991). (4) The foods men acquire (e.g., meat, fish, and honey) tend to be shared widely outside the household (Kaplan and Hill 1985, Bliege Bird and Bird 1997, Hawkes, O'Connell, and Blurton Jones 2001), with the result that the wives of good foragers may receive no more food than others in camp. (5) The effect of fathers' absence on offspring survival does not predict marital stability as well as do men's mating opportunities (Blurton Jones et al. 2000). These challenges to the paternal-investment theory have left some asking just how important provisioning by men is to forager women.

Females should allocate available energy to reducing offspring mortality and/or increasing fertility in ways that maximize their own reproductive success (Lack 1968, Smith and Fretwell 1974). Therefore, in this paper I use demographic data on foragers to test whether male provisioning—for which male contribution to diet serves as a proxy—enhances female reproductive success. While others have analyzed the demography of some foraging populations (Hewlett 1991; Bentley, Jasienska, and Goldberg 1993; Kelly 1995; Pennington 1996; Sellen and Mace 1997, 1999) and some have analyzed male contribution to diet among foragers (Lee 1968, Hiatt 1974, Barry and Schlegel 1982, Ember 1978, Kelly 1995), here I consider the two together. In addition, I control for habitat quality. The comparative approach I adopt ignores variation within societies, which would be more informative about cause and effect, but allows me to generalize across all foraging societies. I will show that male contribution to diet does enhance female reproductive success and go on to discuss the implications of this for our mating system.

MATERIALS AND METHODS

The sample included every society that derived all or almost all of its subsistence from foraging and had data on at least one of the relevant variables, a total of 161. Many of these data come from previous compilations (Hewlett 1991, Kelly 1995, Hill and Hurtado 1996). The variables examined were the following:

1. Primary biomass (kg/m^2), a measure of the resource abundance or quality of a habitat. These data were taken from Kelly (1995) or calculated using his formulas for deriving primary biomass from primary production—the plant biomass expected given the effective temperature and precipitation. Even though there is relatively less edible food for humans as primary biomass increases (per kilogram), the absolute amount of edible food increases because there are more kilograms per square meter (Kelly 1995). Primary biomass (henceforth simply biomass) may be a crude measure of habitat quality, but it is an important control variable in this analysis. Because there was no formula for arctic biomass, arctic societies were excluded from all tests that control for biomass.

2. Male contribution to diet, the percentage of the diet produced by men as opposed to women. In some cases, the data available are only estimates based on the sex that tends to acquire certain foods and the percentage of those foods in the diet (Barry and Schlegel 1982, Kelly 1995), but I used actual measurements wherever they were available. For example, in the case of the Hadza, I weighed all food entering camp, converted this to kilocalories by type of food, and then calculated the percentage of the total kilocalories contributed by adult males. (Ideally, one would measure actual consumption by all members of a household while in camp and out foraging and the exact amount of kilocalories acquired by individuals on their own versus the amount given to them by others over a long period of time and for all households.)

3. Infant mortality, the percentage of children who die within the first year of life (0–1 yr).

4. Juvenile mortality, the percentage of children who die before age 15 (0–15 yr).

5. Postinfant juvenile mortality, mortality between ages 1 and 15 years.

6. Total fertility race (TFR), the mean number of children women have borne by the time they reach menopause.

7. Age at weaning, the mean age at which children are weaned.

8. Interbirth interval, the mean number of years between births, whether the previous child survived or died before the birth of the subsequent child.

9. Female reproductive success, the number of children surviving to age 15, calculated by multiplying TFR by juvenile survivorship (100% − juvenile mortality) and dividing by 100. This takes into account both fertility and mortality and is the best measure of the target of natural selection.

All tests are either Pearson correlations or multiple linear regressions, with $p < 0.05$ recognized as significant.

RESULTS

Variation in male contribution to diet. Male contribution to diet varies from 25 to 100%, with a mean of 64% (S.D. = 18.3, $n = 95$). Because there is less edible plant food for women to gather in colder climates, male contribution is higher at higher latitudes, where effective temperature is lower ($r = -.512$, $p < 0.0005$, $n = 82$). Male contribution is also greater where biomass is greater ($r = .300$, $p < 0.031$, $n = 52$), but because there is no formula for calculating the biomass of arctic environment this correlation is inflated by the exclusion of low-biomass arctic habitats, where the importance of male provisioning is indisputable. In environments where effective temperature is greater than 13°C (between about latitudes 45°N and 45°S, mean male contribution to diet is 55% (S.D. = 15.9, $n = 36$). Not sur-

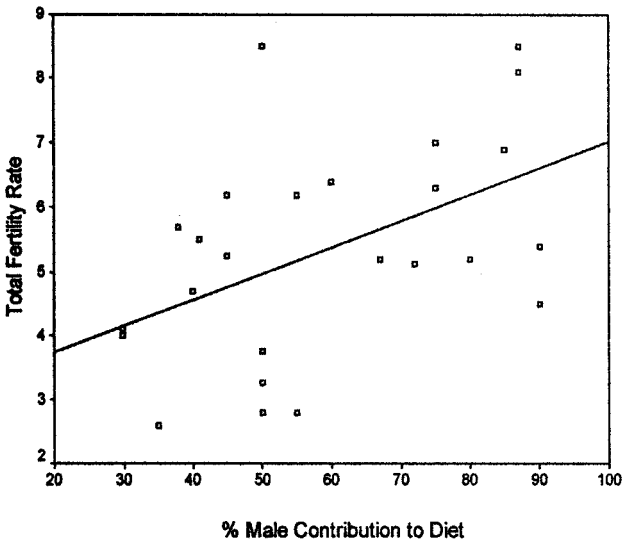

FIG. 1. *Total fertility rate as a function of male contribution to diet* (β = *.497,* p = *0.010,* d.f. = *24*).

prisingly, male contribution to diet is lower where more of the diet comes from gathering ($r = -.742$, $p < 0.0005$, $n = 62$) and higher where more of the diet comes from fishing ($r = .513$, $p < 0.0005$, $n = 62$). It is not significantly higher where more of the diet comes from hunting ($r = .238$, $p < 0.063$, $n = 62$). It is higher in the New World (70%, S.D. = 14.9, $n = 67$) than in the Old World (50%, S.D. = 17.9, $n = 27$), even when effective temperature is greater than 13°C to exclude arctic societies (62%, $n = 20$ versus 47%, $n = 16$). Both hunting and fishing contribute more to the diet in the New World.

Variation in subadult mortality. Mean infant mortality is 22.32% (7.8–46%, S.D. = 9.4, $n = 18$). Infant mortality may be largely due to diseases that paternal investment can do little to ameliorate. Since diseases should be more prevalent in warmer, wetter habitats, we might expect infant mortality to increase with biomass, and to a degree it appears to. Mean juvenile mortality is 45.08% (22–61%, S.D. = 9.4, $n = 20$). Perhaps because the immune system improves with age and exposure, juvenile and postinfant juvenile mortality appear to decrease slightly with increasing biomass, suggesting that infant and juvenile mortality have different causes (and see Sellen and Mace 1999).

Variation in female fertility and reproductive success. Mean TFR is 5.28 (.81–8.5, S.D. = 1.68, $n = 46$). We might expect fertility to be higher in high-biomass habitats, and to a degree it appears to be ($r = .130$, n.s.). However, it should also be affected by mortality, since when a nursling dies and nursing ceases there should be a shorter interval to the next birth. Alternatively, more children may put more strain on parental resources, resulting in higher offspring mortality. TFR is significantly correlated with juvenile mortality ($r = .530$, $p = 0.016$, $n = 20$) but not with infant or postinfant juvenile mortality. Therefore it is probably the mortality of nurslings up to three or four years old rather than infants below one year that is most closely correlated with TFR, but there are too few populations for which nursling mortality rates are available to test this.

Mean age at weaning is 2.62 years (1–4.5, S.D. = .84, $n = 41$) and is slightly less where TFR is higher. Mean interbirth interval is 3.22 (1.75–4.83, S.D. = .76, $n = 18$). The later women wean, the longer lactational subfecundity should delay the next birth, and, as expected, interbirth interval is longer when age at weaning is greater ($r = .803$, $p = 0.016$, $n = 8$). Neither age at weaning nor interbirth interval is correlated with biomass. Mean female reproductive success is 3.11 (1.93–5.27, S.D. = .79, $n = 20$) and appears to increase with biomass ($r = .359$, n.s.).

The impact of male contribution to diet on subadult mortality. There is no relationship between male contribution to diet and infant mortality, even when both biomass and TFR are controlled. The same is true of juvenile mortality and postinfant juvenile mortality, although when biomass and TFR are controlled postinfant juvenile mortality is slightly but not significantly lower where male contribution to diet is greater ($\beta = -.632$, $p = 0.213$, d.f. = 6). The potential benefit of greater male provisioning to older, weaned children in contrast to infants would be straightforward, since their nutritional status could be directly enhanced. Given the small number of cases, all we can say is that male contribution to diet may lower postinfant juvenile mortality but certainly does not appear to lower infant mortality. This again suggests that infant and postinfant juvenile mortality have different causes and that if we could analyze

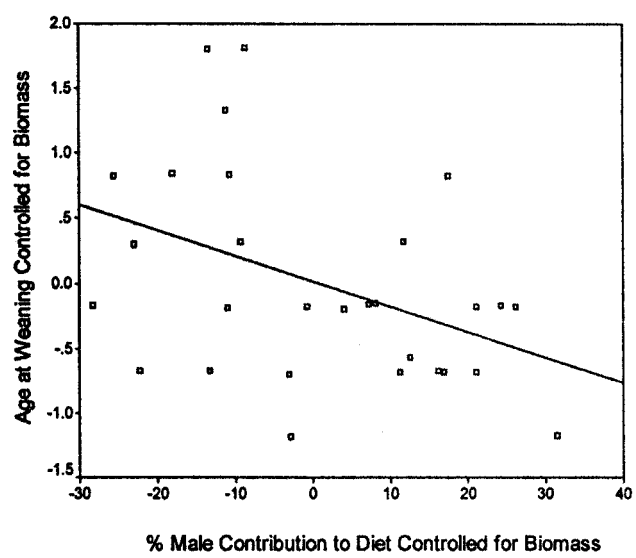

FIG. 2. *Age at weaning as a function of male contribution to diet, controlling for primary biomass of habitat* (β = *.437,* p = *0.028,* d.f. = *26*).

postweaning juvenile mortality (children 4–15 years old), we might see an effect of male contribution to diet.

The impact of male contribution to diet on female fertility and reproductive success. Although greater male contribution to diet may or may not reduce subadult mortality, its impact on fertility is clear. Where male contribution to diet is greater, women's fertility (TFR) is higher ($\beta = .497$, $p = 0.010$, d.f. = 24) (fig. 1). Controlling for biomass, the positive effect of male contribution to diet on TFR is even greater ($\beta = .740$, $p = 0.003$, d.f. = 20). Greater male contribution to diet must allow females to maintain ovarian function better by foraging less and spending less energy or by weaning earlier or both. Age at weaning is lower where male contribution to diet is greater ($r = .279$, $p = 0.081$, $n = 40$) and significantly lower controlling for biomass ($\beta = -.437$, $p = 0.028$, d.f. = 26) (fig. 2).

Because greater male contribution to diet appears to help women wean earlier and have more children, we might expect it to shorten the interbirth interval. However, higher mortality will also have this effect, since a woman can more quickly resume cycling after the death of a child and the end of nursing. Interbirth interval is therefore not a straightforward measure of reproductive success, since it can be short because of high fecundity or because of high mortality. When juvenile mortality is controlled, interbirth interval is shorter where male contribution to diet is greater, though not significantly. Here the lack of significance can be attributed to the inclusion of the Batak, for whom births are concentrated early in life, when interbirth interval tends to be shorter. Mean age at last birth is 26.3 for the Batak (Eder 1987), compared with 39 for other foragers (Kaplan et al. 2000),

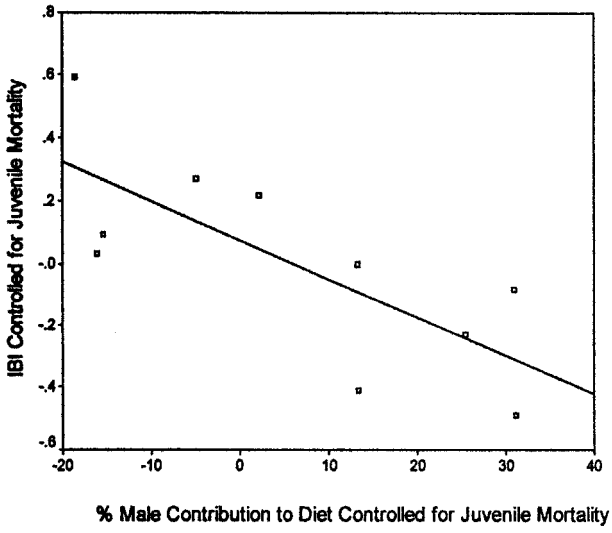

FIG. 3. *Interbirth interval as a function of male contribution to diet, controlling for juvenile mortality, with the Batak excluded* ($\beta = -.765$, p = *0.007*, d.f. = *7*).

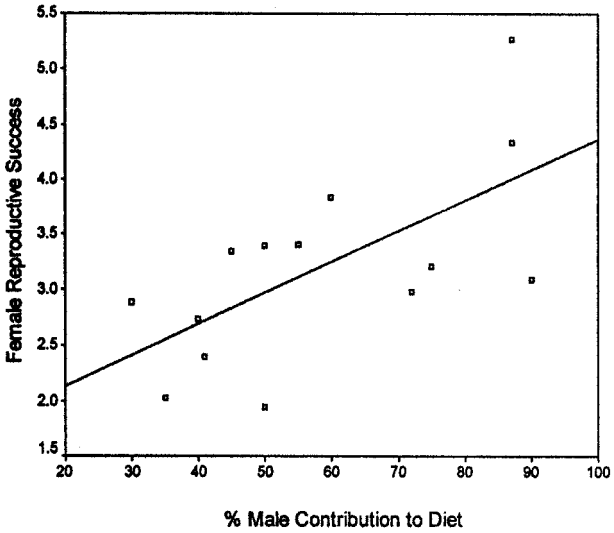

FIG. 4. *Female reproduction success as a function of male contribution to diet* ($\beta = .648$, p = *0.0012*, d.f. = *12*).

partly because women may ingest a sterility-inducing plant after a difficult birth or on reaching a desired number of children (Eder 1987). When the Batak are excluded and juvenile mortality is controlled, greater male contribution to diet does predict a shorter interbirth interval ($\beta = -.765$, $p = 0.007$, d.f. = 7) (fig. 3). When biomass is also controlled, the effect of male contribution to diet is not significant ($\beta = -.585$, $p = 0.076$, d.f. = 5), but if nursling mortality could be controlled there would likely be a stronger effect.

Since greater male contribution to diet increases TFR, which in turn is positively correlated with juvenile mortality, it is not clear whether we should expect greater male contribution to diet to reduce subadult mortality. What matters is whether it leads to greater female reproductive success, the result of trade-offs between fertility and mortality, and it does. The greater male contribution to diet, the higher female reproductive success ($\beta = .648$, $p = 0.012$, d.f. = 12, $n = 14$) (fig. 4). Controlling for biomass makes the effect of male contribution to diet even stronger ($\beta = .861$, $p = 0.007$, d.f. = 10).

Male contribution to diet predicts female reproductive success mainly because it raises TFR. Female reproductive success is not significantly correlated with infant mortality, juvenile mortality, postinfant juvenile mortality, interbirth interval, or age at weaning. The only demographic variable correlated with it is TFR ($r = .808$, $p < 0.0005$, $n = 20$). Controlling for biomass, and regardless of which other demographic variables are entered in a regression analysis, TFR still has the strongest significant relationship to female reproductive success.

Male contribution to diet and TFR are much greater in the New World, especially in South America, than in

the Old World. Although some areas significantly predict female reproductive success, male contribution to diet remains the best predictor of female reproductive success throughout the world, even after controlling for area. The surest way to increase female reproductive success is to increase TFR, and that is what greater male contribution to diet appears to do.

DISCUSSION

Human mortality in the first year is the same as that of chimpanzees. However, by age 5 chimpanzee mortality is 40% (Hill et al. 2001), considerably higher than among human foragers. This suggests that there is little extra that forager women can do to reduce mortality in the first year but that provisioning by men may help around the time of weaning. When there is little else females can do to reduce offspring mortality, they should use energy coming from males to increase fertility. We can even expect females to accept some increase in mortality if that is the price of sufficiently increased fertility to yield the highest reproductive success in the end. They might, for example, prefer that males forage rather than provide direct care even if direct care could help reduce infant mortality.

Without help from males, females might get enough children weaned to reproduce themselves but take longer and lose out to females getting help. The regression line in figure 4 shows that reproductive success is expected to be about 2.2 when male contribution to diet is 20%, whereas the mean reproductive success for all foragers is 3.11. Even with low rates of mortality, 2.2 children reaching age 15 could simply be too few. But a male contribution to diet of only 20% would still be a gain for females unless males ate more of the food in camp than the 20% they brought in. There is no reason male contribution to diet must be over 50% to represent a real benefit to females; what matters is the amount of food over and above personal consumption that males contribute.

What explains the variation in male contribution to diet? As noted, it is greatest in cold climates, where there is little that females can gather. The higher level of male contribution in the New World might be due to cultural inertia, if migrants through the Arctic maintained high levels of male contribution even after moving into other habitats, or to high mortality due to diseases in the New World to which humans had less resistance, with high mortality requiring high TFR and high male contribution. Differences in types of foods available may be important. Females may contribute a greater percentage of the diet in low-biomass habitats because in such places tubers, which are usually acquired by women, are a more important source of food. In high-biomass habitats, females may need to provide more direct care to prevent higher nursling mortality, and if this caregiving interferes with their foraging (see Hurtado et al. 1992, Marlowe n.d.) males may have to provide more food. Alternatively, female preference for providers may everywhere push male contribution up to the highest level males can afford, given the opportunity costs.

One opportunity cost of paternal provisioning is the pursuit of mates. Since male contribution to diet appears to increase fertility rather than reduce mortality, one could argue that it reflects mating rather than parenting effort. If so, is it mating effort directed toward one's own wife or toward all females? Men should pursue a foraging strategy that maximizes their own reproductive success, which means that they may trade off provisioning of wives and children for maximizing mating opportunities through affairs, simultaneous polygyny, or serial monogamy and remarriage of younger wives later in life, a strategy which could be far from optimal for their wives (Marlowe 2000*b*). Prior to effective tools, it would have been difficult for males to acquire much surplus food for provisioning, and what they could acquire would have been, for the most part, the same food that females acquired. With increased foraging efficiency, males may have gained more by targeting foods different from those females acquired since their trade value would have been greater, resulting in a sexual division of labor. However, once males began to bring in high-nutrient foods such as meat and honey, they would have become more vulnerable to scrounging from everyone in camp, diluting the potential benefit going to wife and children.

Since male contribution to diet is only a proxy for actual provisioning, these results do not confirm the paternal-investment theory of pair bonding. It is conceivable that females could obtain the same benefit from male contribution to diet by trading sex for food at every opportunity as they could from bonding and trading paternity confidence for long-term provisioning. These result do, however, show that male contribution to diet has important consequences for forager women—a necessary condition for the paternal-investment theory. Women begin reproducing later than chimpanzee females and have only a slightly longer reproductive span but manage to exceed them in fertility through earlier weaning (Hawkes et al. 1998) and shorter interbirth intervals (Kaplan et al. 2000). This analysis suggests that one way they achieve this is through male provisioning. While the paternal-investment theory has usually emphasized the benefit of reduced offspring mortality, the real benefit of male provisioning among foragers is increased fertility.

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Ethnocentrism and Xenophobia: A Cross-Cultural Study[1]

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People readily though not inevitably develop strong loyalties to their own ethnic group and discriminate against outsiders. In this report I use cross-cultural data to (1) determine the factors that strengthen and weaken these tendencies and (1) ascertain whether they have the same determinants. It is often supposed that ethnocentrism and xenophobia are opposite sides of the same coin, but a few voices have cautioned that this need not be the case.

Van den Berghe (1999) points out that it would be maladaptive for xenophobia to be an inevitable result of ethnocentrism. Ethnic affiliation, he reminds us, usually involves some claim of common ancestry (real or fictive), and a propensity to favor fellow ethnics is no doubt enhanced by this feeling of kinship. But reciprocal relationships with members of other groups can frequently be adaptive also, and it would be foolish to assume an attitude of hostility. The threshold for cooperation may be higher and the insistence on reciprocity may be greater, but a smart opportunist keeps his options open.

Recent experimental work in psychology also suggests that in-group favoritism is not a necessary concomitant of out-group hostility (Rabbie 1982, 1992; Ray and Lovejoy 1986; Struch and Schwartz 1989). While both can be enhanced by competition and external threats (see Sherif 1961 for the classic field experiment), in-group favoritism should be expected only if affiliation with the in-group can successfully counter the competitive threat (Rabbie et al 1974). If a group is unable to be successful, hostility to outsiders may be mirrored by ethnic breakdown and further hostility and competition within the group. Finally, threats can arise from environmental catastrophes as well as from outsiders, and we might expect that such disasters would foster enhanced group loyalty without any concomitant hostility to outsiders.

The cross-cultural data analyzed here provide no support for the proposition that out-group hostility is a nec-

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essary concomitant of in-group loyalty. The threat to the group that arises from catastrophic food shortage enhances ethnic loyalty without increasing hostility to outside groups, and even when the threat arises from other groups (external warfare), the associated ethnocentrism and xenophobia seem to have different causes. Overall, ethnocentrism and xenophobia were uncorrelated in this dataset, with the latter being most strongly associated with the overall level of violence within as well as between ethnic groups.

METHODS

The study uses published codes and data collected for the standard cross-cultural sample of 186 societies (Murdock and White 1969). The sample was selected to maximize geographic and linguistic independence. The phylogenetic methods advocated by Mace and Pagel (1994) to ensure independence were not used here both for theoretical reasons suggested elsewhere (Rogers and Cashdan 1997) and because the intensity of intraethnic loyalty and the intensity of interethnic hostility are highly labile traits and therefore unlikely to be affected by distant historical connections between societies. Each society in the sample is pinpointed to a specific place and time; for most societies, that time is the early to middle 20th century.

Ross (1983) and Lang (1995) have independently used somewhat different subsets of the standard sample to code both ethnic loyalty and out-group hostility. Ross defines the former as "in-group loyalty, or we feeling, directed towards the wide society" (i.e., in contrast to the local community, which he coded separately). Lang's definition refers specifically to "loyalty within the ethnic group," loyalty being defined as "consciousness of belonging together." Both measures of out-group hostility refer to attitudes rather than behaviors: Ross's measure "seeks to evaluate the feelings towards other societies" and Lang's definition specifies "negative attitudes and emotions, contempt, mistrust." As with the loyalty measures, Ross specifies hostility to "other societies" while Lang specifies hostility to "other ethnic groups." I have sometimes reversed the order of Ross's ratings in order to make them consistent with those of Lang and the other researchers cited. In all cases reported here, larger numbers indicate more of a variable (greater loyalty, more frequent warfare, more severe famine, etc.). The measures of Ross and Lang are significantly, although not strongly, correlated with each other.

Two types of threat are considered here: famine, coded independently by Ember and Ember (1992*b*) and Dirks (1993), and external warfare, coded independently by Ember and Ember (1992*b*), Ross (1983), and Lang (1995). As with the loyalty and hostility variables, consistent results from independently derived data allow greater confidence in the conclusions. These and other variables used in the analysis are fully defined in the appendix. All data are available not only in the cited sources but in the electronic journal *World Cultures*.

Nonparametric statistics (Spearman's correlation coefficients) are used throughout because the data are ordinal, with most variables taking only four values. Although I have predicted the direction of effects, all significance tests reported below are two-tailed.

TABLE 1
Intraethnic Loyalty and Interethnic Warfare

	Loyalty (Ross)			Loyalty (Lang)		
Measure	r_s	p	n	r_s	p	n
All societies						
External warfare (R)	.21	.07	74	.30	.04	45
External warfare (E)	.44	.0006	59	.28	.02	63
External warfare (L)	.13	n.s.	35	.11	n.s.	61
Interethnic violence (L)	.44	.003	43	.24	.05	72
Unpacified societies only						
External warfare (R)	.45	.001	47	.45	.01	30
External warfare (E)	.60	<.0001	37	.32	.04	42
External warfare (L)	.33	n.s.	21	.35	.02	41
Interethnic violence (L)	.63	.0002	30	.37	.008	50

NOTE: See text and appendix for variable definitions.
SOURCES: R, Ross (1983); E, Ember and Ember (1992*b*); L, Lang (1995).

INTRAETHNIC LOYALTY

Threats and competition from outside groups are often cited as an important force in fostering ethnic loyalty (Levine and Campbell 1972, Roosens 1989, van der Dennen 1987, Durham 1994). In order to explore this proposition cross-culturally, I correlated frequency of external warfare, as measured by Ross (1983), Lang (1995), and Ember and Ember (1992*b*), with ethnic loyalty as measured by Ross (1983) and Lang (1995). As table 1 shows, this proposition receives support from the codings of Ross and Ember and Ember. The absence of patterning with Lang's external-warfare variable probably derives from differences in the way this variable was coded. Lang's definition of external warfare differs in applying only to societies in which formal political offices are present. His measure of interethnic violence ("frequency of interethnic violence/attacking") is applicable to all societies and shows patterning similar in strength and direction to the external-warfare variables of Ross and Ember and Ember. The absence of association with Lang's measure of warfare suggests that external warfare promotes ethnic loyalty more strongly in egalitarian societies.

Ember and Ember, noting that some societies lack warfare only because they have been pacified, omitted such societies from their sample when they analyzed the determinants of warfare, since pacified societies might still have conditions that predisposed to it (1992*a*, *b*). I used their measure of pacification for an analogous reason and found that the relationship between external warfare and ethnic loyalty was much stronger when only unpacified

TABLE 2
Intraethnic Loyalty and Risk of Famine

	Loyalty (Ross)			Loyalty (Lang)		
Measure	r_s	p	n	r_s	p	n
Routine food shortage						
Chronic resource problems (E)	.07	n.s.	54	.05	n.s.	55
Ordinary nutrition (D)	−.13	n.s.	52	.11	n.s.	53
Short-term starvation (D)	.07	n.s.	74	.08	n.s.	80
Seasonal starvation (D)	−.02	n.s.	73	.03	n.s.	80
Catastrophic food shortage						
Threat of famine (E)	.39	.008	44	.23	.11	48
Severity of famine (D)	.19	n.s.	51	.17	n.s.	54
Persistence of famine (D)	.37	.009	49	.28	.04	56
Recurrence of famine (D)	.30	.02	59	.17	n.s.	67
Catastrophic food shortage, unpacified societies only						
Threat of famine (E)	.58	.0009	29	.42	.02	32
Severity of famine (D)	.41	.02	33	.24	n.s.	36
Persistence of famine (D)	.63	.0002	30	.39	.02	38
Recurrence of famine (D)	.42	.007	39	.19	n.s.	46

NOTE: See appendix for variable definitions.
SOURCES: E, Ember and Ember (1992*b*); D, Dirks (1993).

societies were considered. Pacification need not mean the end of interethnic competition—indeed, colonialism has often exacerbated it (Gulliver 1969, Arens 1978, Roosens 1989)—hence we might expect that absence of warfare due to pacification would typically not remove the competitive pressures that lead to strengthened ethnic loyalty. In order words, warfare in pacified societies was suppressed but the competition that fostered ethnic loyalty typically was not. If this argument is correct, including pacified societies would weaken the correlation between frequency of warfare and ethnic loyalty, as it does here (see table 1).

Threats to the group need not come from outsiders. In order to see whether environmentally induced hardships also promote group loyalty, I correlated loyalty with various measures of food stress and famine. As table 2 shows, routine food shortage (mild, chronic, or seasonal) has no effect on ethnic loyalty whereas real famine (severe and socially disruptive food shortage) has a moderate but statically significant positive effect. The strongest correlations are with threat of famine as measured by Ember and Ember and persistence of famine as measured by Dirks. Threat of famine measures the likelihood of its occurrence and is chiefly a measure of frequency. Persistence of famine assesses specifically how often living members of the society have experienced famine (see appendix for complete definitions). Famine, by Dirks's definition, is never routine, but a society that has some experience of it in its cultural memory might be more likely to respond in productive, culturally mediated ways. A society facing famine with no history to guide it might be more susceptible to societal chaos and the breakdown of mutual support.

Table 2 also shows that the correlation between famine and ethnic loyalty is stronger when the sample is limited to unpacified societies. This result was unanticipated, but the explanation may lie in the greater infrastructure and organizational complexity of the "pacifying" society, together with the economic dependency such a situation often imposes. Any society able to pacify another is likely to be better able to buffer food shortages through storage and trade, so perhaps people in pacified societies respond to famine by relying on the dominant society rather than by bonding together to help themselves.

I explored some likely antecedents of warfare and famine to see whether these variables were also correlated with ethnic loyalty but found no relationships. Variables I considered included measures of density pressure (population density, land availability, agricultural intensification), Ember's measure of natural disasters (a correlate of warfare), and various climatological measures of harshness and unpredictability (a possible cause of famine). Variable and unpredictable climates have a strong effect on the spatial extent of ethnic groups (Cashdan 1991), but I found no consistent climatic associations with ethnic loyalty or interethnic hostility.

INTERETHNIC HOSTILITY

If interethnic hostility is the flip side of intraethnic loyalty, the two should be strongly correlated and have the same determinants. Neither is the case.

Both external warfare and famine are associated with ethnic loyalty. It is reasonable to expect external warfare to be associated with interethnic hostility, and table 3 shows that this is indeed the case. But interethnic hostility is also associated with *internal* warfare (warfare between communities of the same society or ethnic group); the associations are in the same positive direction

and of similar magnitude. The same is true of Lang's measures of intra- and interethnic violence (see table 3), and Ross's measures of local and intercommunity conflict show trends in the same direction. Taken together, these data suggest that hostility to outsiders is not simply a direct response to external threat but is likely to reflect the prevailing level of violence in the region.

Levine and Campbell (1972:213–14) note that while most theories of ethnicity predict an inverse relationship between in-group loyalty and out-group hostility (albeit for different reasons), other theories predict a continuity in the violence experienced at different levels of grouping. These data suggest there is a continuity of violence at local (intraethnic) and regional (interethnic) levels rather than the discontinuity that would result if in-group loyalty were reflected in out-group hostility.

Famine, the other threat considered here, is correlated with ethnic loyalty but not with interethnic hostility. The lack of correlation may reflect the complexity of these relationships, as illustrated by Levine and Campbell's account of catastrophic food shortages in Kenya. They argue that while destruction of cattle by rinderpest exacerbated out-group hostility, famines due to grain crop failures in the same area "were traditionally times of formal peacemaking, increased trade, sharing across ethnic group lines, and the peaceable transfer of children and women from the group with most famine to others more fortunate, in exchange for grain" (Levine and Campbell 1972:36).

Rabbie (1982, 1992) has shown experimentally that intragroup cooperation can foster an in-group bias without necessarily increasing the level of hostility between groups. This finding is supported in this dataset by the relationship between crosscutting ties within a society (data from Ross) and ethnic loyalty as measured by Ross ($r_s = .62$, $p < .0001$, $n = 77$) and Lang ($r_s = .40$, $p = .005$, $n = 47$) and the absence of any such correlation with hostility. Extensive crosscutting ties (presumably related to the level of intragroup cooperation) foster ethnic loyalty but are unrelated to interethnic hostility.

TABLE 3
Hostility to Other Societies, Violence, and Warfare

	Hostility (Ross)			Hostility (Lang)		
Measure	r_s	p	n	r_s	p	n
Interethnic fighting						
External warfare (R)	.72	<.0001	68	.34	.03	42
External warfare (E)	.32	.02	53	.17	n.s.	56
External warfare (L)	.34	.07	30	.37	.008	52
Interethnic violence (L)	.34	.03	39	.19	n.s.	64
Intraethnic fighting						
Internal warfare (R)	.47	<.0001	69	.12	n.s.	43
Internal warfare (E)	.48	.0005	49	.29	.04	50
Intraethnic violence (L)	.40	.007	45	.21	.09	68
Intercommunity conflict (R)	.37	.002	69	.07	n.s.	40
Local conflict (R)	.22	.06	69	.24	n.s.	40

NOTE: See appendix for variable definitions.
SOURCE: R, Ross (1983); L, Lang (1995); E, Ember and Ember (1992*b*).

TABLE 4
Intraethnic Loyalty and Interethnic Hostility

Correlates	r_s	p	n
Loyalty (Lang) × loyalty (Ross)	.54	.0001	44
Hostility (Lang) × hostility (Ross)	.37	.03	36
Loyalty × hostility (Lang)	−.06	n.s.	64
Loyalty × hostility (Ross)	−.01	n.s.	69
Loyalty (Lang) × hostility (Ross)	.08	n.s.	41
Loyalty (Ross) × hostility (Lang)	−.25	n.s.	39

Since ethnic loyalty and interethnic hostility appear to have different determinants, we might expect them not to be strongly correlated. This is indeed the case. We see in table 4 both the correlation between Ross's and Lang's measures of the same variables and the absence of any relationships between ethnic loyalty and hostility to outsiders in either dataset.

The absence of correlation between ethnic loyalty and hostility to outsiders is encouraging for the prospects of a peaceful multiethnic state and suggests that the flowering of ethnicity is not necessarily something to fear. Most of the societies in this sample, however, were described in the early to middle 20th century. As ethnic groups become increasingly class-based elements in complex societies, the frustration of being have-nots in a wealthy society is always a potential source of violence and hostility. What this study shows is that interethnic hostility is not an integral part of strong ethnic identity and that its source must be sought elsewhere.

APPENDIX: VARIABLE DEFINITIONS AND NOTES

From Dirks (1993):

Famine: "an episode of starvation that is attended by sharply increased mortality rates and marked disruptions in community life. Its duration exceeds short-term starvation. Unlike seasonal starvation it does not occur annually. Unlike short-term and seasonal starvation, famine lacks a routine character. It disrupts society from the start and it can progress to the point of massive institutional collapses" (p. 30). (This distinction parallels the distinction in table 1 between "routine food shortage" and "catastrophic food shortage.")

Endemic starvation: "a condition of chronic undernutrition, unrelated to daily contingencies, season, or the fortunes that affect food availability in any particular year" (p. 30). "Endemic starvation exists when there is evidence that some members of society suffer caloric insufficiency under normal conditions."

Short-term starvation: "an episode of starvation that has a duration of a few days or weeks. . . . [These episodes

are] typically recurrent and familiar. As a result, outbreaks do not excite alarm [and one] usually does not result in death" (p. 30).

Seasonal starvation: "occurs at regular times every year. It may last from several weeks to as long as three or four months. [Increased morbidity and mortality] are not detected readily, and, until recently, not often reported. Like short-term starvation, seasonal starvation is a familiar event. Consequently communities that experience it have a repertoire of customary adjustments by means of which they avoid social disruption" (p. 30).

Severity of famine: "the extent to which a community or some segment of it progresses toward complete institutional breakdown" (p. 31).

Persistence of famine: "the frequency of its occurrence over a relatively short period of time [50 years]—how often a living set of generations has had direct experience with famine" (p. 31).

Recurrence of famine: "its repetition over long periods of time. . . . at least one famine in each of the two immediately preceding centuries" (p. 31).

From Ember and Ember (1992*b*):

Famine: "a time of starvation when either many human deaths occur or it is reported that a substantial segment of the society has to move because of a lack of food . . . [or] the ethnographer uses the word famine" (p. 180). The measure "picks up only extremely serious resource problems" (p. 180) and does not include chronic hunger. It is chiefly a measure of famine frequency.

Chronic resource problems: distinguished from "unpredictable resource problems" (p. 181).

Warfare: "socially organized armed combat between members of different territorial units (communities or aggregates of communities)" (p. 172).

Internal warfare: "socially organized armed combat between territorial units (communities or larger aggregates) *within* the same society. By 'society' we mean a more or less continuously distributed population that speaks a common language" (p. 173).

External warfare: "war between the focal society and other societies" (p. 173).

Pacification: "the elimination of war by an external power *before* the twenty-five-year time period" (p. 175). (I considered societies coded 1 or 2 "unpacified.")

From Lang (1995):

Ethnic group: "group of persons perceiving themselves as unit and set themselves apart from other such units. The unity is based on real or supposed common origin, common fate, common language or relation, adherence to common norms and values" (p. 50).

Loyalty: "consciousness of belonging together . . . the variable measures the degree of loyalty within the ethnic group as a whole. If for instance there are strong feelings of loyalty among a small part of the ethnic group and no loyalty within the group as a whole the code 1 [=low] applies" (p. 50).

Hostility: "negative attitudes and emotions, contempt, mistrust" (p. 51). (Code incorporates both degree of hostility and its targets; I lumped values for the different types of targets so that the scale measured only degree of hostility.)

External warfare: "warfare where at least one party involved is a maximal unit of political authority" (p. 36). (I deleted cases coded 0, "no formal political office present," since this does not discriminate the amount of fighting in such societies.)

Interethnic violence: "frequency of interethnic violence/attacking" (p. 54). (I deleted the few societies with no interethnic contact.)

Intraethnic violence: "intensity of intraethnic violence" (p. 54).

(Internal warfare was not rated for societies with "no political office above the level of the local community," more than half of the codable societies. I did not use these variables for this reason and because of their lack of comparability with the internal-warfare measures of Ross and Ember.)

From Ross (1983):

Loyalty to the wider society: "in-group loyalty, or we feeling, directed towards the wider society" (distinguished from loyalty to the local community, which Ross coded separately) (p. 180).

Hostility toward other societies: "bitter feelings" toward "outsiders" (p. 180).

Internal warfare: warfare "between communities of same society" (p. 179).

External warfare: "with other societies" (p. 179).

Local conflict: political conflict and social conflict more generally at the local community level (p. 177).

Intercommunity conflict: conflict between communities of the same society (p. 178).

Crosscutting ties: "politically relevant" links between individuals living in different communities of the same society (p. 181).

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