Reports

The Afroasiatic Language Phylum: African in Origin, or Asian?[1]

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Greenberg (1971) noted that four of the five members of his Afroasiatic language phylum are located on the African continent and only one mainly in southwestern Asia; therefore he saw the mother language from which they descended as being in northeastern Africa. This conclusion is based on the principle that linguists call "least-moves"; it is logically simpler to accept that one moved out of the continent of Africa than that four moved into it. This is an example of Occam's Razor, which holds that the simpler explanation is the more likely to be true; it doesn't prove that it is true in any particular case.

Least-moves does not mean that the protolanguage was in the center of distribution of its descendant branches. Bantu's geographical point of derivation is on the periphery of the Bantu distribution without contradicting the least-moves principle. A similar diffusion from the periphery might be posited to derive the other Afroasiatic languages from southwestern Asia, but the cases are not comparable: Bantu can be shown to be one branch within a larger family, but the branches of Afroasiatic do not segment in a way that would make that plausible; in fact, they segment in a way that reinforces the hypothesis of a North African origin. If the Semitic-speakers remained in place while the other Afroasiatic-speakers moved across the Suez peninsula, this should have resulted in a bifurcation with Semitic being one branch and all the other segments of Afroasiatic in the other branch, but that is not the case (Voegelin and Voegelin 1977:12).

Among the modifications of Greenberg's original designations of language families is the discovery, by Harold Fleming (1969), that Omotic is a family in itself, separate from Cushitic, to which it had been assigned. More recently, Fleming et al. (1992) have claimed that another language seen as Cushitic, Ongotan, may also be a separate family, though we must await assessment of Ongotan by other historical linguists. Furthermore, Beja is suspected by some linguists (Ruhlen 1991) to be also separate from Cushitic. The five families of Afroasiatic specified by Greenberg now become at least six and possibly will eventually be recognized as eight, still with only one in Asia. The assertion of an African origin for Afroasiatic is therefore stronger than ever.

Acceptance of an African genesis of the phylum is widespread among Africanists (see, e.g., Trigger 1982:488). Munson (1986:79) finds "a possible archaeological correlation" for Proto-Afroasiatic in a microlithic industry, the Cataract tradition, in the Central Nile region ca. 18,000 B.C. Ehret (1995) classifies Semitic along with Egyptian and Berber as a Boreafrasian group younger than Omotic and Cushitic—that is, he argues that Omotic and Cushitic were already separate branches when Semitic emerged, presumably in Africa, from Boreafrasian. Curtin (1995) asserts that "linguists today believe that this language family [Afroasiatic] originated in Africa, probably somewhere in northern Ethiopia or in the Red Sea hills." He points in this connection to the then-forthcoming work of Cavalli-Sforza and his colleagues "pulling together a set of genetic maps based on more than a hundred inheritable genes."

Cavalli-Sforza, Menozzi, and Piazza (1994), however, consider Afroasiatic to have originated in southwestern Asia. They report that the gene frequencies of populations in the northern tier of Africa, where Afroasiatic languages are located, are closer to those of populations on the other side of the Suez isthmus than to those of populations in the rest of Africa, and they attribute this distribution to the movement of food producers from southwestern Asia into Africa, carrying with them Proto-Afroasiatic speech.

How can one relate population genetics to historical linguistics? A "population" in geneticists' terms is a group within which most matings occur. Fertile matings produce in the offspring a mix of the genes of the two parents. Each individual has a very large number of genes, and each person differs somewhat from others, even from siblings, in the combination of genes. Variant forms of genes, called alleles, exist, and new alleles, as well as new genes, are continually coming into existence by mutation, but most of these are eliminated by death of the person with the mutant because of their deleterious effects; some genes are neutral and may survive to be passed on to subsequent generations, and some new genes or alleles are beneficial for adaptation to a particular environment and will flourish among descendants. Thus a *population* is characterized by the frequencies of a large number of specific genes. The difference between the frequencies found in one popula-

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tion and those of another population is called ''genetic distance.'' Small populations of similar composition can be pooled to compare to other clusters.

Three factors are involved in creating genetic distance: migration of peoples, mutation within populations, and natural selection. Migration creates separate populations. From the time of separation, each generation allows the process of acquiring and losing genes to proceed in both of the now separate populations. Each change in either population increases the genetic distance between the two, and, although there are a few factors that may affect these changes, the overall process is essentially regular. This is a simple matter when the movement is into an unoccupied territory but becomes more complicated when the move is into the habitat of another population. Calculation of the time elapsed since the separation of populations is possible but may require hypotheses about gene mixture or language replacement. Mutation rates are critical to this process, and their regularity or lack of it is still debated. At the moment the ''molecular clock'' is not totally reliable.

Cavalli-Sforza and his colleagues construct a general history of genes throughout the world and a number of specific histories of groupings, geographical and/or linguistic, on the several continents. Can the data they provide and the data from linguistics be interpreted to be consistent with a unitary history?

Paolo Francolacchi (1995:395) puts the elements of the problem succinctly:

> the mechanism at the base of the differentiation of languages (diffusion and subsequent isolation) is the same as that which is at work in the evolution of living beings. However, the linguistic transmission is not only vertical (from parents to offspring) as in the case of transmission of genes, but also horizontal (learning from neighbors). A single individual or an entire people can replace a language in a relatively short time, while obviously this cannot be done for genes. This can explain the incongruities when comparing the linguistic affiliation of a population with its genetic pattern. . . . Nevertheless, in most cases, the correlation between the tree drawn from the genetic distances and that based on linguistic families is strong. . . .

According to Cavalli-Sforza, Menozzi, and Piazza (1994:99) ''The one to one correspondence between genetic clusters and linguistic families is remarkably high, but it is not perfect.'' Responding to critics of a preliminary announcement of their results, they had earlier (1989:1128) reported ''6 exceptions to the rule that every linguistic phylum corresponds to one of the major genetic clusters. These are amply justified by linguistic substitution.'' Their 1994 book makes a more general statement (p. 99):

> Gene replacement is more likely to be partial and tends to follow demographic history. Two neighboring peoples may mix by asymmetrical gene flow, with only one of them contributing a small number of individuals to the other in every generation. . . . the continuation of this process over a substantially long time may determine an almost complete gene substitution. In general, the gene pool tends to reflect rather faithfully the numerical contribution from the two parental groups. Thus, genetically intermediate populations can be generated, with all possible degrees of admixture. This process need not be accompanied by language change. Languages tend to behave more like a unit, and be replaced as a whole, if at all. One can, and usually does, notice contributions to the lexicon of neighbors, but the structure of language is more stable, and certain specific groups of words are more highly conserved. In certain cases, therefore, one can observe massive genetic contributions from an external source with little if any language change, and in other cases, language substitutions with little genetic change.

This is relevant to Afroasiatic in that speakers of Chadic languages are genetically part of the Sub-Saharan genetic cluster, Cushitic-speakers tend to be intermediate, ancient Egyptians had a certain amount of mixture, and Berber-speakers and Semitic-speakers are predominantly aligned with the Southwest Asian genetic cluster. Thus, with allowance for genetic mixture, Afroasiatic is a language phylum that—with this explanation—could have an origin in the Southwest Asian genetic cluster. Before we go on, it should be stated that the quantity of genetic data on Afroasiatic-speakers leaves much to be desired (Cavalli-Sforza, Menozzi, and Piazza 1994:169).

There is a prologue to this story of the geographical and temporal origin of Afroasiatic. First, it was found (Ammerman and Cavalli-Sforza 1973) that with the fairly ample genetic data on Europe the present distribution of gene frequencies might be explained as due to the steady expansion of agriculturists from the cradleland of the Neolithic in southwestern Asia; then it was suggested that this movement of food producers might be identical with the spread of Indo-European languages. Since this is the model for the interpretation of Afroasiatic origins, it requires review.

Neither linguists nor archaeologists have been able to achieve a consensus on the location of the cradleland of Indo-European. J. P. Mallory's survey (1989:144) of Indo-European problems has a map showing the discordance of the choices of various scholars, and only 2 of the 13 displayed point to Anatolia. One of them, the archaeologist Colin Renfrew (1987), argues that only the Neolithic sites in Europe offer an adequate archaeological correlation with the spread of Indo-European. Other archaeologists hold otherwise, and there has been no rush to adopt his position. Marija Gimbutas was unconvinced by Renfrew's argument, and Mallory (p. 164) dismisses it. The linguist Merritt Ruhlen (1994:180) refers to studies by Sturtevant and Hahn (1951 [1933]) and by Aron Dolgopolsky, both concluding that Anatolia was the Indo-European homeland—the former because of the recognition of Hittite as Indo-European (he took it to be coordinate with all the rest of Indo-European, a position that has found little acceptance) and the latter because of the presence of some Semitic loanwords (which require some kind of contact) in early Indo-European.

David Anthony (1991) marshals linguistic and archaeological evidence and presents an extensive survey of the literature that matches vocabulary with excavated artifacts from relevant sites to conclude that a "relatively intact" Proto-Indo-European language community occupied the Pontic steppes until at least 3500 B.C. but began to break into separate languages ca. 2400–2200 B.C. His article may well be the coup de grace for the Anatolian hypothesis. The correlation of genetic distances in Europe with the diffusion of agriculture is probably correct, but the addition of Indo-European to the proposition is dubious.

Without a comparable amount of data, it was postulated subsequently that from the same general region of southwestern Asia Proto-Afroasiatic-speakers migrated across the Suez isthmus, leaving behind some who became ancestors of the Semites, and the Dravidian languages spread into Iran and India. The map in Cavalli-Sforza and Cavalli-Sforza (1995:160) has a trefoil arrangement of Neolithic areas: the Levant coast for Proto-Afroasiatic, ancient Anatolia for Proto-Indo-European, and Mesopotamia for Proto-Dravidian. The caption under this map reads "Possible languages spoken by Neolithic cultivators who expanded from the Middle East, independently proposed by L. Luca Cavalli-Sforza and Colin Renfrew."

Ruhlen (1994:188) considers Renfrew's position "archeologically based" and sees "the Renfrew–Dolgopolsky–Cavalli-Sforza model" as signifying convergence of their three disciplines. This is a significant assertion: according to Ruhlen, we have three independent sets of data, each independently read by different scholars in different disciplines. But is the Renfrew position really independent of Cavalli-Sforza's? Renfrew (1987:xiii) states that it was in 1973 that his ideas began to take shape. That was the year he edited Ammerman and Cavalli-Sforza's article. He merely mentions that article, relying on models of social change and skepticism about migration as an explanation in prehistoric archaeology. He even seems to distance himself from the genetic argument (p. 158):

> I think experience has shown that genetic arguments in relation to language and culture quite readily lend themselves to misleading interpretations. So that although the blood group data could indeed be used to reinforce the case presented here, I feel it would be wiser to await further assessment of the arguments relating to these data, which have not yet been thoroughly reviewed.

What we have here, it seems, is finding a way to a preselected goal by an original argument; Renfrew appears to be reaching for independence whereas on examination it looks like stimulus diffusion—the idea was adopted and adapted.

Renfrew (personal communication, 1996) holds that he used "the model of demic diffusion previously developed by Ammerman and Cavalli-Sforza, but not given linguistic force by them." Borrowing the demic-diffusion model may call into question Ruhlen's characterization of Renfrew's position as "archaeologically based," and the degree of "linguistic force" is still under discussion by linguists. Robert Sokal, a biostatistician, is quoted in *Science News* (June 24, 1995) as reporting of his study of the geographic patterns in a number of related words, "At this point, I can't substantiate any hypothesis of Indo-European origins."

The three-pronged Neolithic expansion concept is elegant, but the evidence for the second stage of its construction, the addition of Dravidian and Afroasiatic, is very sparse, while the original equation of Indo-European and Neolithic expansion is still disputed. There is a caveat: Peoples outside the Near Eastern Neolithic area may have adopted farming, and if they maintained their own languages, that would upset Cavalli-Sforza et al.'s neat formula. We may suspect that this happened in the case of Afroasiatic: the bottleneck of Suez gave some protection to the people already in northeastern Africa from the "wave of advance" of Southwest Asian farmers. If the Africans took over the farming economy and spread it farther into Africa, there would be some cultural continuities from the Southwest Asian Neolithic but no continuities in language. The spread of languages in connection with farming is in all cases a hypothesis to be weighed by whatever evidence can be found.

Inasmuch as there is continuing disagreement among scholars in the historical disciplines, archaeology, linguistics, and genetics on the point of origin of Indo-European, the best-known language phylum, the problem of the much less investigated Afroasiatic phylum cannot currently be settled to the satisfaction of a majority of concerned scholars.

Interpretation of currently available genetic data does stimulate rethinking and is to be welcomed, but the genetic argument for a Levant origin of Afroasiatic needs a more substantial data base. And as with any data, the problem may be in the interpreting. The model of the genetic picture of Europe is one case; Afroasiatic and Dravidian are two others. Dravidian is problematic because Sumerian, currently considered a language isolate by most linguists, occupied the suggested area of departure.

Renfrew (1992) adds a fourth wave radiating out from the Southwest Asian Neolithic, this one into Central Asia and correlating with the Altaic language family. Barbujani et al. (1994) have assessed the three-wave and four-wave hypotheses by statistical weighing of genetic data for the relevant populations and concluded that the Eurasian waves appear to be substantiated as demic diffusion. However, they add (p. 152): "The status of Afroasiatic-speaking populations will need further studies to be defined, but linguistic and genetic evidence agree in suggesting that their evolutionary history might have been different." The Afroasiatic populations are considered "one of the two main biological objections to the NDD [Neolithic demic diffusion] model" (p. 151). Renfrew, one of the four authors of the article, has since said (personal communication, 1996) that the relation of "farming and Afro-Asiatic is only a hypothesis, and I don't imagine anyone is claiming more than that at present." The "further studies" called for by Barbujani et al. may "define" the position of Afroasiatic, but fitting it to the demic-diffusion model seems unlikely.

Christopher Ehret (1995) offers a reconstruction of roots of words in Proto-Afroasiatic. In his index of reconstructed root meanings, we find a few that might be considered as possibly related to food production. Out of several hundreds of reconstructed roots, there are only a dozen that might be suggestive of Neolithic activities (e.g., "seed," which may have been gathered instead of planted) and none that are definitive. It is significant that no unequivocal root in Proto-Afroasiatic (as distinct from Boreafrasian) for food-producing activities or domesticates was found: this may be read as indicating that Proto-Afroasiatic was pre-Neolithic. (Note: The Neolithic demic-diffusion model might apply to Boreafrasian.)

The difficult question of the ages of phyla bears on the relation of Proto-Indo-European and Proto-Afroasiatic. They are assumed in Cavalli-Sforza's and Renfrew's interpretations to be coeval, and some linguists of the Nostratic school agree, but there is a contrary position. Greenberg's current research is not compatible with a contemporary origin of Afroasiatic, Indo-European, and Dravidian in the Southwest Asian Neolithic. His forthcoming *Indo-European and Its Closest Relations,* according to preliminary reports, has Indo-European and its closest linguistic relatives in a phylum he calls "Eurasiatic." He considers this northern Eurasian phylum younger than Afroasiatic but possibly related at a deeper level. Greenberg's classification contrasts on this point with that of Nostraticists, who lump Afroasiatic and Dravidian along with Indo-European and some other language families in a macrophylum. Incidentally, Barbujani et al.'s conclusion is more congruent with Greenberg's Eurasiatic than with Nostratic. However, some Nostraticists now accept a Southern Nostratic made up of Afroasiatic, Dravidian, and Kartvalian (in the Caucasus) and a Northern Nostratic that is essentially Greenberg's Eurasiatic though lacking some of its constituent languages. Cavalli-Sforza et al. (1994) measure their own data against the two proposed phyla, Nostratic and Eurasiatic, finding support for deeper clustering biologically but without rating one linguistic hypothesis better than the other.

Is there really a contradiction between the linguistic rule of "least moves" and the closeness of the Northeast African genetic cluster to that of southwestern Asia? Perhaps not—if the genetic distances were already distributed in Africa essentially as they are today *prior* to the expansion of Afroasiatic. The major split in world populations is between Sub-Saharan populations and the rest of the world, reflecting the African origin of humanity and the diaspora out of that continent at least 100,000 years ago that began the process of separation ultimately reflected in the present genetic distances among populations (see Cavalli-Sforza and Cavalli-Sforza 1995: chap. 3). It is possible to see the African Late Stone Age distribution of physical types seen in the fossil record as due to a Late Paleolithic movement across Suez into Africa. David Phillipson (1985) points to the importance of the Dabba culture site at Haua Fteah, Cyrenaica, because of its relation to contemporary industries in western Asia and Europe: "the closest connexions of this phase of the Haua Fteah sequence are with the Levant." The tools in question have been radiocarbon-dated to 32,000–38,000 B.P. The movement of an Asian population into North Africa 20,000 years or more before the beginning of food production could, perhaps, account for the genetic closeness found by Cavalli-Sforza et al. but would be *prior* to the Neolithic, which they propose as the driving force for the expansion of Afroasiatic.

Butzer (1964:295) found that there was a movement of Palearctic (i.e., Eurasian) fauna and flora into northern Africa during the Würm glacial regression (i.e., the late Pleistocene); it is plausible that some hunters in the Levant followed them. A serious difficulty, however, is that the Haua Fteah tools are not associated with any skeletal remains, so it is not certain that their makers were of the same physical stock as the makers of the similar industry in the Levant. Thus we are not sure that we do not have a case of cultural diffusion. Yet the dating suggests that certain contemporary events may be associated:

1. *Homo sapiens neanderthalensis* became extinct 40,000 years ago, more or less (in some regions earlier than others). Neanderthals are associated with Mousterian tools; a Mousterian type of tool underlies the Haua Fteah tools mentioned above. The intrusive tools from the Levant are of a general type to which the term "Aurignacian" is applied, a type associated in numerous instances with *H. sapiens sapiens.*

2. This period 40,000 years ago also saw the spread of anatomically modern humans into Europe and more widely in Asia, pushing Neanderthals into extinction, and it is plausible that the Aurignacian stone tools carried by this ancient "population explosion" came into North Africa as one of the waves of migration taking place at that time.

And in the Kom Ombo region of the Upper Nile Valley there are Late Paleolithic sites that have also been identified as similar to those of the Levant (Smith 1966). What relations there may have been between Haua Fteah, Kom Ombo, and the pre-Neolithic culture posited by Munson as probably Proto-Afroasiatic-speaking is not clear.

The problem centers, now, on chronology: without precise dating it is difficult to specify whether one event occurred before another. What is lacking is chronometric methods in which we can have full confidence; neither population genetics nor historical linguistics has such methods. The geneticists' "molecular clock" is incapable of pinpointing a population movement or, to use Cavalli-Sforza's term, a diaspora in time. Linguists have argued about glottochronology, an attempt to measure language change per millennium that has had only limited success (in fact, many refuse to credit it at all). Dating the diaspora that created today's Afroasiatic-speaking population is crucial.

In asserting that the Northeast African bearers of a genetic makeup similar to that of Southwest Asians were Neolithic food producers intrusive as Proto-Afroasiatic-speakers, Cavalli-Sforza et al. (1994) picked a date that could not be earlier than ca. 10,000 years ago and was probably a bit less than that. A population much older than the Northeast African Neolithic ought to read older on the "molecular clock" than the dating assigned it by Cavalli-Sforza et al. (1994). The genetic calculation in this instance was not a simple one because of the extent of gene replacement (noted by Cavalli-Sforza et al.) in two branches of Afroasiatic, and given the extinction of the Egyptian language only Berber- and Semitic-speakers were compared. Also, a number of historically known incursions of groups from other gene pools into North Africa and southwestern Asia affected these two Afroasiatic branches. Thus, the case presents more than ordinary difficulties in making the calculation. Determining the age of Proto-Afroasiatic would require fully adequate samples of gene frequencies from all Afroasiatic populations that do not now exist. Also absent is adequate fossil evidence. Perception of events remains blurred. Without genetic dating in which we can have full confidence, it is difficult to demonstrate whether Afroasiatic began to expand prior to the Neolithic or subsequently, but Ehret's reconstruction casts doubt on a Neolithic dating.

Afroasiatic could have originated in Africa in accordance with the linguistic principle and still have a genetic affiliation with Southwest Asian populations. This does not dispute the calculations of genetic distances from existing samples; it merely questions the adequacy of the samples and the interpretation drawn from them. Bringing Aurignacian into the picture may point to a possible resolution of the problem. Archaeology may help to provide an answer; more discoveries of Aurignacian tools, especially associated with skeletal remains in Northeast Africa, would be desirable, as would genetic studies designed specifically to test the question. In other contexts Cavalli-Sforza et al. specify other factors besides food production that contributed to sustained population growth. Why not then see the rapid spread of Aurignacian tools as the moment of arrival of foragers with a Southwest Asian genetic makeup?

The new challenge of geneticists to the existing formulations is intellectually stimulating, and in time the various types of data will join in sustaining a coherent interpretation of the whole, but that moment is not yet here. Until the doubts raised above regarding Cavalli-Sforza et al.'s interpretation of their data on Afroasiatic are either substantiated or vitiated, it may be prudent to suspend acceptance of their hypothesis of a Levant Neolithic origin for the phylum. My prediction is that Africa will turn out to be the cradle of Afroasiatic, though the speakers of Proto-Asiatic were a reflux population from Southwest Asia.

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Intensive Mining of Specular Hematite in the Kalahari ca. A.D. 800–1000[1]

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Mining is a topic of long-standing fascination in archaeology (Griffin 1961, Jovanovic 1979, Torrence 1986, Knapp and Pigott 1997). Research on mining has uncovered comparative evidence bearing on the ingenuity and effort that people have expended in pursuit of materials deemed valuable and has increased our understanding of the development of exchange networks and the relationships between ethnic groups. Sub-Saharan Africa has, however, seen few systematic case studies of ancient mining (van der Merwe 1980, Friede and Steel 1976, Wenner and van der Merwe 1987, Huffman et al. 1995). Our study of ancient specularite mining in the Tsodilo Hills of Botswana is of special interest because it deals with a self-contained area with diverse habitation sites that can be related to the mines (fig. 1). In addition, our research on mining offers an opportunity to examine relationships between hunter-foragers and iron-using peoples in the interior of southern Africa during a critical time period. The mining activities at Tsodilo, which may have involved hunter-foragers over 1,000 years ago, indirectly relate to a significant debate in anthropology centering on the degree of isolation and autonomy of hunting-and-foraging peoples of which the Kalahari has been the centerpiece (see, for example, Sadr 1997, Lee 1992, Solway and Lee 1990, Wilmsen and Denbow 1990).

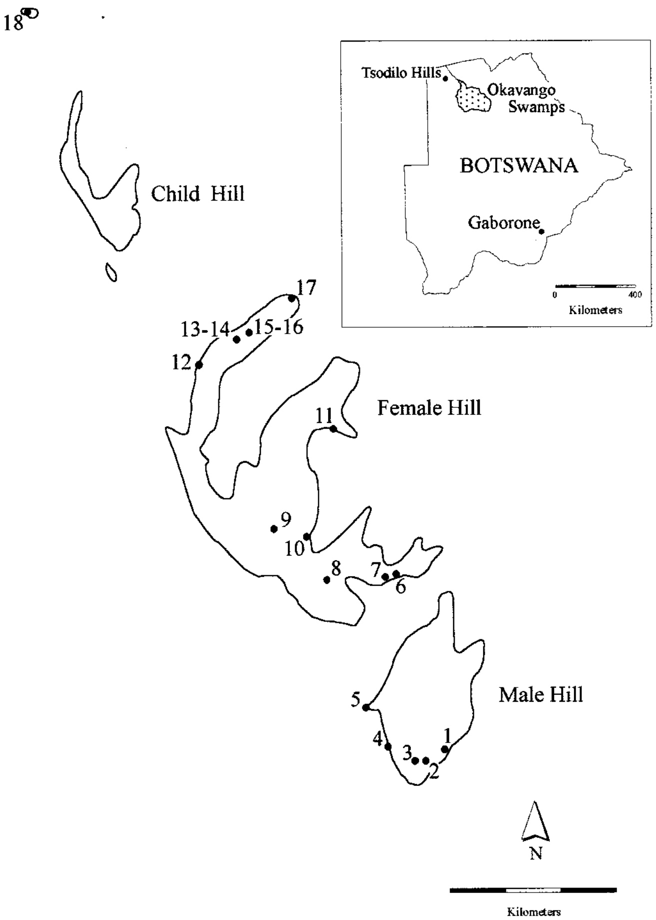

FIG. 1. *Locality map.* 1, *Greenstone Mine;* 2, *Upper Male Hill Cave;* 3, *Upper Male Hill Mine;* 4, *Male Hill Mine A;* 5, *White Paintings Shelter;* 6, *Lower Elephant Mine;* 7, *Upper Elephant Mine;* 8, *Nqoma Village;* 9, *Divuyu Village;* 10, *Depression Shelter;* 11, *Glittering Hands Mine;* 12, *Water Hole Mine;* 13, *Rhino Cave;* 14, *Big Mine;* 15, *Below Procreation Rock Mine;* 16, *Main's Mine;* 17, *Kudu Horn Mine;* 18, *North Hill Mine.*

THE SETTING

At Tsodilo there are over 4,000 rock paintings, several deep rock shelters and caves containing Late and Middle Stone Age deposits, and two major villages of iron-using agropastoralists (Campbell, Denbow, and Wilmsen 1994; Denbow and Wilmsen 1986; Robbins 1990; Robbins et al. 1994, 1996).[2] Hills are relatively rare in

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[2] In this paper the term "Stone Age" is used to refer to the period that predates evidence of the use of iron, ceramics, and food production in southern Africa. For the period of the mining, when sites with lithic technology and evidence of foraging are contemporaneous with sites containing substantial amounts of food production as well as the use of iron, the terms Late Stone Age and Early Iron Age will not be used. See Huffman (1980), Hall (1983), Sinclair, Shaw, and Andah (1983), Kinahan (1991), and Sadr (1997) for discussion of interpretive issues related to nomenclature during the later prehistory of Africa.

the Kalahari and are widely scattered. The Tsodilo Hills rise sharply out of the low, rolling tree-covered dunes. There are four main hills, three of which are known locally as the "Male," the "Female," and the "Child"; the smallest, northern hill has no name. Male Hill, a single huge inselberg, rises to a height of 410 m above the surrounding desert. Female Hill rises to more than 300 m above the surrounding surface. Child Hill is a low V-shaped ridge, and the small northern hill is only about 40 m high. The hills are composed of Upper Proterozoic quartzites and schists that are variously metamorphosed. There appears to be an older banding which may represent original bedding in the now-metamorphosed sediment. The bands, which are rarely more than a few millimeters thick, are rich in specular hematite and sometimes rutile. The schists are largely quartz-mica or muscovite.

THE MINES

Since 1991, at least 20 mines have been located at Tsodilo, not including numerous small-scale initial efforts at mining, resembling potholes, that represent either exploratory work or the exploitation of limited quantities of the desirable material that were soon exhausted (Robbins 1996). It is clear from analysis of samples taken directly from the veins exposed in the Tsodilo mines that varieties of specular hematite, or specularite, were the principal materials that the miners were seeking. However, other materials, such as mica and various crystals, were most likely also obtained. In most mines the tailings included quartzite with hematite-rich bands, quartz-mica schist, and muscovite schist. One mine contained a relatively rare green muscovite that is often associated with the occurrence of gold. Another contained substantial kyanite veins with bladelike crystals up to about 3.0 cm long. In a third mine there was a massive crystalline quartz vein containing blue-gray metallic hematite crystals up to 2 cm long.

Most of the mines resemble natural caves, but they have generally been hollowed from the quartz-mica schist or muscovite schist bedrock by the miners. The tailings frequently found adjacent to the entrances of these cavelike features, the veins of specularite leading from the exterior bedrock into the entrances, and the evidence that miners followed the veins are compelling signs of human activity, as are the numerous battered hammerstones found in the mines. Many of the mines are approximately horizontal where the specularite veins are also horizontal. When these veins dip, the mine adits may be inclined up to 35°, following the veins. While some mines resemble shallow caves or shelters with large mouths, entrance areas tend to be small, and the interior chambers in the largest areas are often dome-shaped. Because of rockfall and because the miners pushed tailings from the working face into previously exploited areas, many of the adits within the larger mines are mere crawlways. However, in such mines there are also areas where there is adequate space to stand, or nearly stand. No vertical shafts or evidence for the use of timbers for shoring have been found. In the mine we called Big Mine, a stack of slabs with a large hammerstone resting on top of them was found at the end of a large cairnlike pile of rocks the purpose of which was not apparent. No other traces of artificial structures were found in any of the mines. The mine floors usually contain numerous rocks and boulders overlying 20 cm–1 m of sand, schist fragments, and rocks. The rock debris removed by the miners was generally pushed aside into previously exploited areas or thrown down the hill at the entrance of the mine; if the nearby terrain was comparatively level, a low mound of tailings was formed just outside of the entrance.

Mining involved spalling the rock by setting fires; the evidence for this includes the dark carbon layer on ceilings and walls (fig. 2) and the large pieces of charcoal found in the mine fill in the floors of most mines.[3] Miners used hammerstones and stone wedges to break up the rocks. These heavily battered artifacts were found inside the mines, on the floors and in test pits excavated in the fill. They were also recovered in excavations conducted in mine tailings. Since the Tsodilo bedrock is generally quite hard, the mining effort centered on the use of these tools in combination with fire is quite remarkable. Loose blocks on the ceilings of the mines and the many boulder-sized rocks that have fallen to the floor bear witness to the fact that mining was a dangerous activity. Areas of ceiling collapse can be readily observed in several of the largest mines. Indeed, our work, sometimes conducted in dark, cramped, and precarious conditions, gave us a firsthand appreciation for what the miners accomplished with relatively unsophisticated technology.

Overall, the mines are quite variable in size, ranging from about 4.5 m in length to as much as an estimated 43 m; width ranges from about 2 to 18 m, while maximum ceiling heights vary from approximately 1 to 2.5 m. Two of the mines merit further discussion both because they are large and because they have multiple entrances. Big Mine, situated above a valley containing at least three other mines near the north end of Female Hill, has two entrances and two corridors, or adits, about 43 m in length. A conservative estimate based on the overall dimensions of the mine converted to volume (2.7 g per cc) is that at least 500 tons of rock were removed from this mine. Its northwest corner was completely filled with rubble excavated from the adits. A high-water mark is apparent as a black stain near the ceiling just inside the lower entrance; the lower part of the mine was flooded by rain water flowing down the slope from the upper entrance, creating a cistern that was about 1 m deep. We suspect that the two entrances of the mine were initiated separately and the connecting of the adits allowed the water to flow to the lower area. Indeed, flooding may well have contributed to the abandonment of this mine. Our work (see Brook et al.

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[3] Thus far, one species of tree has been identified from the charcoal, *Acacia nigrescens.*

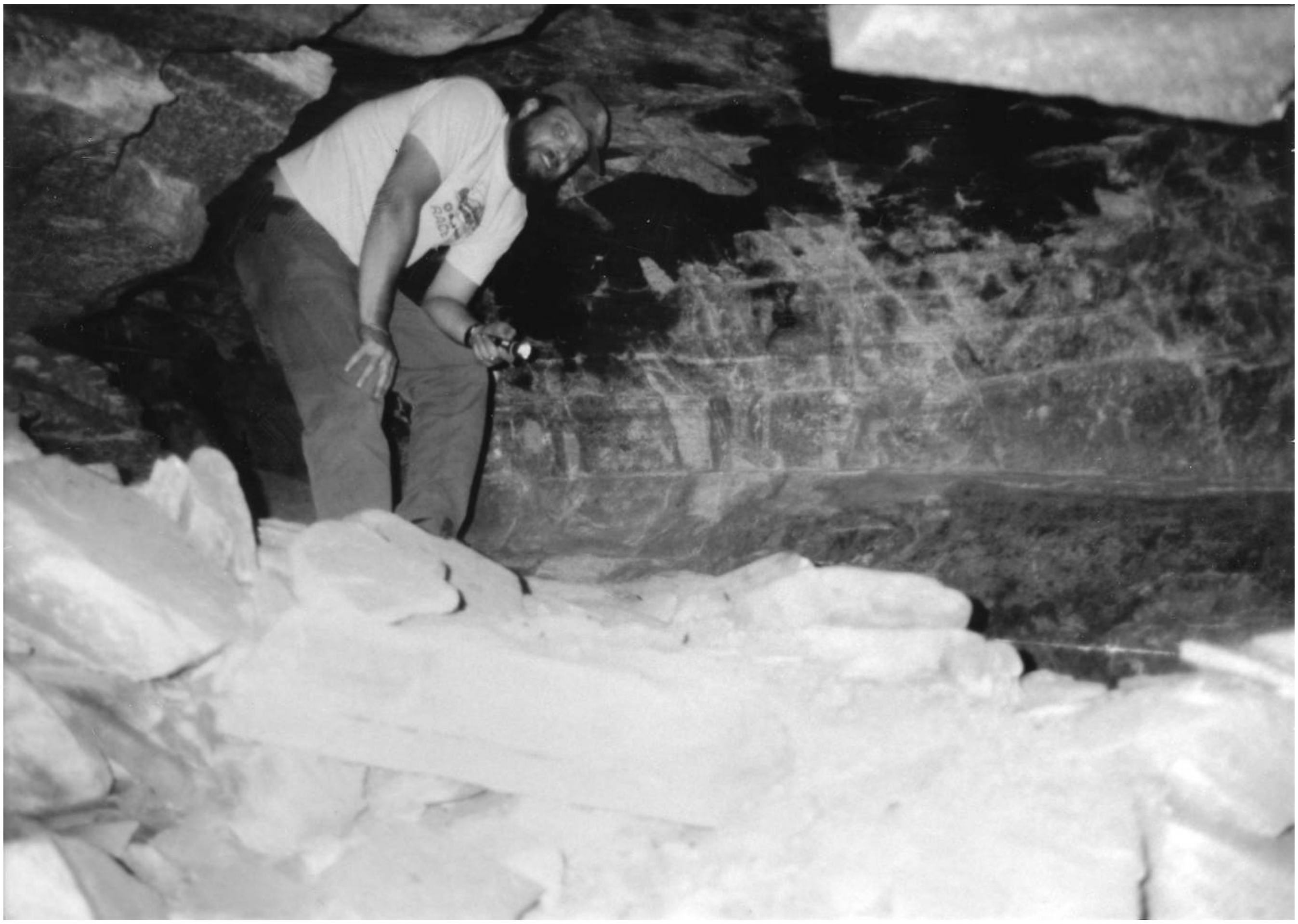

Fig. 2. *Interior chamber of Greenstone Mine showing carbon on wall.*

1997) on periods of enhanced cave speleothem (stalagmites, stalactites, flowstones) and tufa deposition in the summer-rainfall zone of southern Africa has demonstrated a late-Holocene wet period from about 1,500 to 1,000 radiocarbon years B.P. Perhaps we are seeing evidence of this wet period in the flooding of the mine.

Another large mine is Greenstone Mine, named for the greenish muscovite found in tailings fragments below it. Veins of specularite are visible in one chamber of this mine. Located not far above the base of Male Hill, it has four interconnected terminal chambers, extending over an approximate 40-m area. We believe that at least 1,000 tons of rock were moved in this large complex of mining chambers. Greenstone Mine may have been abandoned after the desirable material had been removed from most of the chambers.

THE MATERIAL MINED: HISTORICAL INSIGHTS

Specularite is generally known in southern Africa as black hematite. In Setswana it is called *sebilo.* Historical sources describe the use of specularite in the area. At the Blinkklipkop specularite mine, in the Northern Cape, John Campbell (1835:112) writes: "Blink Mountain is a kind of Mecca to the nations around, who are constantly making pilgrimages to it, to obtain fresh supplies of the blue shining powder and the red stone." Burchell (1967 [1822–24]:256) describes *sebilo* as "a shining powdery iron-ore of a steel grey or bluish lustre." He writes that it is prepared by grinding it with grease "and smearing it generally over the body, but chiefly on the head; and the hair is so much loaded and clotted with an accumulation of it, that the clots exhibit the appearance of lumps of mineral." He comments that the glittering ore is considered valuable and "constitutes in some degree an article of barter with the more distant tribes . . . so that the use of it extends over at least five degrees of latitude, or among every tribe I visited." In *Life in the Wilderness,* Metheun (1846:95) notes that "the members of the different Bechuana tribes often smear their bodies with fat and red ochre, and cover their hair with a paste consisting of black lead-ore, called sibilo, till their heads shine with it as if . . . they wore metal skull-caps." Metheun adds that the application of such substances to the body helps to prevent the skin from chapping or cracking because of exposure to the sun and air.

Such records reveal that specularite and other types of hematite, as well as mica, were of considerable importance as cosmetics or body/clothing paints and as a natural form of sunscreen. The use of specularite was not restricted to a single ethnic group but characteristic of a variety of peoples speaking both Khoisan and Bantu languages. The material extracted from the ancient Tsodilo mines was probably used for the same general purposes as have been described in the historical sources. Whereas smelting for iron production is certainly another possibility, Denbow and Wilmsen (1986) state that their surveys and excavations at Tsodilo have failed to reveal evidence of large-scale iron production such as furnaces or substantial slag heaps. Although some slag was found, Miller's (1996) recent analysis indicates that it was most likely the result of iron smithing rather than smelting.

RADIOCARBON DATES AND THEIR IMPLICATIONS

The highly consistent cluster of 19 radiocarbon dates obtained from 10 different mines is one of the most comprehensive dating records of prehistoric mining that is available for a single area (see table 1). Quite clearly, most of the mines were being worked between approximately A.D. 800 and 1025. Interestingly, this period is several hundred years earlier than the more dramatic labor projects of southern Africa associated with the stone-walled ruins of the Zimbabwe state. No earlier dates or other inconsistent dates were recovered from the mining deposits at Tsodilo.

Closer inspection of the dates shows that at least nine mining sites were being worked within about a century. Moreover, three mines (North Hill, Kudu Horn, and Upper Male Hill) have identical radiocarbon ages. This is impressive because one is located high, in a relatively inaccessible area on Male Hill, another is at the base of Female Hill, and the third is a considerable distance away on the slope of North Hill. Two additional mines (Glittering Hands and Male Hill A) also have identical dates and are very close in age to the three mines just mentioned; these two mines are also located on different hills. Comparative dates from spatially separated areas within two of the largest mines include five dates for Greenstone Mine (obtained from four different chambers) and three dates for Big Mine (one from an alcove adjacent to the upper entrance, one from inside the upper entrance, and a third from the center of the mine, near the stack of rocks mentioned above). The closeness of the dates for the different areas in each of these large mines, given the tons of rock moved by the miners, is striking evidence of the intensity of the work efforts of prehistoric peoples at Tsodilo.

This intensity can be understood in the context of scarcity. Good sources of specularite were comparatively rare. There are relatively few hills in the Kalahari sandveldt, and the nearest to Tsodilo are about 200 km distant. Other large specularite mines are known from distant areas to the south of Tsodilo, such as at Sebilong, near Thamaga on the southeastern hardveldt of Botswana, and at the southern margin of the Kalahari

TABLE 1
Tsodilo Mines Radiocarbon Dates

Site	Data B.P.	Calibrated (1 sigma)	Beta Lab #
Lower Elephant tailing, F	1,230 ± 60	A.D. 705–885	84706
Male Hill A, tailing, M	1,210 ± 60	A.D. 770–890	84715
Lower Elephant, F	1,160 ± 60	A.D. 800–975	84705
Mica Schist (Upper Elephant), F	1,150 ± 50	A.D. 865–975	47863
Main's Mine, F	1,140 ± 60	A.D. 865–985	96237
Greenstone, Lower, M	1,120 ± 70	A.D. 875–1000	96234
Big Mine, alcove, F	1,120 ± 60	A.D. 880–995	84709
Big Mine, center, F	1,110 ± 60	A.D. 885–1000	84708
Big Mine, upper, F	1,100 ± 60	A.D. 885–1005	84707
Glittering Hands, F	1,090 ± 60	A.D. 890–1010	84710
Male Hill, A, M	1,090 ± 60	A.D. 890–1010	84716
North Hill, N	1,080 ± 50	A.D. 905–920, 950–1010	84718
Kudu Horn, F	1,080 ± 50	A.D. 905–920, 950–1010	84713
Upper Male Hill, M	1,080 ± 50	A.D. 905–920, 950–1010	65194
Greenstone, Lowest, M	1,060 ± 60	A.D. 960–1020	96236
Greenstone, Left, M	1,050 ± 60	A.D. 970–1025	84712
Greenstone, Right, M	1,000 ± 50	A.D. 1000–1040	84711
Greenstone, Lower, M	1,000 ± 60	A.D. 995–1040, 1105–1115	96235
Below Procreation Rock, F	960 ± 60	A.D. 1015–1170	84717

NOTE: F = Female Hill; M = Male Hill; N = North Hill. Other designations (e.g., A) refer to field names of sites. Dates were processed by Beta Analytic and calibrated following the Pretoria Calibration Procedure program. All dates are from charcoal deposits overlying the floor of mines with the exception of the two tailings dates, which were from charcoal at the base of the tailings.

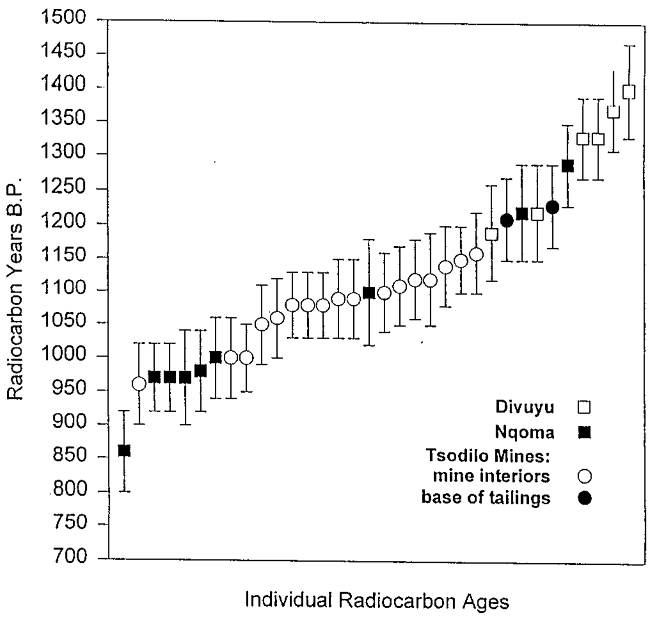

FIG. 3. *Radiocarbon ages for Divuyu, Nqoma, and the Tsodilo mines. The chronological overlap of the dates is indicated by the lines presenting the range of variation associated with each date at one sigma.*

in South Africa at Blinkklipkop (Thackeray, Thackeray, and Beaumont 1983). Elsewhere in South Africa and Swaziland specularite mines are also well known (see Beaumont 1973). In several cases in South Africa and Swaziland, ostrich-eggshell containers lined with specularite have been found, one of them with an undated burial (Humphreys 1974).[4] In another case, in southern Namibia, two Khoi pots half-filled with specularite were excavated from a cache by Jacobson (1977), and the nearest known source of specularite was reported to have been about 100 km away.

THE MINES IN THE CONTEXT OF THE ARCHAEOLOGICAL RECORD

There is a very close fit between the dates for the Tsodilo mines and the ages of the two well-dated villages of Divuyu and Nqoma, located on Female Hill (fig. 3). Nqoma, which was heavily involved in the use of iron as well as regional and long-distance trade, flourished between the 9th and the 10th century (Denbow and Wilmsen 1986). Quite clearly, the mining activity at Tsodilo was at its peak during the time of Nqoma and ceased at approximately the same time as the abandonment of the village if not slightly earlier. Whereas some mines may have been exhausted by this time, there are other sources of specularite and mica that could have been exploited at Tsodilo. More than likely, the mining at Tsodilo ceased because long-distance and regional trade in the northwestern Kalahari had declined and villages such as Nqoma had become less significant or been abandoned. The trade routes and centers shifted to other areas in southern Africa at around A.D. 1100. Significant new centers with evidence of sociopolitical complexity such as Mapungubwe emerged far to the southeast in the Shashe/Limpopo River area (Huffman 1996). However, environmental factors may have contributed significantly to the abandonment of Tsodilo. As we have seen, cave speleothems and tufas suggest a wet period from about 1,500 to 1,000 years ago, and, significantly, the occupation of Divuyu and Nqoma lasted from about 1,400 to 900 years ago and the mining occurred in the period 1,200–1,000 years ago. This striking correspondence between the occupation of the iron-using villages in the Tsodilo Hills and the mining at a time of increased rainfall is surely not coincidental. It would not be unreasonable to suggest that the Nqoma settlement and the mining ended in part because traditional sources of water were no longer reliable.

Whereas our review of the radiocarbon dates has established that the intensive mining occurred during a relatively brief period, we have learned that specularite was also being used by stone-tool-using peoples who occupied rock shelters and caves at Tsodilo. Most of what is found in the rock shelters consists of metallic crystals, although pieces of the specularite vein material occur in the form of small grindstones and hammerstones. Much of this use was substantially earlier than the period indicated by the dates for the mines and even much earlier than the villages or the use of iron and pottery in the area. It is by no means certain how these earlier peoples obtained the materials. They could have hammered them from surface exposures in the bedrock or utilized open-cast mining rather than tunneling into the rock. Open-cast mining removes materials from the surface of rocks or deposits. We have found one mine of this type with no charcoal or other suitable material for dating. Moreover, there is an extensive, heavily battered area, rich in metallic crystals, adjacent to the Upper Male Hill Mine.

Excavations at Depression and White Paintings Shelters as well as Rhino and Upper Male Hill Caves have revealed small metallic crystals in various levels in association with flaked stone artifacts. In the Tsodilo rock shelters and caves the time range for the use of this material extends from the period of chronological overlap with the villages of Divuyu and Nqoma back to at least 5,000 years ago. In Rhino Cave, both small metallic crystals and either grindstones or hammerstones of specular hematite were recovered in Late and Middle Stone Age levels. While the Middle Stone Age levels are not dated, they have produced numerous typical Middle Stone Age projectile points that elsewhere in Botswana may be as much as 60,000 years old (Brooks et al. 1990).[5]

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[4] It is likely that ground or powdered specularite would be difficult to find in archaeological contexts outside of such containers. For this reason, the metallic crystals and pieces of bedrock containing traces of specularite veins are the materials that are found in the rock shelters of Tsodilo.

[5] Usually, the metallic crystals occur in low frequencies, about five or fewer per 10-cm level, in both LSA and MSA 1-m^2 excavation units.

At Tsodilo, the emergence of villages of iron-using agropastoralists coincides with the change from casual procurement of specularite to systematic and intensive exploitation. As we have seen, both White Paintings and Depression Shelters and the villages were occupied during the time of the mining. Our work shows that there were relationships between the villages and the shelters; small quantities of charcoal-tempered decorated pottery similar to the village pottery have been found amidst the lithic debitage in the White Paintings and Depression Shelters. In addition, iron clips found associated with stone artifacts at Depression Shelter are identical to clips found at Nqoma. Moreover, Denbow and Wilmsen (1986) report that stone artifacts were found in the deposits at the village of Nqoma, along with numerous iron artifacts and evidence of food production, suggesting that local stone-tool-using foragers were incorporated into the life of the village. Interestingly, metallic crystals of hematite identical in appearance to the material from the Upper Male Hill Mine were recovered with the flaked stone artifacts at Nqoma (Phaladi 1991).

Paleolithic archaeologists in the Kalahari have often associated late prehistoric flaked stone artifact assemblages with the ancestors of Khoisan peoples (Yellen and Brooks 1989, Yellen 1990), whereas early agropastoral villages containing abundant evidence of iron such as Divuyu and Nqoma have generally been related to the prehistory of speakers of Bantu dialects. One of the goals of the Tsodilo mines project was to try to determine who was doing the mining. The comparative radiocarbon dates provide a powerful link with the inhabitants of Nqoma, but a closer look at the archaeological data suggests a more complex story. The hammerstones found in the mines were not typical of those found at sites where iron was important. Such hammerstones are ground smooth on most sides and are usually almost square, with dimples on two to four sides where they have been used to strike metal tools. They also tend to be small. Ours were large and smooth but not necessarily ground and had not been shaped; they had bash marks on the ends and lacked dimples. Furthermore, no metal tools were recovered, nor did we see metal tool marks inside the mines such as would have been produced by the use of iron gads as in the gold mining studied by Summers (1969) and, more recently, by Swan (1994). No iron-mining tools, with the possible exception of a chisel, were found in either of the villages, but other iron artifacts were numerous. Over 500 were found at Nqoma, and the majority of them are thought to have been used for making jewelry (see Miller 1996). Given the survival of a variety of iron artifacts in the villages and iron beads in the rock shelters, we would have expected to find iron tools (especially broken mining tools) in the mines or in the tailings if the miners were people who relied heavily on iron. At the same time, low frequencies of lithic debitage and a few decorated, charcoal-tempered potsherds were recovered from excavations in several of the mine tailings as well as in a test pit excavated to bedrock in Greenstone Mine. This 1-m^2 test pit was excavated to a depth of 110 cm in an interior chamber that was nearly sealed from the outside by rockfalls. The deposits contained small bladelets and flakes of crystalline quartz, a chert microcore and flake, hammerstones, and incised and undecorated potsherds. Small pieces of bedrock bearing horizontal veins of specularite were found in the same level as the chert lithics. There were also pieces of sheet mica.

The overall archaeological signature from the investigation of numerous mines, which includes both stone artifacts and potsherds, hints at three possible interpretations: (1) Stone-tool-using foragers living in caves and rock shelters did the mining. Perhaps the nearby iron-using communities created an increased demand for specularite and the stone-tool-using peoples who inhabited sites such as Depression and White Paintings were simply filling this demand, exchanging specularite for trade items. (2) Both stone-using and iron-using peoples were actively involved in the mining in a collaborative effort; certainly there is a striking parallel in the historical record showing that specularite was valued by different ethnic groups. (3) The people who occupied the shelters, caves, and villages during the time of the mining were one and the same people but were using different aspects of a broad range of available technological and subsistence options at different sites.

It is clear from our work that most of the subsistence activities represented in the Tsodilo rock shelters during the time of the mining reflects hunting, fishing, and foraging.[6] If either of the first two interpretations is correct, our findings add a new dimension to the study of hunter-foragers in the Kalahari. Few anthropologists would have envisioned mining as a likely activity for the Kalahari foragers of 1,000 years ago. If the third interpretation is correct, then the problem will ultimately be much more difficult to resolve and may require a rethinking of the ways in which archaeological materials from the region have been interpreted to date.

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[6] There are two rock shelter sites at Tsodilo that contain significant amounts of fauna in microlithic artifact levels that have also yielded pottery. The Depression Shelter fauna, while not abundant, contained only wild animals (Robbins 1990). The fauna from White Paintings Shelter was much more abundant. Species identified by R. G. Klein and R. Milo (personal communication) were all wild with the exception of a single domesticated sheep mandible. This specimen, from sq. 23, 40–50 cm, has recently been dated by AMS to 1,225 ± 60 B.P. (Sealy and Yates 1996). The age is consistent with the dates of the Tsodilo early iron-using villages and the mines. Among the wild plants found at both shelters mongongo nuts are predominant (Robbins and Campbell 1990). Maize is evident at White Paintings Shelter in more recent contexts.

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Evolution of Skin Color in Yemenite Jews[1]

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Studies of human microevolution and adaptation are often plagued by the short time span for which data are available. Many indices of biological variation, such as blood groups, have been discovered only in this century or, in the case of many DNA markers, only in the past several years. Even anthropometric traits such as height and head length are often available only for somewhat longer lengths of time. The long-term effects of natural selection and other evolutionary forces must often be inferred from other clues, such as the geographic distribution of traits (e.g., the correspondence of sickle cell allele and malaria or the relationship between cranial shape and latitude).

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Human skin color provides an excellent example of using geographic correspondence to infer past natural selection. The strong relationship of skin color and latitude suggests long-term genetic adaptation to ultraviolet radiation (Roberts and Kahlon 1976, Robins 1991). However, our observations are limited to a single point in time, the present. We can detect the outcome of past evolutionary changes in skin color but are much less able to track changes in skin color over time. While some work has examined generational changes in skin color (e.g., Towne and Hulse 1990), we have not been able to examine skin color changes over longer periods of time simply because accurate measurement of skin color is a recent development (Robins 1991). Thus, we can examine short-term changes, over a generation or so, and we can examine the outcome of some unknown but long period of time that has shaped the past evolution of human skin color. What we lack is information on the degree of potential evolutionary change over many generations. How long did the evolution of modern human skin color take? Does skin color change rapidly (on the order of a millennium or so), or does it represent evolution over much longer periods of time? On the basis of computer simulation, Livingstone (1969) suggested that modern differences in skin color could have arisen in as few as 800–1,500 generations (roughly 20,000–37,500 years). However, such simulations show us only what *could* happen and not necessarily what actually *did* happen. We lack information on the rate of change in skin color over time.

The problem here is that an analysis of the rate of change in skin color over many generations requires data that are not available. An alternative, adopted here, is to compare observed skin color with that expected under a geographic model for a population known to have moved from one region to another at some point in the past. Haldane proposed something along this line when he suggested that, because American Indians near the tropics are not as dark as tropical populations in the Old World, they had not yet fully adapted to their new environment and therefore the evolution of modern human skin color differences took longer than the time since initial habitation of the New World (cited in Livingstone 1969). This paper uses similar logic and extends it to a quantitative analysis of a specific human population with a known history of movement into a different latitude—the Yemenite Jews.

The history of the Habbani Yemenite Jews provides an opportunity to examine the potential for evolutionary change in human skin color across several millennia. Thousands of years ago, this endogamous population left the Middle East and settled in the town of Habban in South Yemen. The exact date is not known with certainty, although records indicate a Jewish presence in Habban over 1,500 years ago (Towne and Hulse 1990) and tradition suggests that they might have arrived there 2,600 years ago (Hulse 1969). Prior to 1940, only a handful of Habbani Jews had resettled in Israel. By 1950, as part of Jewish emigration following Israeli independence, all remaining Habbani Jews moved back to Israel (Towne and Hulse 1990).

From an evolutionary perspective, this history suggests a Middle Eastern population that had spent between 1,500 and 2,600 years in a different, more southerly environment. Of particular interest is the difference in latitude between Israel (32° north latitude) and South Yemen (15° north latitude). Several comparative studies have documented a strong relationship between latitude and skin color, with populations being lighter farther away from the equator (Roberts and Kahlon 1976, Tasa, Murray, and Boughton 1985, Relethford 1997). The movement of the Habbani Jews from Israel to South Yemen and back again suggests several hypotheses regarding the rate of human skin color evolution. If there was sufficient selection during the Habbani Jews' stay in South Yemen over several millennia, then we would expect their skin color to be similar to that of other populations at the same latitude as South Yemen. If there was insufficient selection operating over this time, then we would expect their skin color to resemble more closely that of populations at the same latitude as Israel. An intermediate hypothesis is for limited change during their South Yemen occupation, resulting in skin color intermediate between those characteristic of the two latitudes.

These hypotheses can be evaluated by examining the skin color of the Habbani Yemenite Jews and comparing it with skin color in other populations at various latitudes. In 1966, Hulse (1969) measured skin color for 81 adult male and 97 adult female Habbani Jews who had moved to Israel in 1950. Hulse used an EEL portable reflectometer to measure the percentage of reflected light at different wavelengths using the standard upper inner-arm site, which minimizes environmental variation (Robins 1991). For this report I confine my observations to EEL filter 609, which samples the visible wavelength at 685 nanometers (red light). This filter provides maximal separation of human populations and is a common single measure of human skin color (Tasa, Murray, and Boughton 1985, Relethford 1997).

Mean skin reflectance among Hulse's subjects was 52.3% for the adult male sample and 56.7% for the adult female sample. Preliminary comparisons suggest that these values are more typical of the latitude of Israel than of South Yemen. For example, mean skin color (EEL filter 609) is 57.6% for adult male Israeli Arabs and 58.8% for adult female Israeli Arabs (Sunderland 1979); average reflectance for populations at roughly the same latitude as South Yemen range from roughly 29 to 36% for males and from 28 to 36% for females (Huizinga 1968, Harrison et al. 1969, Sunderland 1979).

A more precise comparison makes use of the strong relationship of skin reflectance and latitude across the Old World, thus allowing the latitude of the Habbani Jews to be estimated from their average skin reflectance. Comparative data for EEL filter 609 were taken from the literature as part of an ongoing study of human skin color variation in the Old World (Relethford 1997). Mean skin reflectance was extracted for 48 male samples and 23 female samples located between 10° and 40° north latitude across the Old World, a range that encompasses the geographic movement of the Habbani

Jews. These samples were used to derive a regression equation that allows latitude to be predicted from mean skin reflectance. The regression model used includes sex as a covariate and is $y = a + bx_1 + cx_2$, where y is latitude, x_1 is mean skin reflectance, and x_2 is a binary variable equal to 1 for male samples and 0 for female samples. The Habbani samples are *not* included in the regression, and their latitudes are estimated from the relationship of latitude and skin reflectance in the other samples. Using the Habbani skin reflectances to estimate their latitude can provide insight into the possibility of evolution in skin color over several millennia. For example, if the estimated latitude is close to South Yemen (15°) then skin color has changed rapidly. If, however, the estimated latitude is close to Israel (32°) then no change in skin color has occurred during their stay in South Yemen. An intermediate value ($15 < \hat{y} < 32$) would suggest partial but incomplete natural selection.

The regression is highly significant ($F = 142.90$, d.f. = 2 and 68, $P < 0.001$) and accounts for a large proportion of the observed variation in latitude ($R^2 = 0.81$). Using this equation, the estimated latitude for the Habbani Jews is 30.11° north latitude for the male sample (s.e. = 0.48) and 33.25° north latitude for the female sample (s.e. = 0.71).

These estimates are based on the relationship of latitude and skin reflectance across the Old World between 10° and 40° north latitude and include populations in South and East Asia. To determine if this population heterogeneity affects the results, the analysis was repeated using samples from a narrower geographic region defined by a rectangle ranging from 10° to 40° north latitude and from 30° to 60° east longitude. These criteria provided 19 samples for analysis (12 male, 7 female). This regression is also highly significant ($R^2 = 0.90$, $F = 70.99$, d.f. = 2 and 16, $P < 0.001$). The predicted latitude for the Habbani Jews is 31.41° north latitude for males (s.e. = 1.01) and 33.32° north latitude for females (s.e. = 1.32). The results of the two analyses are virtually the same.

The predicted latitude for the Habbani Jews based on their skin color is roughly 32° north latitude, which is the geographic center of Israel. Even though the Habbani Yemenite Jews spent between 1,500 and 2,600 years in a different environment, there has been no change in skin color. It seems that the evolution of human skin color requires greater time depth and is not indicative of a rapid microevolutionary change. Other populations, also with known histories, must be examined to provide further insight into the rate of microevolution in skin color. The basic method here can also be applied to other traits that show a strong geographic relationship.

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Climate, Plant Ecology, and Central Mexican Archaic Subsistence[1]

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Human subsistence relies on obtaining plant resources through foraging (harvesting the native flora), cultivation (manipulating plant distributions), and domestication (genetic modification of plants) (Rindos 1984, Blumler and Byrne 1991). To understand how plant domestication arose in Mexico (Harlan 1971, McClung de Tapia 1992), we examined foraging and cultivation practices between 11,000 and 4,000 B.P. The best evidence

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for subsistence patterns comes from excavations in the semiarid valleys of Oaxaca (Guilá Naquitz Cave) and Tehuacán (Coxcatlán Cave) (Byers 1967*b,* Flannery 1986*b*). At both sites the nature of plant exploitation is viewed as changing through time; Flannery's (1986*b*) analysis implicates changes in harvesting strategy and scheduling at Guilá Naquitz, while Smith (1967) and MacNeish (1992) ascribe changes in plant remains to cultivation at Coxcatlán. These analyses explicitly assume climatic and ecological equivalence with modern times (Smith 1967, Flannery 1986*b*) and consequently assign any changes to human behavior. However, recent paleoclimatic studies indicate that significant climatic change occurred during this time period (Markgraf 1993). As these important archaeological sites occupy semiarid environments in which several ecological zones are closely juxtaposed (MacNeish 1992), even modest climate changes could dramatically shift species' ranges relative to the archaeological sites. Moreover, previous analyses assumed that entire plant communities moved together, whereas in fact species often respond individually to climate (Graumlich and Davis 1993). Finally, because preservation is a problem for these early Holocene sites (Pearsall 1995), it is important to control for species-specific and stratum-specific differential preservation. This study reexamines the nature of plant exploitation at these sites in the light of paleoclimatic, meteorological, and ecological data, proposing testable hypotheses and evaluating them in terms of the observed plant remains at Guilá Naquitz and Coxcatlán Caves. The observations at these sites correspond to the plant distributions predicated by the environmental changes of the early Holocene, suggesting that humans at Guilá Naquitz and Coxcatlán were mostly foraging on the natural local flora.

METHODS

The temporal controls for this study were developed entirely from ^{14}C dates (uncalibrated throughout this paper). There are chronological inconsistencies with both the Coxcatlán (Long et al. 1989, Hardy 1996) and Guilá Naquitz (Flannery 1986*c*) sequences. Both of these sites are dry caves, and the remains may have been subjected to postdepositional mixing (e.g., by rodents). To minimize these effects, we used only the most abundant plant remains for analyses. For Guilá Naquitz we eliminated the deepest zone (E) because all the radiocarbon dates from it appear to be intrusive (Flannery 1986*c*). For Coxcatlán Cave, Hardy (1996) has pointed out that the levels below XVIII have a less clear stratigraphic order. Therefore our analysis combines many strata and divides Coxcatlán's into only two time periods, early Holocene (strata XXVI–XIV, Ajuereado and El Riego phases) and middle Holocene (strata XIII–VIII, Coxcatlán and Abejas phases). The inclusion of the Ajuereado-phase samples does not affect our analyses, since these strata contain very few botanical remains.

Paleoclimatic records for Central Mexico (defined as west of Tehuantepec isthmus and south of 22° N) from the late Pleistocene to mid-Holocene were surveyed (fig. 1). To provide a basis for relating regional paleoclimatic studies to Central Mexico, we examined the present-day meteorological relationships between regions. Precipitation data covering 30 years or more were analyzed for 75 stations in Central Mexico (Vose et al. 1992). Mean precipitation (or sea-level pressure) and z-scores were calculated for each station for each year between 1921 and 1987, and a mean of all the stations' z-scores was calculated for each year. Modern Central Mexican precipitation trends were correlated with modern precipitation trends from regions with good paleoclimatic records. Pearson correlations were used to estimate the strength of associations (Wilkinson 1989).

A statistic was developed to control for differential preservation of species and of strata in which the number of plant remains found at time t for species s is R_{st} and the total number of remains for species s is $R_s = \Sigma R_{st}$. The proportion of a species's remains at a given time is $P_{st} = R_{st}/R_{st}/R_s$. The total frequency of plants at time t is $P_t = \Sigma P_{st}$, and the total number of species in the analysis is n. The expected frequency for a given species at a given time is $E(P_{st}) = P_t/n$, and the observed-to-expected ratio is $O(P_{st})/E(P_{st}) = nP_{st}/P_t$. This ratio indicates whether a plant was more or less abundant than expected at a given time relative to the abundance of other plants at the site.

Expected ranks of abundance during each time phase were determined from climatic and ecological reconstructions. For each species a Spearman rank correlation was calculated between the observed $O(P_{st})/E(P_{st})$ variable and a second variable based on the species's environmental group. To test whether species abundance was correlated with paleoecological changes, each species's relative abundance was correlated with its predicted abundance based on the paleoecological reconstructions and all of the species correlations were averaged. To test whether plants with similar environmental niches were exploited together, each species's relative abundance was correlated with that of all other species in the same environmental group and an unweighted average calculated for all of the correlations within the group. A permutation test with 1,000 randomizations evaluated the significance of these average correlations. For each permutation P_t was randomized across each time zone for each species and average correlations were recalculated.

CENTRAL MEXICO PALEOCLIMATE

Brown (1985) summarizes the climatic evidence from the Central Mexican lacustrine pollen, and we agree with most of his interpretations. Central Mexico was generally cold and dry from 12,000 B.P. to 9,500 B.P. (fig. 1); from 9,500 B.P. to 6,500 B.P. it was generally warmer and moister, and from 6,500 B.P. to 4,000 B.P. it was variable but generally drier. Markgraf (1993) has recently summarized pollen and lake-level data for Mexico as follows: cold and dry from 18,000 to 10,000 B.P.,

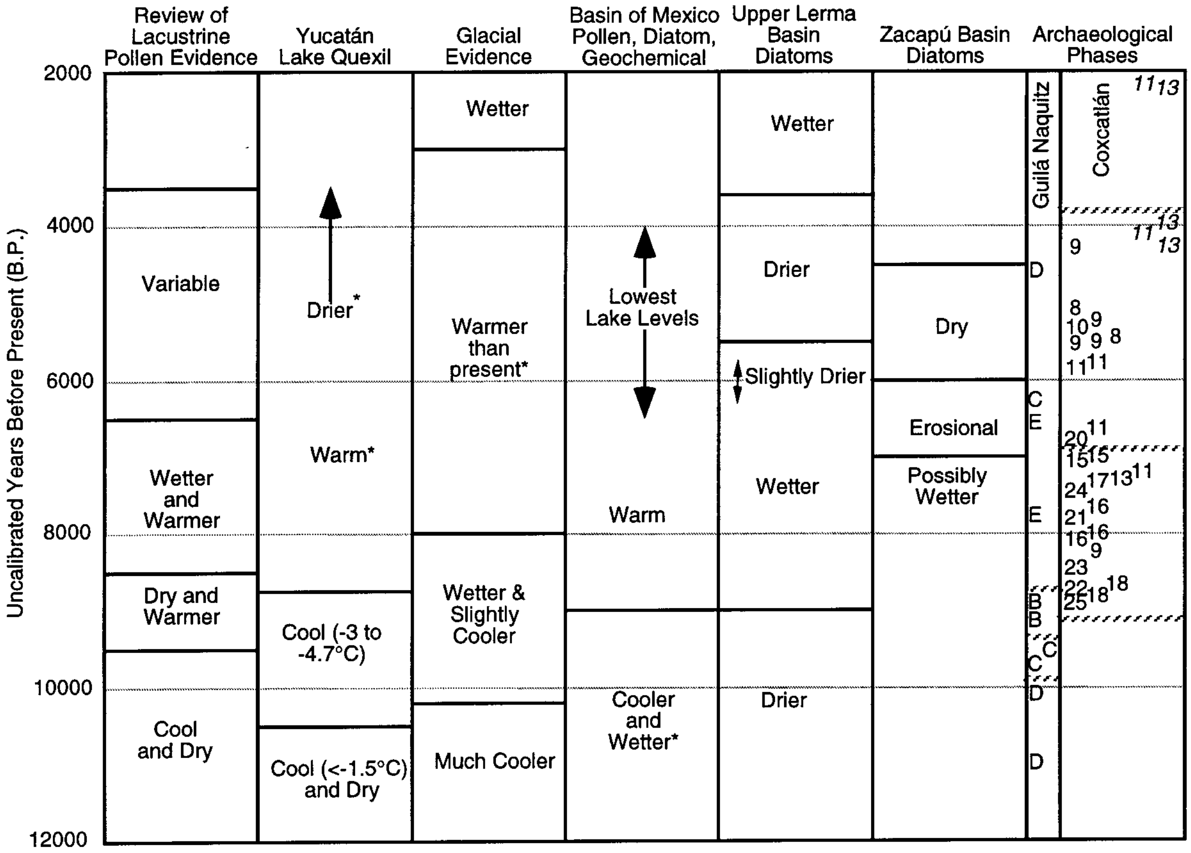

FIG. 1. *Summary of paleoclimatic evidence for Central Mexico. Analyses come from a review of lacustrine pollen composition (Brown 1985), Yucatan pollen (Leyden et al. 1994), glacial positions (Heine 1988), Basin of Mexico (Lozano-Garcia et al. 1993), Upper Lerma Basin diatoms (Metcalfe et al. 1991), and Zacapu Basin diatoms (Metcalfe 1995). Paleoclimatic records with poorly dated stratigraphy are indicated by an asterisk. Guilá Naquitz zones B through E and Coxcatlán strata 8 through 25 are plotted on the basis of radiocarbon dates, both accepted and rejected, for each stratum. The AMS radiocarbon dates on the maize from Coxcatlán Cave are in italics (Long et al. 1989).*

warm and moist from 9,000 to 6,000 B.P. and modern conditions from 5,000 B.P. to the present.

Mexican glacial advances generally result from greater moisture rather than colder temperatures (Heine 1988). The glaciations of Mexican volcanoes clearly indicate greater moisture in the early Holocene (10,000–8,200 B.P.) (Heine 1984, 1988), as two large glacial advances (about 2,100 m) occurred during this period. Cooler temperatures were not involved in these advances, since glaciers did not develop on the temperature-sensitive low-altitude volcanoes.

Unfortunately, the Tehuacán and Oaxaca valleys have no stratified lakes that would facilitate the reconstruction of ancient climate, and the few pollen samples available from archaeological contexts (Schoenwetter and Smith 1986) are small and poorly dated. The environmental reconstruction suggests significant changes in the vegetation between 12,000 and 4,000 B.P. Rodent faunal remains were examined at both Tehuacán and Oaxaca, but their climatic interpretation is inconclusive (Flannery 1967, Flannery and Wheeler 1986). The distribution and changes of rodents at Guilá Naquitz possibly indicate a different climate from the present one (Flannery and Wheeler 1986), but small sample size precludes conclusive support. The Tehuacán Valley's rodent and large fauna were very different from modern fauna in the undated stratigraphic zones below 8,700 B.P.; the fauna includes extinct animals and animals with adaptations to arid, cool regions (Flannery 1967). The relative abundance of various rodent species also differed during the Holocene (Flannery 1967); there were significant differences between the late Ajuereado- and El Riego–phase rodents and the Coxcatlán- and Abejas-phase rodents (*G*-test of independence using the rodent MNI, $G = 14.6$, d.f. $= 4$, $P = 0.006$). We suggest that these differences reflect early Holocene climatic variation.

Hydrologic studies of the El Riego Cave in the Tehuacán Valley indicate that the water table has dropped at least 22.5 m since the early Holocene (Brunet 1967).

Layers 5 and 6 of El Riego Cave show signs of a higher water table including travertine deposition, the bases of two stalagmites, and an absence of plant remains (MacNeish and Cook 1972). The strata's good lithic-artifact samples are seriated with Coxcatlán strata XXIII (^{14}C 8,200 B.P.) and XVI (avg. ^{14}C 7,667 B.P.) (Johnson and MacNeish 1972). In contrast, layer 4 of El Riego Cave appears dry and has few botanical remains (MacNeish and Cook 1972), and this layer seriates with Coxcatlán strata XII and XI (avg ^{14}C 6,121 B.P.) (Johnson and MacNeish 1972). The water table never again reached such heights. This drop in groundwater level probably resulted from both erosion (Brunet 1967) and reduced regional precipitation. To conclude, this valley was wetter and had a much higher water table in the early Holocene, and this would have had a substantial effect on the abundance of plants which use groundwater.

Botanical remains decompose in the humus layer of the soil. Fungi and bacteria, assisted by hydrolysis, break down plant remains; uncharred botanical remains are decomposed in active moist humus layers. Soil moisture explains most of the variance in decay rates and microbial activity (Zhang and Zak 1995). Therefore the preservation of botanical remains will reflect the moisture level of the humus layer. Lithic and to a lesser degree bone artifacts are unaffected by soil moisture levels during deposition. If we assume that the number of lithic and bone artifacts is representative of site activity levels and proportional to the number of botanical remains originally deposited, then we can roughly estimate soil moisture at the time of deposition (fig. 2). Botanical preservation relative to lithic and faunal deposition was poorest in Guilá Naquitz zone B and Coxcatlán's El Riego strata. This poor botanical preservation between 9,000 and 7,000 B.P. suggests high soil moisture during this period.

These climatic changes in Mexico were part of worldwide changes in climate. Before 10,000 B.P. world climate was heavily influenced by glaciers. Recent circulation models suggest that the tropics were much colder, with temperatures 5–6°C cooler in Central Mexico (Webb et al. 1997). Most terrestrial data (noble-gas concentrations, pollen, and glacial advances) indicate much lower temperatures (5–6°C) for tropical America and the Southwestern United States (Stute et al. 1995, Thompson et al. 1995, Colinvaux et al. 1996).

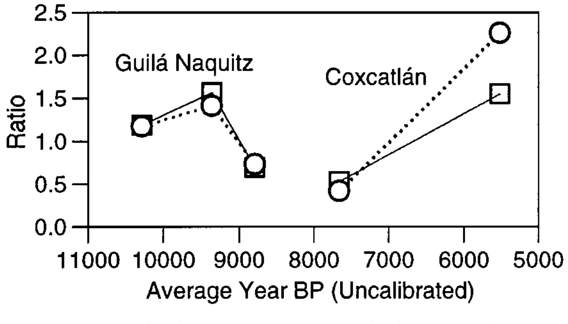

FIG. 2. *Ratio of plant remains to lithic artifacts* (squares) *and faunal remains* (circles) *over time. Faunal remains frequencies are based on minimum number of individuals (MNI) of deer and cottontail.*

The climatic changes from 10,000 to 5,000 B.P. in Mexico agree with paleoenvironmental indicators from five major climatic systems that affect modern Mexican precipitation (table 1): (1) El Niños explain much of the interannual variability in Mexican precipitation patterns (Diaz and Kiladis 1992), but paleoclimatic studies conflict as to the persistence of the El Niño/Southern Oscillation throughout the Holocene (McGlone, Kershaw, and Markgraf 1992, Damnati and Taieb 1995, Sandweiss et al. 1996). (2) The increase in early Holocene precipitation is in accord with the predicted effect of Early Holocene glacial melt on the North Atlantic Deep Water formation (Fairbanks 1989). (3) Climatic circulation models suggest that the intensity of Aleutian pressure systems has changed since the early Holocene (Kutzbach et al. 1993), which could have increased winter precipitation in Mexico (Namias 1963, Sanchez and Kutzbach 1974). (4) Atlantic storm tracks may have differed from present ones, as Florida and Haiti provide conflicting paleoclimatic accounts. (5) All indicators suggest that a stronger Intertropical Convergence Zone (see Metcalfe 1987) may be responsible for greater precipitation in Mexico in the early Holocene.

In summary, from the Last Glacial Maximum to 10,000 B.P. the climate of Central Mexico was substantially cooler than at present. It was also more arid throughout most of that time, although effective moisture may have increased for a period around 12,000 B.P. From 10,000 to 8,000 B.P. temperatures were equivalent to present with increases in precipitation. From 8,000 to 5,000 B.P. temperatures, especially summer temperatures, were probably higher than at present. Beginning at 6,500 B.P. there are signs of greater aridity, and by 5,000 B.P. the region was more xeric than it is today.

VEGETATION AND ECOLOGICAL CHANGES

The modern environments and plant ecology of Central Mexico provide a baseline for studying the impact of different paleoclimatic regimes on plant distributions. Both the Tehuacán and the Oaxaca region exhibit a close juxtaposition of several ecological zones along an altitudinal gradient (fig. 3). This altitudinal gradient is directly related to precipitation levels and mean temperatures. Mean temperature and the associated frequency of frost, along with precipitation, are the obvious ecological determinants at Guilá Naquitz. Precipitation alone is the greatest determinant of plant distributions near Coxcatlán Cave. In order to predict how plants reacted to various climatic regimes, the subsistence plants were divided into ecological groups. More than 50 species of plants were used by people at Guilá Naquitz and Coxcatlán (Pearsall 1995); we concentrated on the 19 most numerous species and divided the Guilá Naquitz plants into groups based on water require-

TABLE 1
Meteorological Correlations with Central Precipitation

System[a]		Correlation[b]			Observed[d]		Expected Precipitation	
		R	*P*	Percentage of Precipitation[c]	9,000 B.P.	6,000 B.P.	9,000 B.P.	6,000 B.P.
ENSO-W	Southern Oscillation	−0.44	0.0003	–	±	±	±	±
ENSO-S	Southern Oscillation	0.42	0.0006	–	±	±	±	±
NADW-W	North Atlantic salinity[e]	−0.37	0.0312	–	–	0	+	0
NP-W	Aleutians pressure	−0.31	0.0114	–	–	0	+	0
ATL-S	Florida precipitation	−0.53	0.0000	64	–	0	+	0
ATL-S	Haiti precipitation	0.32	0.0129	53	0	+	0	+
ITCZ-W	Hawaii precipitation	−0.32	0.0084	26	–	+	+	–
ITCZ-S	Panama precipitation	0.45	0.0004	55	0	–	0	–
ITCZ-S	Venezuela precipitation	0.25	0.0471	69	+	0	+	0
ITCZ-A	Galapagos precipitation	−0.39	0.0341	100	–	0	+	0

[a] ENSO, El Niño/Southern Oscillation; NADW, North Atlantic Deep Water; NP, North Pacific; ATL, Atlantic storm tracts; ITCZ, Intertropical Convergence Zone; W, winter, December through February; S, summer, May through September; A, annual, January through December.
[b] Pearson correlation coefficients between the *z*-score of modern Central Mexican precipitation and the *z*-score of other meteorological events.
[c] The proportion of annual precipitation that falls during either the winter or the summer for a given region. Since paleoclimatic records reflect total precipitation, regions with high proportions of precipitation during a certain season are more informative correlates.
[d] Southern Oscillation, McGlone, Kershaw, and Markgraf (1992), Damnati and Taieb (1995); salinity, Fairbanks (1989); Hawaii precipitation, Burney et al. (1995); Aleutians pressure, Kutzbach et al. (1993); Florida precipitation, Watts, Hansen, and Grimm (1992), Watts and Hansen (1994); Panama precipitation, Bush et al. (1992); Haiti precipitation, Hodell et al. (1991); Venezuela precipitation, Bradbury et al. (1981), Leyden (1985); Galapagos precipitation, Colinvaux (1972).
[e] North Atlantic high-salinity years (1948–63) versus low-salinity years (1964–81) (Brewer et al. 1983, Dickson et al. 1988).

ments and frost or chilling tolerance and the Coxcatlán plants into groups based on water requirements only (table 2).

Although the taxa have been divided into xeric versus mesic, many of the mesic species's ranges would increase tremendously with only modest increases in precipitation or groundwater. For example, *Persea* irrigation studies show that growing-season precipitation-to-evaporation coefficients of 0.46 allow very good avocado growth (Levinson and Adato 1991), while today the Coxcatlán region has a ratio of 0.38 (Byers 1967*a*, Woodbury and Neely 1972). This supports the idea that 100–200-mm greater precipitation and/or a higher water table could make the Coxcatlán region hospitable to *Persea*. *Prosopis* is currently restricted in Tehuacán and Oaxaca to the regions with the highest groundwater (Flannery 1967, Kirkby, Whyte, and Flannery 1986). Although it can survive with fairly deep groundwater, a closed-canopy forest can form only with perennially available groundwater, and riparian habitats with good groundwater can increase *Prosopis* productivity tenfold (Stromberg et al. 1992, Stromberg, Wilkins, and Tress

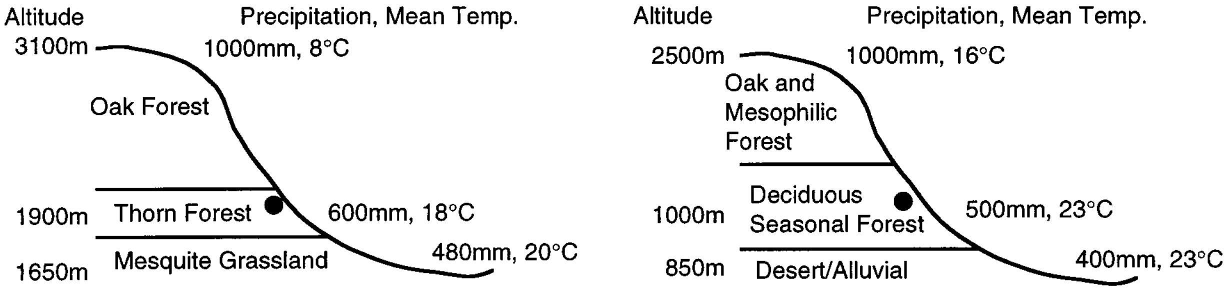

FIG. 3. *Idealized representation of ecological zones near Guilá Naquitz* (left) *and Coxcatlán* (right). *The black dot indicates the position of the cave relative to the ecological zones. The deciduous seasonal forest contains thorn forest and mesquite grassland facies. Ecological and environmental data are summarized from several sources (Byers 1967*a*, Kirkby 1973, Rzedowski 1983, Kirkby, Whyte, and Flannery 1986).*

TABLE 2
Ecological Characteristics of Plants Remains at Guilá Naquitz and Coxcatlán Caves

Species[a]	Site[b]	Frost[c]	Mesic[d]
Agave spp. (maguey)	G, C	Tolerant	−
Ceiba parvifolia (pochote)	C	n.d.	−
Celtis sp. (hackberry)	G	+*	−*
Condalia mexicana	C	n.d.	−
Cyrtocarpa procera (*chupandilla*)	C	n.d.	−
Jatropha neopauciflora	C	n.d.	−
J. neodiocia (*susi* nuts)	G	+	−
Lemaireocereus hollianus	C	n.d.	−
Leucaena sp. (*guaje*)	G	*	+
Malpighia sp. (*nanches*)	G	*	*
Opuntia sp. (cacti)	G, C	−	−
Persea americana (avocado)	C	n.d.	+
Phaseolus sp. (beans)	G	−	+
Pinus sp. (piñon nuts)	G	+	+
Prosopis juliflora (mesquite)	G, C	−	+[e]
Quercus sp. (acorns)	G	+	+
Setaria cf. *macrostachya* (grain)	C	n.d.	+[e]
Sideroxylon cf. *tempisque* (*cosahuico*)	C	n.d.	−
Spondias mombin (ciruela)	C	n.d.	+

[a] Ecological characteristics were extracted from many ecological studies: general flora descriptions, Flannery (1967), Pennington and Sarukhan (1968), Rzedowski (1983), Kirkby, Whyte, and Flannery (1986), Smith (1986); *Agave* and *Opuntia,* Gentry (1982), Russell and Felker (1987), Nobel (1988); *Jatropha,* Yeaton and Manzanares (1986); Dehgan and Schutzman (1994); *Leucaena,* Standley (1922), Brewbaker and Hutton (1979); *Persea,* Byers (1967*c*), Levinson and Adato (1991); *Phaseolus,* de Souza Lima et al. (1986), Wolfe (1991), Foster, Pajarito, and Acosta (1995); *Prosopis,* Scifres and Brock (1972), Felker et al. (1982), Stromberg et al. (1993); and *Setaria,* Callen (1967), Tiedemann, Klemmedson, and Ogden (1971). The asterisk indicates that the identification was only to generic level and this prevented a reliable estimate of the taxon's ecological characteristics.

[b] Where these plant remains were most common: Guilá Naquitz (G) and/or Coxlatán (C).

[c] +, frost-tolerant; −, frost-sensitive; n.d., not determined. (Frost-tolerance was not determined for plants in the warm Tehuacán Valley.)

[d] +, mesic; −, xeric.

[e] *Prosopis* is a phreatophyte; *Setaria* is not a phreatophyte but is favored by associating with *Prosopis.*

1993). *Setaria* exploits groundwater through a commensal relationship with *Prosopis* (Tiedemann, Klemmedson, and Ogden 1971); it benefits tremendously from the groundwater that *Prosopis* provides to the upper soil layers through hydraulic lift and from nitrogen fixation (Tiedemann and Klemmedson 1973, Dawson 1993).

We made predictions as to how the various ecological groups would have reacted to the paleoclimatic changes at Guilá Naquitz and Coxcatlán without human interference. At Guilá Naquitz (table 3), frost-tolerant plants should have fared well in the cold late Pleistocene (zone D) and frost-intolerant ones during the early Holocene (zone B). Modifying this basic model are changes in precipitation, with drier conditions early (zone D) followed by more mesic conditions later (zone B). The xeric plants with low frost tolerance would have followed these trends in conflicting ways; therefore we suggest that they were dominant in the intermediate time period (zone C). At Coxcatlán the most dramatic changes involved precipitation and groundwater levels, with more moisture during the early Holocene and less during the middle Holocene. From this we predict (table 4) that mesic species should be closer to the cave during the wetter periods around 7,700 B.P. and xeric species more prevalent during the drier period around 5,700 B.P. The phreatophyte species should have had a more expansive range early on, as the water table was higher.

ARCHAEOLOGICAL CHANGES IN ABUNDANCE

The abundance of the most important plants at Guilá Naquitz changed substantially through time (see fig. 4). *Quercus, Pinus,* and *Jatropha* were all prevalent in the late Pleistocene but declined in the early Holocene, which is consistent with their cold-tolerances. In contrast, *Celtis* exhibited little change between phases and in general did not reflect the changes in temperature; perhaps its xeric character was also important for its success, which might explain why its pattern is similar to that of *Agave* and *Opuntia. Phaseolus* and *Prosopis* both exhibited major increases in the early Holocene, perhaps reflecting their preference for the warm conditions of this phase. *Agave* and *Opuntia* showed little change over time but were most abundant in zone C. Xeric species appear to have done well in this transitional period.

The Guilá Naquitz abundance changes correlate well with the environmental predictions (table 3). Within each ecological group there is a good correlation among members, which indicates that the abundances of plants with similar ecological traits varied together through time. Since zone C had less definite ecological predictions and a significant proportion of the remains were closely associated with rejected radiocarbon dates, we also tested the correlation with zones D and B only. Zones D and B also correlated extremely well with the environmental predictions.

At Coxcatlán the abundances of the most important plants also changed tremendously (fig. 5). The mesic species, *Spondias* and *Persea,* that were abundant in the wetter early Holocene became rare in the drier middle Holocene. The phreatophyte *Prosopis* and the commensal *Setaria* exhibited a steady decline throughout the period, which may reflect the gradual decline of the groundwater in the region. The xeric species, including *Cyrtocarpa, Condalia, Ceiba, Lemaireocereus, Opuntia, Sideroxylon,* and *Agave,* were much less abundant in the early Holocene than in the dry middle Holocene. However, *Jatropha neopauciflora* did not follow the pattern of increased abundance in the middle Holocene. This could reflect the sampling variance, a lack of understanding of its ecology, or the arbitrary division of time periods (93% of its remains from the El Riego

TABLE 3
Guilá Naquitz Correlations between Plant Distribution and Environmental Ranking

	Stratigraphic Zone				Average Correlation with Environment	
Ecological Group	D (10,295 B.P.)	C (9,365 B.P.)	B (8,790 B.P.)	Average Correlation within Groups	All Data	Only Zones D and B
Frost-tolerant, mesic	1[a]	2	3	1.00	1.00	1.00
Frost-tolerant, xeric	1	2	3	0.50	0.75	1.00
Frost-intolerant, xeric	3	1	2	1.00	1.00	1.00
Frost-intolerant, mesic	3	2	1	1.00	1.00	1.00
Average				0.88[b]	0.94[c]	1.00[c]
P				0.013[b]	0.000[c]	0.010[c]

[a] Predicted abundances were estimated from the paleoclimate, e.g., frost-tolerant mesic species should be most abundant in zone D and least abundant in zone B.
[b] Mean of group correlations; *P* value determined by shuffling each species's zone samples.
[c] Mean of species correlations; *P* value determined by shuffling each species's zone samples.

phase were found in the stratum adjacent to the Coxcatlán phase).

In general, the abundance changes of Coxcatlán's plant remains correlate well with the predicted environmental changes (table 4). Within each ecological group there is good correlation among members, which indicates that the abundances of plants with similar ecological traits varied together through time.

Subsistence plant remains at both Guilá Naquitz and Coxcatlán exhibited similar correlations with environmental changes and within ecological groups. The correlation of abundance with environmental change suggests that humans were generally exploiting the most abundant plants in their immediate surroundings. The correlation of abundance among species within ecological groups suggests that the humans were exploiting plants with similar ecological preferences. The most parsimonious explanation for these results is that plants were not being cultivated.

Flannery has interpreted these data differently. He rejects a model in which climate change drives variations in plant exploitation (Flannery 1986*a*), viewing climate change as moving entire ecological zones rather than adjusting their species content. Within this framework, he notes the conflict between the increase in *Prosopis* and the concurrent decrease in *Celtis.* However, our ecological analysis suggests that the arid-adapted frost-tolerant *Celtis* would respond differently from the frost-intolerant phreatophyte *Prosopis.* Flannery's preferred interpretation is that "the percentage fluctuations we see have to do with changes in harvesting strategy over time" (Flannery 1986*a*:316). To test their harvesting-strategy theory, Reynolds (1986) and Flannery develop an excellent model and simulation for adaptive change

TABLE 4
Correlations between Plant Distribution and Environmental Ranking

	Stratigraphic Zone			
Ecological Group	XXVI–XIV (7,669 B.P.)	XIII–VIII (5,519 B.P.)	Average Correlation within Groups	Average Correlation with Environment
Mesic	1[a]	2	1.00	1.00
Phreatophytic	1	2	1.00	1.00
Xeric	2	1	0.50	0.75
Average			0.83[b]	0.83[c]
P			0.009[b]	0.004[c]

[a] Predicted abundances were estimated from the paleoclimate, e.g., mesic species should be more abundant in zones XXVI–XIV and less abundant in zones XIII–VIII.
[b] Mean of group correlations; *P* value determined by shuffling each species's zone samples.
[c] Mean of species correlations; *P* value determined by shuffling each species's zone samples.

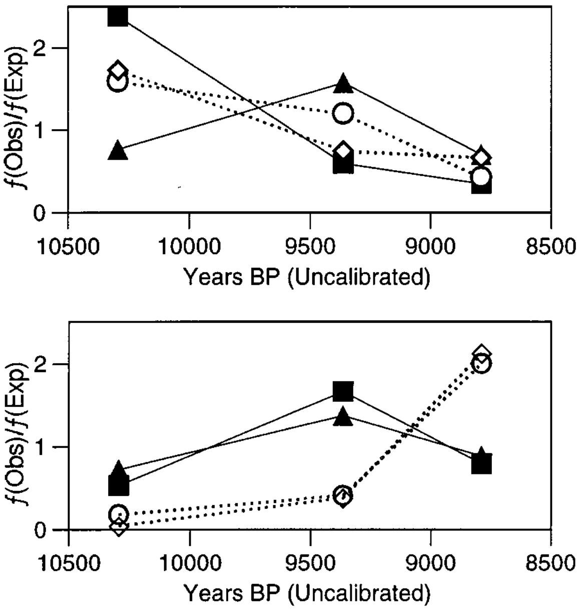

FIG. 4. *Observed-to-expected ratios of abundance of plant remains for zones D, C, and B at Guilá Naquitz Cave.* Top, *frost-tolerant: semimesic* (dotted line), ◇, Quercus *sp.*, ○, Pinus *sp.; xeric* (solid line), ▲, Celtis *sp.*, ■, Jatropha dioica. Bottom, *frost-sensitive: mesic* (dotted line), ◇, Prosopis juliflora, ○, Phaseolus *sp.; xeric* (solid line), ▲, Agave *spp.*, ■, Opuntia *sp.*

that stresses trial and error, learning, memory, and microclimatic variability. Unfortunately, this model makes poor predictions for the two most important and abundant plants (Reynolds 1986). For *Quercus,* the model predicts an 80% increase in collection tasks through time, but *Quercus* remains decline in relative abundance roughly threefold. For *Prosopis,* the model predicts a 23% increase in collection tasks, while *Prosopis* remains indicate a 3,780% increase. These changes can be explained by our climatic model. We have no doubt that shifts in harvest strategies were responsible for some of the patterns of change, but future models should account for the redistribution of plant species caused by climate.

IMPLICATIONS FOR DOMESTICATION

The conventional view suggests that several important domesticates were first cultivated and domesticated at Guilá Naquitz or Coxcatlán (table 5). Smith (1967) argues that the local plants *Persea, Prosopis, Agave, Opuntia, Setaria, Spondias, Sideroxylon,* and *Cyrtocarpa* were cultivated at Tehuacán. In contrast, on the basis of recent archaeological dating, biogeographic domestication studies, and this climatic model, we argue that little cultivation or domestication occurred at Coxcatlán or Guilá Naquitz. Most of these species exhibit no morphological signs of domestication by the mid-Holocene, and the changes in distribution over time, used by Pearsall (1995) to argue for subsistence shifts in the early stages of domestication, can now be explained more parsimoniously in terms of climate change. The early remains of *Capsicum* (pepper), *Lagenaria* (bottle gourd), *Phaseolus* (bean), and *Zea* (maize) are interesting, but direct AMS dating suggests that some of the samples may be intrusive (Smith 1995). The small number of remains makes stratigraphic intrusion a strong possibility. Guilá Naquitz's few AMS-dated *Cucurbita pepo* (squash) remains show signs of domestication by 8,900 B.P. (Smith 1997), but biochemical biogeographic analyses indicate that the Tehuacán and Oaxaca regions are not the regions of origin for *Zea mays* ssp. *mays, Capsicum annum, Cucurbita pepo,* or *Phaseolus acutifolius* (table 5).

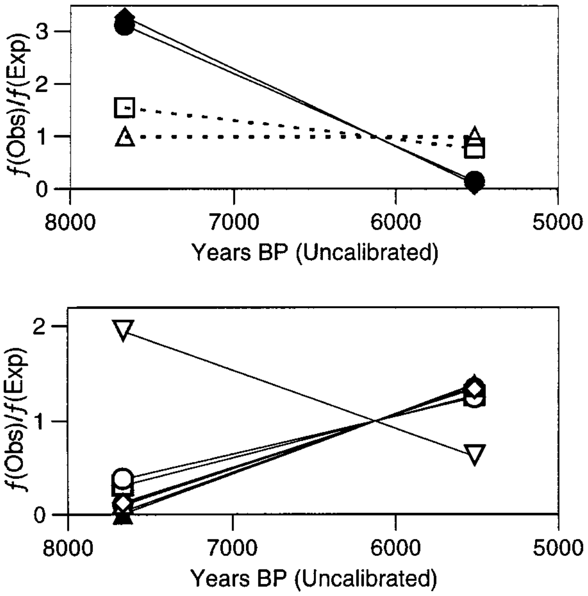

FIG. 5. *Observed-to-expected ratios of abundance of plant remains for zones XXVI–XIV and XIII–VIII at Coxcatlán Cave.* Top, *mesic and phreatophyte: mesic* (solid line), ◆, Spondias mombin, ●, Persea americana; *phreatophyte* (dotted line), △, Prosopis juliflora, □, Setaria macrostachya. Bottom, *xeric:* ▽, Jatropha neopauciflora; ◇, Agave *spp.*, Cyrtocarpa procera, Sideroxylon tempisque; ▲, Condalia mexicana, Ceiba parvifolia; ❍, Opuntia *sp.;* □, Lemaireocereus hollianus.

Persea is in fact the mostly likely local domesticate at Tehuacán (Smith 1966, 1969, 1987), as it is found in moderate quantities there and the plant remains include both wild and domesticated seeds. A full progression of seed sizes is seen at Tehuacán with a large jump during the Santa Maria phase (2,900–2,200 B.P. [Smith 1967]). Cultivation would have been necessary to maintain *Persea* near Coxcatlán Cave, especially by the mid-Holocene, and it was probably undertaken to retain this favored food item despite climate change that reduced the availability of wild stands.

Domesticated maize appears in Tehuacán only by

TABLE 5
Possible Domesticates at Guilá Naquitz and Coxcatlán Caves

Species	Site	Dating (B.P.)[a]		Remains	*f*(% of Total)	Region of Origin[b]	Notes on Archaeological Remains
		Stratigraphic	Direct AMS				
Capsicum annum	Coxcatlán	8,600–6,750	–	8	1.07	Northeastern quarter of Mexico	Domestication status not established
C. annum	Coxcatlán	7,050–4,050	–	12	0.15	Northeastern quarter of Mexico	One sample possible domesticate
Cucurbita pepo	Guilá Naquitz	10,750–8,670	8,990–6,980	17	0.10	Northeastern Mexico	Domestication under way by 8,900 B.P.
Cucurbita sp.	Guilá Naquitz	10,750–8,670	–	77	0.45	Native	–
C. mixta	Coxcatlán	6,750	–	2	0.27	Lowland Mexico and Central America	Domestication status of pre-5,000 B.P. samples questioned
C. mixta	Coxcatlán	7,050–4,820	–	6	0.08		
Lagenaria siceraria	Guilá Naquitz	9,450–8,870	–	7	0.06	Brazil and Africa	–
L. siceraria	Coxcatlán	7,050–4,050	–	9	0.11	Brazil and Africa	–
Persea americana	Guilá Naquitz	8,670	–	2	0.03	Mexico	Probably intrusive
P. americana	Coxcatlán	8,600–6,750	–	32	4.27	Mexico	Seed size increases at roughly 2,500 B.P.
P. americana	Coxcatlán	6,975–4,050	–	39	0.50	Mexico	
Phaseolus Type 1	Guilá Naquitz	10,750–8,670	–	310	1.80	Native?	–
P. acutifolius	Coxcatlán	5,200–4,820	–	118	1.50	Northwestern quarter of Mexico	Date questioned
P. vulgaris	Guilá Naquitz	10,750–8,670	–	4	0.02	Mexico	Probably intrusive
P. vulgaris	Coxcatlán	6,375–4,820	2,285	2	0.03	Mexico	–
Zea mays ssp. *mays*	Coxcatlán	7,050–4,050	4,700–450	115	1.47	West Central Mexico	–

SOURCES: *Capsicum,* Loaiza et al. (1989); *Curcurbita,* Wilson (1989), Nee (1990), Decker et al. (1993), Smith (1997); *Lagenaria,* Heiser (1989); *Persea,* Smith (1966); *Phaseolus,* Schinkel and Gepts (1988, 1989), Becerra and Gepts (1994), Garvin and Weeden (1994); *Zea,* Doebley (1990), Buckler and Holtsford (1996).

[a] Stratigraphic dates are the entire range of ^{14}C dates associated with the plant remains either at Guilá Naquitz Cave, the El Riego phase of Coxcatlán Cave, or the Coxcatlán and Abejas phases of Coxcatlán Cave.

[b] Reconstructed region of origin is based on biogeographical and biochemical data if available.

4,700 B.P. (Long et al. 1989). Is this a reflection of a very recent domestication (Fritz 1994), or were these semiarid regions late in the adoption of domesticated maize? Maize (*Zea mays* ssp. *mays*) was domesticated from western Central Mexican populations of annual teosinte (*Zea mays*), as is indicated by all molecular systematic evidence (Doebley 1990, Buckler and Holtsford 1996); wild maize theories (Mangelsdorf 1986, MacNeish 1992) have been refuted. The date of this domestication event has yet to be established, but three points suggest that maize domestication occurred well before its appearance at Tehuacán. First, the high levels of molecular differentiation suggest an early divergence (Doebley 1990, Buckler and Holtsford 1996). Second, molecular evolution studies indicate that the founder population of maize was large (Gaut and Clegg 1993), which would have extended the time necessary for domestication (perhaps several thousand years). Third, there is good phytolith evidence for maize in Panama and Ecuador by 7,000 B.P. (Piperno et al. 1985, Pearsall and Piperno 1990). Together these observations suggest that maize domestication had its beginnings in Central Mexico (probably in Guerrero) before 9,000 B.P. Given the much cooler temperatures in Mexico before 10,000 B.P., we postulate that the teosintes would have had their ranges depressed by 1,000 m, which would have placed maize's teosinte ancestor in the lowlands of Guerrero. We speculate that domestication may have begun among coastal people cultivating teosinte to maintain populations near the coast. This theory suggests that the semiarid highland regions of Mexico were late in the adoption of maize agriculture.

Cucurbita pepo and *Lagenaria siceraria* have been recovered from Guilá Naquitz and Coxcatlán (Cutler and Whitaker 1967, Whitaker and Cutler 1986). The change in *C. pepo*'s seed size has reasonably been attributed to early Holocene human selection and domestication (Smith 1997), but it may also result from the plant's response to the favorable climate of the early Holocene. From the locations of related species and feral populations it seems that *Lagenaria* probably originated in Africa and/or Brazil (Heiser 1989). Considering other developments in Brazil (Roosevelt et al. 1991), it may be the first domesticate in the New World. Wetter early

Holocene climatic conditions would have been favorable for the cultivation of *Lagenaria* and for growth of *Cucurbita* at Guilá Naquitz. The geographic origins of both of these possibly early domesticates should be further investigated.

Although a wild *Phaseolus* species was an important subsistence item at Guilá Naquitz, there is no strong evidence for its cultivation or domestication (Kaplan 1986). The domesticated *P. acutifolius* was abundant in one of the Abejas zones, but the dates are questionable; *P. acutifolius's* drought-tolerance would have been adaptive during this arid time. The nature of Tehuacán's *Capsicum* is difficult to discern (Smith 1967), but the more mesic climate of the early Holocene would have been conducive to wild *Capsicum* growth near Coxcatlán Cave.

To conclude, the correlations with the environment suggest that humans at Guilá Naquitz and Coxcatlán were more passive participants in the local ecosystem than previously thought. Virtually none of the early Holocene plant remains found at Guilá Naquitz (0.18%, including possible intrusives) and at Coxcatlán Cave in the El Riego phase (0.00%) are cultivars. Only during the mid-Holocene dry phase do cultivars and domesticates become more important at Coxcatlan Cave (4.35%). We suggest that increasing aridity provided the impetus for cultivation in the Tehuacán Valley, but the most important cultivars had already been domesticated by the time of the shift; this implies that the semiarid Tehuacán Valley was a backwater for subsistence technology. While the best-reported and most extensive archaeological sites in Mexico occur in these semiarid regions, this ecological zone was unimportant in the domestication of most Mexican crops.

CONCLUSION

Central Mexico experienced substantial climatic variation from the Pleistocene through the Holocene. Before 10,000 B.P. it was colder and slightly drier, while from 9,000 to 6,500 B.P. it was wet and warm and, finally, between 6,500 and 4,500 B.P. it was dry. These changes would have significantly modified plant distributions. Human plant exploitation from 11,000 to 4,000 B.P. is correlated with the changes in plant-community composition predicted from climate changes. Future empirical and theoretical studies on cultivation and domestication in the tropics must take these significant paleoenvironmental changes into account.

Previous research in Central Mexico failed to account for climate changes and therefore ascribed changes in plant exploitation to new harvesting strategies or to cultivation. The correlations with climate suggest that the inhabitants of Guilá Naquitz and Coxcatlán were predominantly foraging on the most abundant local plants. Cultivation appears to have been insignificant, with only a limited start during the mid-Holocene dry phase. None of the major New World crops appear to have been important for subsistence in Tehuacán and Oaxaca. The search for the origins of domestication should instead focus on the coastal escarpment regions of Mexico.

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The Barí Partible Paternity Project: Preliminary Results[1]

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For a century, studies of human sexual behavior and its evolution have emphasized the importance of paternity certainty and the lengths to which men will go to ensure their paternity of the children ascribed to them (e.g., Engels 1942 [1884], Alexander and Noonan 1979). In this context, it is a curious and important ethnographic finding that a substantial number of societies appear to go to some lengths to confuse the issue of paternity.

All cultures recognize a distinction between the social father (pater) and the biological father (genitor) of a child (Radcliffe-Brown 1950). However, a substantial number of cultures, in addition to recognizing this contrast, *also hold that it is possible to have several biological fathers.* This belief is particularly common in lowland South America (Crocker and Crocker 1994:83; Hill and Kaplan 1988:298–99; Henry 1941:45; E. Reichel-Dolmatoff, personal communication), although there are sporadic reports of superficially similar or behaviorally equivalent beliefs elsewhere (e.g., Bledsoe 1980:93). In lowland South America, the core belief is that all the men who have intercourse with a woman during her pregnancy share the biological fatherhood of her child. The woman's husband, if he cohabited with her during pregnancy, is usually considered the primary biological father. The lovers are secondary fathers.

THEORETICAL BACKGROUND

Both Hrdy (1981, 1988) and Hawkes (1990, 1991) have proposed models indicating how females may, under some conditions, increase their reproductive success by "confusing" the paternity of their children. Hrdy (1981: 174) focuses on the survival advantages accruing, in primate society in general, to offspring of uncertain paternity:

> If we recognize that a female's reproductive success can depend in critical ways on the tolerance of nearby males, on male willingness to assist a nearby infant, or at least to leave it alone, the selective importance of an active, promiscuous sexuality becomes readily apparent. Female primates influence males by consorting with them, thereby manipulating the information available to nearby males about possible paternity. To the extent that her subsequent offspring benefit, the female has benefitted from her seeming nymphomania.

However, Hrdy (1988:123) specifically asserts that these arguments are inapplicable to modern humans: "Certainly I would not argue that promiscuity is currently adaptive for women anywhere in the world."

Hawkes (1990) takes an opposing view as to the current adaptiveness of a woman's mating with multiple men. She emphasizes frequency-dependent strategies of mother and father. She divides men into "provisioner" and "showoff" types. The former "feeds himself a bit and then procures a steady income for his wife and children," while the latter "seeks resources which he often fails to capture but which give him occasional bonanzas big enough to feed more than a family" (Hawkes 1991: 155–56):

> The value of the occasional large packages brought by the showoffs leads other adults to act towards them in ways which increase the chances that they will remain with the group and continue to supply extra food to all. Women extend them sexual favors, increasing the likelihood of their continual proximity and so of occasional treats for themselves and their children. Men tolerate this pattern of infidelity

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> because of periodic food contributions. . . . All adults may defer more to the showoffs' children than to the children of men who feed only their own families. . . . Under these circumstances the preferred arrangement for a woman would be marriage to a provisioner . . . while other men in the band behaved as showoffs so that she and her children could take additional shares from them. To the extent that band members gave special protection to the children of showoffs, a woman would do best if she could have the regular income of a provisioner with the paternity of all her children assigned to showoffs.

ETHNOGRAPHIC BACKGROUND

The Barí Partible Paternity Project was designed to explore the occurrence and consequences of secondary fatherhood among the Barí of Venezuela, using this ethnographic case to test the proposition that "paternity confusion," as it is labeled in the literature, is adaptive for present-day women in this cultural context. As far as we are aware, the only other investigation of the adaptive effects of secondary fatherhood was part of research carried out among the Aché by Hill and Hurtado (1996:274–75, 465). Their results are similar to ours.

Final analysis of all our Barí data is expected to take some time. Nevertheless, we report here our most striking early results in the hope that two reports on the partible-paternity phenomenon, one on the heels of the other, will stimulate parallel research among other peoples of South America. Relevant secondary-fatherhood and mortality data pertaining to the conditions under which the complex of secondary-fatherhood behaviors developed are rapidly becoming unavailable, and time is of the essence in investigating this phenomenon.

The Barí, inhabitants of the southwesternmost lobe of the Maracaibo Basin (a region divided by the border between Colombia and Venezuela), are a Chibchan-speaking people of the lowland tropical rain forests of South America (Beckerman 1991). Typically for lowland tropical South America, the Barí's staple crop is manioc; they obtain the protein in their diet by fishing and hunting. As pointed out by Dufour (1992, 1994), a manioc-based diet, supplemented by fish and game, is usually ample for adult males but may be marginal for small children because the low nutrient density of manioc may not allow the packing of enough food into a child's gut to provide adequate nutrition. Similar problems may arise for pregnant and lactating women. Maintaining a steady supply of a sufficient level of animal protein (and fat) is important to avoid protein-calorie and micronutrient malnutrition.

The traditional Barí sexual division of labor was robust and exhaustive. Men cleared the fields and planted the first crop; women predominated in the rest of horticulture, did all the food preparation, and participated in fishing and hand-caught some small fish. The spearing of fish was a male monopoly, and these fish belonged to the man who speared them. Hunting was exclusively male.

Marriage normally took place at about age 15 for females and 20 for males. Around the time of modern peaceful contact (see below), about 15% of men were polygynous at some time in their lives; women married only one man at a time (Zaldívar, Lizarralde, and Beckerman 1991). Local-group endogamy was preferred but, given the small size of the local group, was often impossible. Territorial-group endogamy was the fallback preference; it was achieved by most Barí (Lizarralde and Lizarralde 1991).

A high frequency of widowing and remarriage resulted from a high death rate of reproductive-aged adults due to disease (malaria was endemic) and to chronic war—the ongoing attempts by the region's landowners, oil companies, and homesteaders to exterminate the Barí. These criollos sometimes hired bands of professional Indian-killers and sometimes formed such bands themselves. Their most common tactic was to sneak up to a longhouse just before dawn, set it afire, and gun down the occupants as they fled. When they found Barí on the trail, before they reached a longhouse, they opened fire at once. The most common victims, both on the trail and in the longhouse, were adult male Barí (Beckerman and Lizarralde 1995). This situation obtained from the middle of the 19th century until modern peaceful contact with the Barí was achieved on July 19, 1960. It is in this context that the secondary-fatherhood data must be understood.

Life-history interviews with elderly Barí reveal that most women, in precontact times, took one or more lovers during at least one of their pregnancies. In the great majority of cases, the married woman stated that she took a lover only after she was pregnant. Their husbands were usually aware of the lovers, and there is no evidence that the husbands objected. When a woman gave birth, she typically named all the men who had had intercourse with her during her pregnancy. One of the women attending the birth then returned to the longhouse and announced to each of these men, "You have a child." These secondary fathers had recognized obligations to the child. Importantly, they were supposed to provide gifts of fish and game.

Interviews with middle-aged and elderly Barí indicate variable compliance with these expectations of meat provisioning. A man in his early 50s stated that his secondary father never gave him food gifts because he was living not in the same longhouse but far away. A woman in her middle 50s, in contrast, reported that the secondary father of one of her daughters regularly brought food gifts for that girl. When he returned from a fishing expedition, he would give half the catch to her (the mother) and half to his own children. The mother would share the extra fish with all her children. This secondary father stopped giving food when the daughter became an adolescent. The secondary fathers of two other children of this woman did not give food to these children. A woman in her late 60s reported that her secondary father (who was her father's brother) brought her food until she married. When he brought a lot of food, he would tell her to share with the other members of her hearth group; when he brought only a single fish or a squirrel, he would give the meat to her alone.

METHODS

The portion of the Barí Partible Paternity Project interview protocol relevant here begins with a genealogical inquiry, taking the informant as far back in her ancestry as she can go (usually to her grandparents) and then tracing down from these apical ancestors to identify her aunts, uncles, and first cousins as well as her siblings. Then a marriage and reproductive history of the informant is recorded, with all pregnancies noted, miscarriages as well as live births. Children still living are identified by name, current residence, and current spouse, if married. Birth dates are known—sometimes to the month, sometimes to the day—for most children born after July 1960. Birth dates for people born before then are estimated by a variety of means and assigned a reliability code indicating the probable magnitude of error. The age at death of deceased children born in precontact times is estimated by asking the informant to point out a living child who is of approximately the same age as the dead child at his or her time of death. Finally, after we have a birth-ordered list of all the informant's pregnancies including miscarriages, we ask, for each pregnancy, where the birth or miscarriage took place and whether there was a secondary father involved. If there was, we request his identity also. In some cases, women have been able to provide these marriage and reproductive-history data for their dead mothers and/or sisters as well as for themselves.

We are aware of the various methodological difficulties associated with using recall data and with assigning birth dates and ages to people for whom in many cases there is no written record of birth. In particular, since we deal below with survivorship to the age of 15, we are acutely aware of the likely misclassification of some older children who died before contact. Certainly there are errors in the data set. Nevertheless, because the protocol assigns age at death before the presence or absence of a secondary father is ascertained, we have been unable to conceive of any way in which there could be *systematic* errors distorting age at death with respect to the existence or nonexistence of a secondary father for a child.

Research is near the end of the data collection phase. We now have complete secondary-father data for 111 postreproductive women (74 of whom took a secondary father for at least one of their pregnancies) and their 897 known pregnancies, 23% of which involved secondary fathers. We anticipate obtaining new reproductive histories from a few more women in future fieldwork and completing some reproductive histories that we are currently unable to use because secondary-father information is missing for one or more children; we are in the process of cross-checking all information against censuses and genealogical notes taken over the decades before the project began. This work will take a year or more and will surely produce minor alterations in the data base. We do not foresee a change in the direction or significance of the results already obtained.

PRELIMINARY ANALYSIS

We are primarily concerned here with the possible survival advantages accruing to children with secondary fathers; we examine survivorship to age 15. From the 897 known pregnancies, we remove for this preliminary analysis 65 living children who were born less than 15 years ago and 10 dead children whose ages at death have not yet been established to our satisfaction. Of the remaining 822 pregnancies in the sample, 560 reached age 15, for an overall survivorship of 68%.

Survivorship to age 15 was significantly different between children with secondary fathers (156 out of 194, or 80%, survived to age 15) and children without secondary fathers (404 out of 628, or 64%, survived to age 15). This difference corresponds to an odds ratio of surviving to age 15 for a child with a secondary father of 2.28, $p < .001$ by the large-sample Mantel-Haenszel chi-square test (one tail).

These results reveal a case in which matings with multiple men during a single pregnancy are "currently adaptive" in the strict sense for these Barí women: Multiple matings during a single pregnancy increase the survivorship of the resulting children, who, in benefiting from this "paternity confusion," increase their mothers' reproductive success and (presumably) their mothers' fitness. We tentatively attribute the increased survival of children with secondary fathers to improved nutrition.

When we divide the database children into three categories, more interesting differences appear. The three applicable categories are group 1, children who have secondary fathers; group 2, the brothers and sisters of group 1, children who do not have secondary fathers themselves but who do have at least one sibling with a secondary father; and group 3, children with no secondary fathers in the sibling set (i.e., children whose mothers never took a secondary father during any of their pregnancies).

Among these three categories, some differences are highly significant and others are not. In group 1, as reported above, 80% of the children survived to age 15. However, only 61% (230 out of 376) of their siblings (group 2) survived to age 15. The odds ratio associated with survival to age 15 and having secondary fathers within the sibship is therefore 2.59, significant at $p < .001$, indicating that children with a secondary father were more than two and a half times more likely to survive to age 15 than their brothers and sisters without a secondary father. Group 3 children had intermediate survivorship; 174 out of 252 (69%) of them survived to age 15. The odds ratio for a comparison of group 1 and group 3 is 1.83 (significant at $p < .01$), and the odds ratio comparing group 2 and group 3 is 0.72 (significant at $p < .05$).

When this research began, we predicted that siblings of children with secondary fathers would receive some of the benefits of the food supplied to their brothers and sisters by the secondary fathers of the latter, since for small children in particular, gifts of fish and game to the child are typically handed over to the mother, who can then cook the food and distribute it among all her children. This prediction having been shown to be wrong (group 2 children have lower survivorship than group 3 children), we currently suspect that the most important benefit of extra animal protein may be derived not from large amounts of fish and game conspicuously donated and widely shared in times of abundance but, as was mentioned by one of our informants, from small increments given to the focal child alone at moments of scarcity. It may be that the crucial role of a secondary father is to provide not abundance but regularity of animal protein supply. Future fieldwork will explore this possibility.

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