POLLEN STUDIES OF THE OAXACAN ARCHAIC: PRELIMINARY STATEMENT

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This statement is written simply to give others on the project the befefit of my existing notions. It does not constitute a legitimate report for many reasons. First, all the data is not yet in. I still have samples from Cueva Blanca and Guila Naquitz that will be processed to determine whether or not pollen counts are possible. Second, I am not presenting the evidence that will allow evaluation of my statements. Third, these proposals have yet to be systematically considered in light of the various pollen records from Formative and later horizons in the Valley. Fourth, these proposals are evidently premature until they can be viewed in light of the macrofossil plant remains information and in the light of paleoecological reconstructions that may be independently offered from analysis of the faunal, geologic-geomorphologic, and cultural evidence.

But we've got to start somewhere, and I feel that enough data has been garnered to start with the pollen. While my statements should be taken with adequate dosage of salt, they may prove to offer some insights into the kinds of interpretations that other forms of evidence could yeild independently. I cannot overemphasize the tentative nature of these "conclusions" from the pollen record. I have no vested interests in them, and no member of the project should feel the least hesitancy in challenging any of them. If they cannot withstand challenge from friendly quarters, they certainly will not stand up to attack from hostile ones.

I have made two assumptions in my interpretations which will definately be challenged by paleoecologists: (a) that the existence of human groups in Oaxaca has had no evident affect on the pollen records of either ancient or modern sediment samples; and (b) that the differences in vegetation patterns reconstructed for various points in time are due to fluctuations in climatic, rather conditions. I feel that the archaeological record of the Oaxacan Archaic illustrates significantly small human populations who are dependent on the natural products of the

the landscape. I see no cultural evidence that would justify a presumption that these peoples systematically or even inadvertantly precipitated significant alterations in the natural vegetation of the time. It will be argued that such people could have changed the vegetation easily and in any number of ways, as by accidental fire. But I contend that this argument is specious without positive support, and that there is no a priori justification for challenge of assumption (a) in regard to the Archaic Horizon. There is, of course, a priori justification for such a challenge in regard to the Modern Horizon. There are many people now living in the Valley and they alter the landscape daily and in many and various ways, many of which have effects on the natural vegetation. This statement also presumably applies to post-Archaic and pre-Modern time periods. But I can demonstrate that surface pollen records from specific vegetation associations do yeild consistant, patterned, results. This consistancy seems to occur despite man's variable impact on the landscape under modern conditions when it is least expectable. I thus feel pretty confident that man has no "evident effect" on modern pollen records. Since there is as much or more landscape alteration today as can be reasonably postulated for the post-Archaic- pre-Modern period (given plow technology, metal tools, and population density), I see no reason to maintain that pollen records from such periods reflect human activity to any significant degree.

Assumption (b) is stickier. I would very much like to be able to leave the door open to other determinants than climate as a simple matter of scientific hedging. Beyond that, there are good theoretical reasons relating to pollen dispersion and pollen preservation which signal cautious advance in regard to the questions of causality of the data. But I'm afraid that in this case opening the door a crack will have the same effect as unlocking Pandora's Box. If we grant multiple causality at the outset we're likely to get lost in a maze of quasi-educated guesswork and end up with no meaningful reconstructions at all! For the time being, at least, I'm pretending to be much more bold that I wish I had to be. I'll assume a climatic causality until evidence turns up to indicate that I should not.

The surface sample pollen records now number about 25, most of which have been replicate-counted by both Kitchen and myself. Without going into details, I feel that we can now tell a good deal about the vegetation pattern by looking at its surface pollen sample.

(1) The sample can be identified as to forst structure, savanna structure, or open structure.

(2) The sample can be identified as to Pine Zone, Oak Zone, Thorn Forest Zone, or Desert Grassland Zone. By this means we obtain an index to the variety of plant taxa which are most prevelant even though these taxa are not necessarily represented by pollen directly. For example, common taxa of the Thorn Forest Zone are leguminous, cactaceous, and malphigiaceous shrubs. These are rare in the pollen spectra. But all Thorn Forest samples have higher frequencies of Compositae pollen than samples from non-Thorn Forest locales. Compositae pollen in high frequency thus may be considered an "index fossil" of Thorn Forest floristics.

(3) The relationship between pollen taxa frequencies and temperature and moisture values indicated by floristics at the sample locality can be worked out in a rough way. We get significantly greater amounts of oak pollen, for example, only when we have significant growth of narrow-leaf oak (encino) and no local pines or quantities of Thorn Forest shrubbery. I think the presence of quantities of narrow-leaf oak is a good index to temperature-moisture values. This tree does not seem able to compete effectively against pine or broad-leaf oak where moisture values are high, but it can tolerate drier habitats than they. Alternatively, encino does not seem able to compete against Thorn Forest shrubs when temperatures are high despite its tolerance for aridity. Narrow-leaf oak in any quantity thus seems a good index to a locale too cool for Thorn Forest and too dry for Pine Forest. High oak pollen values in an ancient sample at an elevation where we now have Thorn Forest would thus be an indication of pre-existing cooler temperatures but no relative change in moisture. A moisture increase accompanying the temperature decrease would more likely result in pollen frequencies typical of the Pine Zone--i.e. low oak pollen values.

I readily admit I'm on very shaky ground in making interpretations such as the one just presented. But more palynological interpretations are made without the justifications offered by surface samples than are made with them. From the pollen analyst's point of view, the interpretations I am presenting are incredibly more sophisticated than the usual wholly uncontrolled guess-work. I could certainly use another 400 surface samples upon which to base my case. But 25 samples are 25 more than have ever been used before in the interpretation of fossil pollen records in Mexico. There is no question, though, that Smitty, the Kirkbys and I should put some concentrated energy into the whole question of the ecological meaning of plant taxa that show up in the fossil record. Perhaps we'll get a chance this summer.

One of the most interesting results of the pollen work to date is that few of the subsurface pollen spectra really match those of the surface samples. There are parallels and there are statistical matches at the 95% level, but I'm not all that proud of the latter despite the modern ecologist's justifications of mathematical models. So the interpretations I'm presenting about the past vegetation rely not on agreements between surface and subsurface pollen statistics as I would like. Instead they rely on agreements in interpretations of subsurface pollen records with interpretations (like those presented above) of surface pollen records.

Plunging on oblivious of the spectrum of probable error, let's look at the Archaic Horizon. My most ancient sediment which has so far yeilded pollen is probably the weathered ignumbrite at the bottom of Martinez Rockshelter. So far as I am aware, this sediment is culturally sterile. The peculiarities of the edaphic contextmake pollen interpretation even more highly tentative than otherwise, and we have only one sample to work with, but here goes:

This sample is a statistical match to a surface sample collected on the fringe of a mesquite bosque on upper alluvium of the Rio Atoyac near Lache in the Etla arm of the Valley. The pollen spectrum has some peculiarities, however, which make me feel that it probably represents either a savanna or an open vegetation canopy. It does not look like it represents a colder environment than today's, unless

it was so cold at the time that neither oak nor pine existed below 3000 meters. It most definately seems drier than today's environment at this elevation. When and if we get pollen records out of the consolidated sand at the base of the Cueva Blanca site, we might find pollen spectra contemporaneous with this one. It may be significant that Kent reports a fauna from the consolidated sand at Cueva Blanca indicative of a tree-less steppe environment.

The samples from sediment units B, C, and E at Guila Naquitz compose one pollen horizon which we can confidently date from the late 7th or 8th millenium B.C. The vegetation reconstruction is of a pine (possibly pinyon) savanna with an open understory of Thorn Forest shrubs. Oak does not seem to have been any more frequent in the area than it is today; it probably clung to the rocky canyon wall for the most part, as it does now. A moister climate than todays seems indicated. However, it was not as moist as today's climate in the Pine Zone or in the Cloud Forest now existing beyond the confines of the Valley. It was also not as cold as the Pine Zone is today, nor was it warm enough to introduce Cloud Forest elements to the local flora in any quantity. Temperatures thus seem to have been approximately as they are now at Guila Naquitz.

Local human plant foods should have been much like those now available in type, but reduced in quantity because of the less dense character of the vegetation structure. Faunal resources, however, would include some now found in the Pine Zone that could tolerate warmer conditions, and some now found in the Desert Grassland as adaptees to open country if they could compete against forms more tolerant of wetter environments. With small human populations such as are thought to have occurred, the reduction in plant food quantity may not have been the least deterrant to intensive occupation. This climatic reconstruction, by the way, seems perfect for the beginnings of maize selective harvesting. The warm-wet environment with much sunlight on the ground surface would be maximal growing conditions for maize and there seems likely to have been significant habitat variation to allow adaptive radiation as a significant process in maize population genetics.

A recent Harvard PhD dissertation by A. Bartlett is the only good pollen study in which comperable vegetation and climatic reconstructions might be sought. Bartlett investigated a series of lake sediment cores from Panama and believes she has a continuous record from ca. 9,300 B.C. to the present. From 9,300 BC to ca. 5,300 BC her record indicates conditions 2.5° C. colder than the present. She also reconstructs conditions drier than the present, since present Lake Gatun was then dry land. However, sea level changes associated with moisture reconstruction in ways they do not affect our data. Bartlett's interpretation of a much colder climate conflicts with mine of temperature values like today's between 6,500 and 8,000 BC. This conflict may be more apparant than real. Her index of colder temperatures is the presence of pollen of plants now living hundreds of meters higher than the present lake. Perhaps these plants are not so much adapted to cold as they are poor competers against plants now established in the wetter environment of the Panamanian lowlands. Thus the drier conditions existing at this time might have allowed high altitude plants a favorable survival balance at low elevations.

Sediment units B, C, and D at Martinez Rockshelter seem likely to be the next oldest samples in our Archaic sequence. These are undated--a matter I shall return to shortly.

The pollen from unit D and the basal 5 cm of Unit C yeilds an interpretation of oak savanna with an understory of Thorn Forest shrubs. This canopy admitted less light to the ground surface than did the canopy at the time Guila Naquitz was occupied but more light than the present canopy does. A climatic reconstruction of somewhat less moisture than today's seems reasonable; about as much difference as the contrast between a west-facing and an east-facing slope in the Thorn Forest today. The occurrence of an oak savanna indicates cooler temperattures than today's.

For a short while during the early deposition the pollen record indicates a fluctuation to moisture receipts on the order of those received at the locale today, though still cool enough to provide the oak savanna. The situation soon reverted to that described for unit D.

In the lower 10 cm of unit B, the oak savanna is still evident but the character of the Thorn Forest changes. It may or may not have become a bit more open than previously, but more subtropical elements are less in evidence. Rather than the type of Thorn Forest now occurring in the area, we get Thorn Forest with more desertic elements. In the 1967 classification Mike Kirkby and I worked out the change is from Thorn Forest A to Thorn Forest B. It seems to have been as cool as before, but significantly drier than today. In the upper 15 cm of unit B, the pollen record indicates reduction of oak to its present frequency and the clear establishment of a Thorn Forest B vegetation pattern. These pollen samples form a statistical match to Thorn Forest B surface samples. The period seems to be somewhat drier and perhpas slightly warmer than today.

If I were to guess what date should be applied to the cooler conditions evidenced in units C and D, I would say before the "Climatic Optimum" which is well evidenced for the Northern Hemisphere by 5000 BC. So I'd guess-date Martinez Rockshelter between 5000 and 6,500 BC since the environment indicated in unit D is not like that at Guila Naquitz. The warming and drying trends of unit B are perhaps evidence that that sediment was deposited after the "Altithermal" began, so 5,000 BC may be a meaningful date for the base of unit B.

Few samples were collected at Geo Shih because there were few cases where the archaeologist was confidant that he could sample the cultural horizon. Three of the collected specimens have yeilded sufficient pollen for analysis. Those were all evidently laid down during a time when a Desert Grassland vegetation pattern occurred at the site--probably in its grassland facies much like the pastura seen in the Valley today. The surface sample at Geo Shih indicates a rather wetter modern environment than one would expect--as wet as the slopes of Mierve el Agua or the Thorn Forest B district just above the parking lot near Cueva Blance. The moisture shows up in the pollen record even though the modern surface is cultivated--remember assumption (a) in this regard. The climatic reconstruction for Geo Shih during the Archaic, then, is a period warmer and drier than the present.

The radiocarbon date of 1400 BC for Geo Shih is not at all in accord with the

cultural evidence. I think this may well be a site of the Altithermal period. The intensity of the warm-dry conditions at Geo Shih seems much greater than that indicated in the samples from Unit B at Martinez Rockshelter, and I thus feel that Geo Shih is younger than that site. I would guess-date it around 4,000 BC.

One or two of the Geo Shih samples contain maize pollen. Dry farming could well have been undertaken in such an environment, but the crop would have been less dependable than dry-farmed maize crops are today in the Mitla arm of the Valley. If the artifacts indicate a consistant pattern of food grinding greater than that of earlier times, and if we presume the increase was due to maize cultivation, we might suspect floodwater farming to insure maize yeilds. Some Geo Shih samples contain high quantities of pollen referable to the Chenopodiaceae and the genus Amaranthus. While this could be indicative of amaranth cultivation, I think it will more probably turn out to be an index of water table variations along the main floodplain. I need to complete my analysis of floodplain sites to handle this matter competantly.

The youngest of the Archaic samples come from the B sediment unit at Cueva Blanca. These should date between 3,500 and 3,000BC. They form a palynological unit ~~xxxxx~~ rather like the modern Pine Zone samples, except they contain too little oak pollen and too much pollen of Thorn Forest elements for a statistical match. I reconstruct the vegetation pattern as a pine forest with an understory of Thorn Forest shrubs rather than an understory of shrubs now associated with Pine Zone or Oak Zone trees. I see this as due to a much a wetter environment with a temperature balance much like that of the present. The fauna obtained should confirm or dispute these interpretations.

The date of 3,000 BC is not at all innapropriate to this interpretation. Wet conditions are known from ~~xxx~~ a number of Northern Hemisphere locales at this time, though there are probably an equal number where drier conditions are just as well documented. Certainly, there is significant glacial activity in Alaska at this time.

Such an environment at Cueva Blanca, in the most arid arm of the Valley, would indicate amazingly wet conditions elsewhere in the study area. Dry farming and

floodwater farming might have been very difficult on the alluvium because of dense vegetation. One would almost have to postulate slash and burn techniques as necessary for all but the most dry locales. This is not a period in which to expect small farming villages along the floodplains or even the upper alluvium.

Taking a strictly environmentalist view, these various reconstructions would indicate that the history of agriculture--incipient or otherwise--in the Oaxacan preceramic is not likely to be similar to that of Tehuacan. Over much of the preceramic farming would have been dependent on the selection of highly localized specifically productive areas. Agricultural lands would probably have had significantly broad distributions only on the 6, 500 to 8,000 BC horizon and the 4,000 to 5,000 BC horizon to judge by the available data. For most of the time when agriculture was developing in Tehuacan, the animal foods we know to have been important to the preceramic of Oaxaca would have been least plentiful in the districts where agriculture had maximal potential of success and the plant foods which seem relatively more important in the Oaxacan Archaic economy (acorns, pinyon, columnar cactus fruits, etc) would have been least common. I would thus suspect that maize was hardly ever of much importance in the economy of the Oaxacan Archaic--less important than it appears to have been in Tehuacan.

If this is so, I am doubtful that the 3,000-1,500 BC horizon saw a major economic shift to agriculture; the sort of shift which is necessary to the establishment of village farming so far as we know. Why should it have come at that time when it did not come before even though maize may have been known and grown for millenia? Perhaps the reason we can find no early farming villages is that none ever existed in Oaxaca, or at least never developed indigenously out of an Archaic economic base. Perhaps the first Oaxacan communities dependent on agriculture were the urban centers of the San Jose Phase; migrants who had no cultural relationship to the local Archaic peoples.