The Archaic Maize Pollen of Koster:
An Essay on Canons of Evidence

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ABSTRACT

The recovery of putative *Zea* pollen in Helton Phase (ca. 5500 B.P.) deposits at the Koster site is discussed in relation to the details of sampling strategy, site stratigraphy and other palynological data. Objections to the credibility of this pollen as evidence of maize cultivation are analyzed and rebutted. It is argued that canons which apply to the evaluation of non-artifactual evidence of behavioral patterns are distinct from those which apply to artifactual evidence of behavioral patterns, and the logic and archaeological value of former is not less simply because it is not the same as the latter. It is suggested that as multi- and interdisciplinary study becomes a more common characteristic of modern archaeological investigation, concern with the methodological structures of the canons of evidence applicable to the different forms of data recoverable from sites will become more relevant in anthropological archaeology. The purpose of this essay is as much to instruct and sensitize archaeologists to this issue as it is to allay concern that the pollen evidence for Archaic maize cultivation at Koster may not be credible.
INTRODUCTION

In August 1971, a single \textit{Zea} pollen grain was observed in a sediment sample collected from Horizon 2 of the Koster site and a second \textit{Zea} pollen grain was observed in a sediment sample collected from Horizon 6. If the putative associations were correct, the latter observation could relegate maize cultivation to the Helton Phase (Late Middle Archaic) horizon in Illinois. At the time, research at the Koster site was barely beyond the stage of an elaborated testing operation. Since a good deal more excavation and laboratory work remained to be planned and executed at the site, public announcement of the discovery was delayed until data could be accumulated that would allow contextual evaluation of these records.

In essence, five forms of additional information were required for proper evaluation: stratigraphic studies that would confirm the association of the sediment sample with the artifactual record diagnostic of the Helton Phase; radiocarbon assays that would establish the chronometric antiquity of the Helton Phase deposits; flotation studies that would identify the character of the economically significant flora of the Helton Phase residents of the site; geomorphological and faunal studies that would provide reconstructions of the biophysical environment of the site independent of those provided through palynological evidence; and sufficient additional palynological data to allow assessment of the discovery.

The additional palynological information required was available in 1973. Recognizing that it would not alone provide a fully convincing basis for the conclusion that maize was cultivated in Illinois some 5,000 years ago, I declined to press the issue at that time. I presented a paper on the results of the Koster site pollen study at the 1974 meetings of the Society for American Archaeology, however, as a means of communicating the palynological evidence in unambiguous terms. I also prepared and pri-
vately distributed more than 50 copies of a summary report on the pollen of Koster, which included a thorough discussion of the Zea pollen, to archaeologists, paleoethnobotanists and palynologists who worked with Eastern Woodland materials (Schoenwetter 1974a). Since then the various forms of information which would place the pollen records in context have gradually accumulated. A series of eight radiocarbon dates now documents the antiquity of the Helton Phase deposits at Koster as ranging between 4880±250 and 5720±75 B.P. (Brown et al., n.d., Cook 1976:70). Stylistic analysis of three artifact categories and formal analysis of the chipped stone tool manufacture technology of the Horizon 6 deposits (Cook 1976) has established the unity of the behavioral pattern recognized earlier (Houart 1976) as a distinguishable occupation at Koster. Stratigraphic analyses (Butzer 1977, Brown et al. n.d.) have confirmed the Horizon 6 position of the earliest Zea pollen records from the site. Geomorphological (Butzer 1977) and faunal (Hill 1975, Jaehnig 1973) studies have documented the range and variety of probable biotic and physical conditions for the site and its environs during different periods of occupation. And, recently, the results of an extensive and intensive study of the charred macrofossil botanical remains from Koster and other sites in the Illinois Valley, covering the Early Archaic through Mississippian and Early Historic occupation horizons, have become available (Asch and Asch 1980).

Synthesis and evaluation of this large body of information is still somewhat premature. The details of the stratigraphic, paleontological and flotation studies, and the specifics of the pollen study of Koster and other Illinois Valley sites, are not widely available even now. It seems unlikely that they will be published in easily accessed formats within the next few years. But, at this writing, the raw data have been observed and the basic analyses that were required for contextual evaluation of the
1971 discovery have been performed. As a preliminary to the task of synthesis and evaluation, a public statement should now be made regarding the character of the palynological evidence for Helton Phase maize cultivation at the Koster site. Perhaps more important, it is time also to present a thorough discussion of the methodology relevant to the interpretation of that evidence.

In an earlier comment regarding the maize pollen records of Koster (Schoenwetter 1979) I made the claim that archaeologists' disregard of this evidence for the antiquity of maize cultivation in Eastern North America resulted from a lack of familiarity with palynological evidence. Chomko and Crawford (1979) suggested that the fact that the Koster pollen study had not been formally published was a more significant factor, and implied that knowledgeable experts had failed to include the palynological evidence in synthetic assessments of agriculture in eastern North America because they could not credit it. Chomko and Crawford are the only archaeologists to state publicly that the conclusion lacks credibility, but I have heard it before and since. I have been privately advised that any behavioral interpretation of the maize pollen I observed in the Helton Phase deposits must be discredited as a misidentification; that it must be discredited as the result of contamination by modern maize plants; that it must be discredited as an error of long-distance pollen transport; that it must be discredited as an uninterpretable statistical anomaly; that it must be discredited because its occurrence cannot be reconciled with the subsistence-settlement system reconstructions presented for Archaic populations of the Illinois Valley (or elsewhere in North America, for that matter); that it must be discredited unless independent confirmation of the occurrence of maize is established by direct or indirect association with macrofossil remains of maize in the same deposit of the Koster site; and
that it must be discredited unless maize pollen of equal or greater antiquity is independently recovered by other observers at other eastern North American sites in statistically significant quantities.

One of the objectives of this essay is to demonstrate that all of the above are ineffectual objections to the inference that the Helton Phase residents of the Koster Site cultivated maize in Illinois roughly 5500 years ago. A second objective is to instruct readers in the methodological principles relevant to behavioral inference from palynological evidence recovered from archaeological context. To fulfill these purposes, I shall eschew direct discussion of any non-palynological evidence from Koster or elsewhere, and shall refrain from bolstering the case for the conclusion by reliance upon palynological evidence now available at other locations.

The issue here is not the total range and variety of evidence which indicates whether or not the Helton Phase residents of Koster actually grew corn, nor whether the cultivation of maize was a behavior pattern undertaken by a number of Archaic Stage populations in eastern North America. Those are significant questions, but they will be better answered when data which are not now widely available become fully published and independently evaluable. The issue here is the character of the palynological evidence relevant to such archaeological reconstructions, and the ways its credibility as evidence may be legitimately assessed. I persist in the opinion that the palynological observations are an unfamiliar form of archaeological data, and archaeologists are not generally cognizant of the ways in which it is to be handled. In large part, this essay is a study in the methodology of archaeological pollen analysis. The information from Koster is simply a vehicle for exemplification.

A few assumptions are necessary to the ensuing discussion. First, readers are asked to accept the assumption that no deliberate attempt
has been made to deceive by either me or those who provided me written
records of pertinent activities or observations. This includes errors of
omission as well as commission. Though the full record of palynological
observations from Koster is not presented in this essay, no information
pertinent to the issue has been in any sense suppressed. Second, readers
are asked to accept the assumption that my archaeological experience and
training are adequate to the task of methodological assessment of the normal
and characteristic professional activities of that discipline. My formal
publication record is almost entirely palynological, and I realize that
this affects evaluations of my credentials to assume the role of anthropolog-
ical archaeologist. I must beg a measure of indulgence of readers on this
score - or at least a willingness to suspend immediate disbelief. Third,
readers are asked to accept my specific usage of the term "methodology".
Here the term refers to the study of scientific methods, especially the
study of the assumptive constructs and logic which subtend the common uses
of a method or a set of related methods. Finally, readers are asked to
accept the assumption that all varieties of maize are cultivars and, by
application of uniformitarian argument, it therefore follows that any pre-
historic variety of maize must be identified as a cultivar unless evidence
exists to the contrary.

This last assumption requires more elaborate discussion, since it may
be thought the present focus of some debate in biological circles (vis.
Beadle, 1981). Actually, it is not. All varieties of maize known to exist
today are cultivars. Maize plants have no mechanism of seed dispersal from
the rachis - a morphological trait unique within this sub family (tribe)
of grasses. Without human intervention, the husk-enclosed kernals of
maize cobs do not germinate and develop into reproductively successful
organisms. Animals and birds could conceivably scatter and bury sufficient
quantities of maize kernels and cob fragments to accommodate successful reproduction of a small number of generations. But the probabilities that a reproductively successful population of maize plants could exist without human aid are almost infinitesimal.

Uniformitarian argument (which is not the same thing as argument from analogy) involves the assumption of identity. Particularly, that the identity of two forms represents and reflects the identity of the total set of natural processes and their interrelationships which result in that form. Uniformitarian arguments cannot be discredited by simple recognition of the possibility that unknown processes could interactively result in the prior existence of morphologically identical mimics of phenomena which may be studied at the present time. Colloquially, assumption of identity discredits the possibility that objects identical to the bones of a duck could be the result of processes which happen to mimic those that today result in duck bones. Those who would argue the occurrence or existence of such processes are free to do so on the basis of positive evidence, but a uniformitarian argument is logically acceptable in the absence of evidence to the contrary. If maize is today a cultivar, application of uniformitarian argument logically demands that prehistoric maize also be recognized as a cultivar. Debate on the matter can only center on a limited set of issues: (1) does evidence exist to the contrary; (2) is there in fact taxonomic identity between the modern observable and the prehistoric phenomenon; and (3) is a uniformitarian argument theoretically applicable to phenomena of this sort.

(3), above, is resolved. Biophysical reconstruction of any sort is theoretically grounded on uniformitarianism. (2) is the issue upon which debate is centered in discussions of the accuracy of identification of domesticates, such as the debate over whether the maize identified as
wild corn (Manglesdorf 1974, Gallinat 1971) is or is not taxonomically distinct from modern observable domesticated maize. This issue would only apply if there were grounds for taxonomic differentiation between the pollen of modern cultivated maize and that observed in the Koster site.

THE RECORD

The Koster site is most simply characterized in the combination of geomorphological and archaeological terms used by Butzer and Houart in the introductory paragraphs of their respective works:

Koster is located at 39°12′27″ N and 90°33′02″ W in Greene County, west-central Illinois... The site itself is situated at the margin between two macroenvironments, the upland loess and till plains to the east and the floodplain of the Illinois River to the west. It is also situated in a complex of mesoenvironments, on the sloping margin of a minor tributary (Koster Creek) that emerges from the fretted, upland bluffs onto an alluvial fan that grades down onto the valley bottoms. Soils here are primarily developed on Pleistocene or reworked eolian sediments (in the main part, silt-sized material or loess) of various ages, and Holocene geomorphic activity has been characterized by repeated erosion and deposition of materials that were and post facto remain deceptively similar. (Butzer 1977:1)

Koster is a stratified multicomponent site located in the Illinois River Valley... The site was originally defined on the basis of debris scatter from a Late...
Woodland occupation on the present-day ground surface. The area of scatter for this occupation, approximately 25 acres, covers a ridgetop ("East Field"), a hill slope ("North Field") and extends down the floor of a tributary stream valley ("South Field")... the 1969 test excavations at Koster revealed five additional occupations beneath the Late Woodland component. (Houart 1971:1)

Houart (1971:5-8) established the method of delineating the vertical bounds of occupation horizons at Koster through evaluation of the distributions of density patterns for various classes of cultural debris, in support of field assessments of variations in soil color and densities of cultural mottling. This method has been pursued as excavation and stratigraphic analysis has continued with the result that "25 cultural strata were defined occurring as far as 12m below the surface" (Asch and Asch 1980:25). The test Houart undertook at Square 8 in 1969 and completed in 1970 is now recognized to have transected 16 occupation components and a number of culturally sterile strata (Brown, pers. comm. 1978). The 113 sediment samples of Square 8 collected for pollen analysis were taken at 3 inch intervals by Houart to conform with the arbitrary 3 and 6 inch level intervals she had used in the test excavation. These samples, and the corresponding samples removed that year by Jaehnig for malacological study, were collected from freshly scraped wall exposures after the text excavations were completed.

The pollen frequency diagram for Square 8 is illustrated as Figure 1. When I reported the occurrence of maize pollen in Horizons 1, 2 and 6 to the Project Director during the summer of 1971, additional excavations were in progress at Koster. An additional 26 sediment samples were collected:
<table>
<thead>
<tr>
<th>Cultural Horizon</th>
<th>Number of Sediment Samples Examined</th>
<th>Number of Pollen Grains Identified</th>
<th>Samples Yielding ≥ 30 Observations</th>
<th>Samples Containing One Grain of Maize Pollen</th>
</tr>
</thead>
<tbody>
<tr>
<td>1</td>
<td>4</td>
<td>320</td>
<td>4</td>
<td>1</td>
</tr>
<tr>
<td>2</td>
<td>4</td>
<td>378</td>
<td>4</td>
<td>1</td>
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<tr>
<td>sterile 3</td>
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<td>15</td>
<td>0</td>
<td>0</td>
</tr>
<tr>
<td>sterile 4</td>
<td>4</td>
<td>225</td>
<td>3</td>
<td>0</td>
</tr>
<tr>
<td>sterile 5</td>
<td>5</td>
<td>98</td>
<td>1</td>
<td>0</td>
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<tr>
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<td>979</td>
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<td>0</td>
</tr>
<tr>
<td>sterile 8</td>
<td>4</td>
<td>62</td>
<td>1</td>
<td>0</td>
</tr>
<tr>
<td>sterile 9A</td>
<td>1</td>
<td>262</td>
<td>3</td>
<td>0</td>
</tr>
<tr>
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<td>160</td>
<td>1</td>
<td>0</td>
</tr>
<tr>
<td>sterile 1A</td>
<td>4</td>
<td>100</td>
<td>1</td>
<td>0</td>
</tr>
<tr>
<td>sterile 1B</td>
<td>3</td>
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<td>15</td>
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<td>sterile 3</td>
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<td>30</td>
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<td>0</td>
</tr>
<tr>
<td>sterile 6</td>
<td>1</td>
<td>2</td>
<td>0</td>
<td>0</td>
</tr>
<tr>
<td>sterile 2</td>
<td>6</td>
<td>12</td>
<td>0</td>
<td>0</td>
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</table>
After completing excavations through Horizon 8 in Block 12 and part of Block 13, and through the base of Horizon 6 in the remainder of Block 13, we scraped the 84-foot-long profile on the north and south edges of the combined blocks. Horizons 6, 7 and 8 appear beautifully distinct along the full extent of both profiles. The visual position of these horizons in the profiles corresponds closely with their artifactual determination in the data processing lab...

After scraping the profiles carefully we took a series of pollen samples from the top, middle and bottom of Horizon 6, from Horizon 7, and from the top, middle and bottom of Horizon 8 in a series of squares extending from the far western edge of the 84-foot-long block excavation to the eastern edge. (S. Struever, letter of 2 Sept 1971)

Though all the 139 sediment samples collected from the Koster North Field excavations in 1970 and 1971 were submitted to the Palynology Laboratory, it was never intended that all would be studied. The palynological research project was specifically designed to provide information required for program planning purposes, such as the probable costs and duration of palynological research oriented towards paleoecological reconstruction (see Schoenweder 1971, 1974). The recovery of maize pollen was an unexpected side effect of great interest. But neither the project director nor I was sympathetic to the idea of a major reorientation of our mutual research interests and plans immediately to pursue the issues this discovery raised, as we knew that additional sediment samples would be collected for future pollen and flotation studies relevant to those matters. Ultimately, 20
of the 1971 samples and 83 of the Square 8 samples were processed to extract contained pollen. Pollen density in the North Field deposits, however, is low and most samples did not yield much pollen per drop of extract. Most samples were therefore considered unanalyzable by the standards established for the project. Table 1 provides the observation statistics relevant to this essay.

THE OBJECTIONS

1. The putative maize pollen may be misidentified, since certain spores mimic the morphology of maize pollen (Culley and Clary 1980:272) and the range of size variation in grass pollen of other Mayadae taxa can overlap that of Zea mays under certain conditions (Kurtz, Liverman and Tucker, 1960).

This objection is particularly relevant because the spore type involved is observed quite commonly in North Field deposits, and both the pollen and the spores of Koster are poorly preserved. As a result of poor preservation, large spherical spores often lose the tail-like appendage which distinguishes them from pollen most clearly; they become crushed or distorted in fashions similar to the commonly crushed or distorted pollen grains; and they develop eroded or corroded cell wall areas similar to the annulated pore aperture characteristic of maize pollen if the latter is poorly preserved. This problem is a familiar one to palynologists who deal with archaeological context deposits. To reduce misidentification errors, palynologists tend towards conservatism in the identification of Zea pollen (cf. Bohrer 1968). That was the case at Koster. In addition to comparison against reference specimens and identification atlases (e.g. Kapp 1969), a palynomorph was not identified as Zea unless the Zea attributes (size, aperture
system, size-annulus ratio and exine morphology) were clearly evident and the identification was confirmed by a second or third trained observer. Obviously, the possibility that the palynomorphs were misidentified is not lessened by the precautions taken. Methodologically, the situation parallels that encountered by the archaeologist when cataloguing or labeling materials in reference to their field provenience. The field archaeologist is responsible to establish procedures to insure that recovered materials are relegated to known, specific, proveniences. One normal procedure is to bag the materials of a particular provenience together, and label the bag. In the laboratory the materials are removed from the bag, cleaned and otherwise prepared for analysis, and individually labelled with a code that allows recognition of provenience. Precautions are taken at each step to insure that the individual labels accurately identify the original provenience of recovered items. But the possibility that a given label is erroneous is not lessened by the precautions taken. The credibility of labels, in the last resort, depends upon the degree of rigor with which the set of precautions is enforced and the complexity of that set. As the set of precautions increases in complexity, with more interactive cross-checks, the probability that an error will be recognized increases. But the possibility that any individual label is inaccurate remains. The issue, then, is not the possibility of mislabelling but the likelihood of such a possibility given the character of the set of precautions taken and the degree of their enforcement.

The same principle holds when a pollen identification is questioned. One does not resolve the problem by recognizing the possibility that a misidentification has occurred. Of course such a thing is possible, and it is possible that a misidentification may be selective and consistent. One assesses the prospect that a misidentification has occurred in relation
to the precautions that were used to reduce the probability of this kind of error. But such an assessment requires familiarity with the normal range of precautions used. In the archaeological case, someone who has no archaeological training or experience can comprehend the character of the precautions taken, but has no real way to assess their effective value. Such evaluation requires familiarity with the application of precautionary practices and experience with their effectiveness. Similarly, one who has no familiarity with pollen identification can recognize that the independent evaluation of a second observer is a precaution against misidentification. But he has no real way of evaluating its effectiveness without a body of personal experience.

The prospect that the putative maize pollen is in reality the misidentified pollen of a closely related taxon with similar pollen morphology is not lightly dismissed. I have discussed the relevant issues raised by this in an earlier paper (Schoenwetter 1974b), however, and shall only summarize them here. In essence, the only other probable taxa are those of Old World cereal plants, species of the Old World genus Coix, members of the New World subtropical genus Tripsicum or species of teosinte (Zea mexicana). If Old World taxa are in fact involved, the problem is one of contamination by modern pollen, not a problem of misidentification. The contamination objection is dealt with below. If the pollen observed actually is Tripsicum - which does not have the exine morphology of Zea pollen (see Manglesdorf et al. 1978:242), but might have been somewhat different 5000 years ago - the problem is one of long-distance transport, not identification. This objection also is dealt with below. If the pollen observed actually is Zea mexicana rather than Zea mays pollen the taxonomic confusion does not affect behavioral interpretation, since both species would require
cultivation to grow in the Illinois Valley at any time in the Holocene.

Another possible reason for a misidentification is the mutation (or adaptation) of a local species that would allow it to produce pollen which mimics that of Zea. Dealing with such a possibility is exactly equivalent methodologically to dealing with the possibility that natural flakes of local cryptocrystalline rock were modified and transported to a site by natural forces in a fashion which would allow them to mimic flakes of the same rock which were transported and modified by human beings. It is impossible to exclude the possibility that such a thing might occur. It is impossible to exclude the possibility that it could have occurred often enough and consistently enough that misidentifications appear in the record. But the character of the required modifications of the normal action of natural forces, so far as these things are known, is extreme. The likelihood that the possibility is to be granted credence as a probability which accounts for the observed record, then, is too low to serve as a telling negative argument.

2. The maize pollen is a contaminant of the fossil record, introduced from either a younger archaeological horizon or from the modern surface or atmosphere.

The prospect of contamination looms large since, as Asch and Asch pointed out (1980:92), the portion of the Koster site from which the sediment samples derive was a maize field through the year prior to season the square 8 samples were collected. Also, the Illinois valley is today one of the most intensively cultivated districts of the American Midwest, and hybrid maize seed crops constitute a highly significant fraction of that production.

Contamination is a problem pollen analysts have had to face since the initial development of the technique (see Faegri and Iverson 1975:167 ff).
The things observed, the fossil pollen grains, are neither physically nor chemically distinct from their modern counterparts under ideal conditions. Pollen grains undergo no regular process of fossilization over time; fossil pollen is merely old. There is, therefore, no parallel between the petrification process which applies to bone. The process by which pollen is preserved is more like that by which uncharred vegetable material may be preserved at a site. If the depositional environment happens to exclude those physical, chemical and biotic variables which account normally for its degradation and destruction, the pollen will remain intact, unmodified, in situ.

However, pollen grains are more resistant to a wider variety of degradation agents than most plant tissues. Thus pollen will resist destruction in contexts in which few other vegetal materials will be preserved. But most environments on the earth's surface are not hospitable to pollen grains. Modern pollen grains recovered from the soil surface therefore normally appear no more and no less deteriorated than fossil pollen grains. On the whole, modern pollen will tend to be less often affected by the forces that crumple, break, twist, corrode and erode fossil pollen. But identification of a particular pollen grain as a modern contaminant on the basis of the quality of its preservation is rarely secure.

There are essentially two methods that are used to recognize the occurrence of observation errors introduced by contamination. The ecological method relies upon knowledge of both the biological processes of pollen production and dispersal and biogeographic knowledge. Such knowledge is used as a baseline for recognition of the probability that an observed pollen grain is better identified as a contaminant than as a taxonomic feature of the sampled flora. A pollen type representing a taxon exotic to the assemblage of plants otherwise represented is particularly likely to be a
contaminant if it derives from a taxon which produces limited quantities of pollen and is adapted for limited pollen dispersal. The other method is statistical. Essentially, however, it uses a formal (statistically defined) parameter to express the same probability recognized in the ecological method. Application of the statistical method is convincing only where it is justified by argument that a particular parameter is apt for the case in point.

The problem with these methods is that they can recognize the existence of a contaminant only by its exotic or unusual character. On the one hand, this means that actual contaminants may not be recognized by palynological methods. Some of the Compositae pollen grains observed in a Horizon 6 sample may be contaminants. There is no palynological method for recognizing them since there is no basis for evaluating the Compositae pollen as exotic or the Compositae pollen values of Horizon 6 as unusual. On the other hand, it means that the methods do not identify the source of any recognized contaminants. Palynological methodology identifies the maize pollen observed in Horizons 1, 2 and 6 as contaminants because they are exotic to the floral assemblages of those deposits. But it does not address the question of whether this may be true because modern pollen was introduced into the sample during collection. It could be true because an exotic plant was the source of the maize pollen at the time the depositions occurred. Palynological methods work to accomplish the tasks for which they have been designed: identifying arguably contaminant pollen grains among those observed. They cannot be faulted for failure to accomplish a task we would prefer: segregating all possible contaminants from the rest of the pollen spectrum, or identifying sources of arguably contaminant pollen grains.
To illustrate the limitations of these methods it is instructive to review Asch and Asch's note (1980:93) that the prospect that the maize pollen grains of Horizon 6 at Koster are modern contaminants "is raised orders of magnitude above that of samples collected with comparable care from optimal pollen sites." The argument which supports this conclusion recognizes that the low concentration of preserved pollen in the Horizon 6 deposits makes the observation of contaminants which might exist far more probable than would be the case if the samples were polliniferous, since a fixed number of pollen grains (100 or less) was observed from each sample. Essentially, Asch and Asch are saying that one is more likely to spot a member of a 10-grain population of contaminants mixed into a 500-grain population than would be true if the same 10 contaminants were mixed into a 5,000 or a 50,000-grain population. The argument is an application of the statistical method by which contaminant pollen grains are recognized. It is valid, but it does not apply uniquely to maize pollen in the Horizon 6 case. Any of the pollen grains observed in a non-polliniferous sample has a higher probability of being a contaminant than has any of the pollen grains observed in a polliniferous sample. Further, the argument does not apply uniquely to modern pollen as a source of contamination. The method allows recognition of the maize pollen as an exotic, unusual, member of the assemblage but it does not exclude any possible source.

The prospect that the arguably contaminant maize pollen was introduced into the Koster sediment samples between the time the deposits were exposed by excavation and the time laboratory processing was undertaken (i.e. that the samples were contaminated by younger or modern maize pollen in the field) must be evaluated by assessment of the field situation. The issues involved
are those of sources of contamination and mechanisms of contamination.

Since maize pollen grains are microscopic and are light enough to be transported by wind currents, one might think that the issue of sources of contamination would be the problematical one. After all, the Illinois Valley is a major maize production district. Further, in this part of the Illinois maize pollinates in late August (see Struever and Holton 1979:8-9), and the Koster samples were collected at that time of year. But the biological character of maize pollination must be taken into account. Zea produces male and female flowers separately at different locations on the plant. The pollen produced in the tassel flowers at the top of the stalk must be transported to the silk flowers located in the leaf axils arrayed along the vertical length of the stalk. Though Zea pollen is transported by wind rather than an animal (e.g. insect) vector, its basic reproductive adaptation is a reliance on gravity. Most maize seeds represent the result of self-pollination, not cross-pollination.

That maize does not disperse its pollen widely from the parent plant is evidenced empirically by the results of a number of surface sample pollen studies (Martin and Schoenwetter 1960, Berlin et al. 1977, Martin and Byers, 1965, Fish 1971) and by experimental study (Raynor et al. 1972). Producers of hybrid seed corn in the Illinois Valley recognize this characteristic of maize in a pragmatic fashion. When planting in preparation for a crop of cross-pollinated seed, they plant an equal number of male and female parents and position them so each female is adjacent to at least one male. Most female plants are positioned equally adjacent to two males. Since the amount of crop produced is proportional to the number of female parents, the amount of acreage devoted to male plants directly reduces the desired yield. But this must be tolerated because a female plant which is not allowed to self-
pollinate (i.e. is detassled), yet is not immediately proximate to at least one male, yields too small a seed crop to repay the expense of bringing the female to maturity. The occurrence of significant quantities of maize pollen in the air only a few meters beyond the borders of maize field is thus problematical even during the period the plants are pollinating. That maize plants scores of meters away would serve as a source of airborne pollen is highly unlikely. The distance of maize fields from the sample locations at the time of sampling argues that district sources of maize pollen did not serve as effective reservoirs for contamination of the Koster pollen samples by an airborne transport mechanism.

Immediately local sources are also out. The pollen samples of test square 8 were not collected until late in the 1970 field season. No crop was planted the North Field location that year, and the plow zone in the immediate area of the test had been removed the prior year. The samples collected in 1971 came from areas that had not been planted to corn for three seasons and had been stripped of their plowed deposits for months prior to sampling. Thus there were no local plants which might have rained maize pollen down on the ancient deposits. There were also no local sediments which could have served as reservoirs of trapped historic maize pollen that might have rained down as the samples of the ancient deposits were collected.

Another mechanism of pollen transport which might be involved in a field contamination of the samples is water and surface runoff. Both the Square 8 profile sampled in 1970 and the long profile sampled in 1971 had been exposed to rain. More important, though the profiles were protected by plastic sheeting which reduced its erosive force, the profiles were subject to sheet wash by runoff from surrounding surfaces. Air-borne pollen grains
might well have been efficiently collected by raindrops (McDonald, 1962) and deposited upon the exposed surfaces of the profiles. Pollen exposed on the surface of the ground, or washed to that surface from the leaves and stems of local plants, could well have been collected by sheet wash and transported to the profiles' surfaces. The normal precaution used to reduce contaminations of this sort is removal of the surficial sediment layer of the profile (scraping) just prior to collection of the samples. But this precaution may have been ignored, or ineffective as a result of human error. Human error or inexpertise could also result in the transfer of pollen-bearing sediment from younger proveniences to older ones, or the mislabelling of samples.

Unlike contamination by air-borne pollen, these mechanisms of contamination cannot be selective. Raindrops do not strain single pollen grains or single pollen types from the air; they collect those in their gravity and wind directed paths. Sheet wash collects that pollen which is available in its path for transport given the load potential of the rivulet involved. Pollen-bearing sediment which is inadvertently transferred from one sample to another transfers all its contained pollen, not just the pollen of a single taxon. If any of these mechanisms in fact accounts for the occurrence of the maize pollen observed in the Horizon 6 deposits, it must also be responsible for the introduction of contaminants of other pollen types. Further, neither such mechanism should selectively contaminate the samples of Horizon 6. The deposits of other Horizons should be similarly contaminated.

Inspection of Figure 1 indicates that non-selective contamination of the Horizon 6 samples by sheet water transport across a modern surface and down the exposed profile is not very likely. The nature of a pollen spectrum which might be so collected is not known, though one may presume for the sake of argument that it contains a significant fraction of maize pollen.
grains. As it washed down the surface of the profile the water would dis-tribute the pollen it carried proportional to the vertical distance it travelled, since the plastic sheets protecting the profiles slowed downward movement. The upper archaeological horizons would thus be heavily contaminated, and contamination would be progressively less with depth. In the Square B pollen profile, the samples yielding pollen records at depths between 10 and 18 feet (pollen zone E) are thus unlikely to have been contaminated in this fashion at all, and the pollen spectrum they contain can be contrasted with that of the deposits of the upper 2 feet of the profile (pollen zones A and B) to discern what the palynological character of the contamination might be.

The contrasts are fairly obvious. The pollen records at 10 to 18 feet depth contain significantly less arboreal pollen than is the case for those to the 1 to 2 feet depth—particularly, less Quercus (oak), Carya (hickory) and Corylus (hazel) pollen than occurs in the upper two feet. If contamination is occurring by the mechanism proposed, these tree and shrub pollen types must be contaminants transported to the samples along with the maize pollen. In fact, they must make up the great bulk of contaminant pollen. Yet those pollen types account for even less of the pollen spectrum of the ostensibly contaminated sample of Horizon 6 than they account for in the ostensibly uncontaminated pollen spectra of pollen zone E at Square B. Further, if the bulk of Quercus, Carya and Corylus pollen observed in pollen zones A and B is the result of contamination, excluding those types from consideration should provide a closer approximation of the character of the uncontaminated pollen spectra of pollen zones A and B. The predominant pollen types of these zones would then be Compositae, Gramineae, Chenopodineae and Ulmus. Though these are the predominant pollen types of
pollen zone C, at 3 to 4 feet depth, the latter 3 are not prominent pollen types of the Horizon 6 pollen spectra which contain maize pollen. Since water transport is an unselective mechanism of contamination of pollen records, it is extremely unlikely that the maize pollen in the Horizon 6 sample was observed as a result of such a contamination mechanism.

Interestingly, the contrasts which occur between the pollen records of the upper two feet of the Square 8 profile and those which occur at the 2-4 foot depth are what one would expect if the former were contaminated by oak, hickory, hazel and maize pollen and the latter were not. This does not mean the pollen spectra of the upper levels of Square 8 are in fact contaminated. One would determine this by comparing these pollen records with others dating to the same horizons but sampled in different years or from non-profile contexts. I have pointed the matter out only to document that characteristics of pollen spectra which are not necessarily obvious to the non-specialist are those which can be identified as the most relevant indices of potential contamination by the experienced palynologist. Here, the possibility that the upper levels of the pollen profile collected at Square 8 is unselectively contaminated is evidenced by the nature of the contrasts between the spectra of pollen zones A and B and those of zone C. It is not evidenced by the occurrence of maize pollen in Horizon 6.

What of the possibility that the pollen records of Horizon 6 were contaminated as a result of human error during the sampling process? Specifically, sediments of younger deposits which contain maize grains as a true member of the record may have been erroneously incorporated with the Horizon 6 samples. In the Square 8 case, the younger deposits which might have been involved would necessarily have contained the pollen types characteristic of pollen zones A, B and/or C distributed in proportion to the frequency values expressed in those pollen zones. The contaminated Horizon 6
sample should thus have been contaminated to a greater extent by oak, hickory, hazel, elm, Chenopodinae or grass pollen than by maize pollen. But it is not. In the case of the long profile from which the Horizon 6 samples containing maize pollen were collected in 1971, Horizon 6 was the youngest horizon sampled.

The nature of the field situation at Koster during the 1969 and the 1971 seasons makes it unlikely that normal field sources or mechanisms of maize pollen contamination could be responsible for the occurrence of this taxon in the pollen record of Horizon 6. The biology of maize pollination is such that wind transportation of maize pollen from non-local sources is extraordinarily unlikely, and local maize pollen sources did not exist at the sampling locations. Even the depositional unit that could have served as a local reservoir of historic maize pollen had been removed by the time sampling was undertaken in 1971. Contamination by runoff waters washing maize pollen from surrounding soil surfaces across the profiles is a second possibility. In the first place, precautionary scraping was employed to obviate such contamination. But if this had been ineffective, we can predict the palynological character of contamination of this sort from our knowledge of the nature of the measures used to reduce sheet runoff erosion of the profiles and the fact that such a water transport contamination mechanism is unselective. The requirements of the prediction are not met by the pollen records of Horizon 6 in the Square 8 profile. A third possibility is that the Horizon 6 pollen samples were contaminated by maize pollen introduced from a younger deposit by human error. In the case of the Square 8 profile, the palynological characteristics of younger deposits are sufficiently distinct from those of Horizon 6 that unselective contamination of this sort is unlikely. In the case of the long profile, the Horizon 6 samples were the youngest ones collected.
A final possibility is stochastic error resulting from human agency. That is, the selective inadvertent introduction of maize pollen contaminants into a sediment sample from such non-systematic sources as contaminated tools; body oils, mucus or perspiration; drinking water; or food. The possibility of such contamination can hardly be denied. But that it would be only maize pollen that was so introduced to the record, and that such contamination would uniquely affect the samples of Horizon 6, and that they would equally affect the samples of Horizon 6 collected during both the 1969 and the 1971 seasons stretches the point. Speaking charitably, the possibility that a random and peculiar series of human errors might account for the contamination of maize pollen in the Horizon 6 samples is real. But those who would argue that this real possibility in fact occurred must bear the burden of demonstrating it is likely as an apt explanation. The same is true for those who would maintain that selective maize pollen contamination of the Horizon 6 samples occurred in the laboratory environment, rather than the field environment. Unless the contamination is thought to be deliberate, what must be explained is how this specific set of samples could be uniquely contaminated by this particular pollen type.

3. The Horizon 6 maize pollen record, like the maize pollen records of Horizons 1 and 2, are uninterpretable statistical anomalies.

This objection is more commonly, and more colloquially, expressed by the comment that a single observation is statistically meaningless. The objection, and its colloquial expression, are both perfectly true and logically correct statements. Unfortunately, they do not properly apply to the situation at hand. The reason they do not apply is that the statistical characteristics of the pollen record of Horizon 6 at Koster are not relevant to the question of whether or not the maize pollen observed in
Horizon 6 samples represents maize cultivation.

Pollen analysis is a statistical technique in paleobotany. What is not widely appreciated is that pollen analysis is not the same thing as the observation, identification and tabulation of pollen grains. Methodologically, the work of observing, identifying and counting the pollen of a sample is akin to typing and counting the artifactual materials recovered from some provenience unit at a site, while pollen analysis is akin to seriating one's observations of artifacts. The former is a procedure for garnering information; the latter a procedure for organizing information in a logically defensible fashion so certain kinds of inferences may be drawn. The way in which the characteristics of the artifactual record are organized by seriation procedures is logically justified by the objective of seriation as an archaeological technique: to identify orderly variation that may (granting certain anthropological assumptions) represent chronologically-dependent cultural change. The way in which statistical standards are applied in paleobotanical pollen analyses are exactly comparable: to identify orderly variation that may (granting certain assumptions about how pollen is produced, dispersed and preserved) represent floristic change.

Palynological records which are expressed as frequency values are only interpretable floristically if those values are significantly greater than 0.0%. This statistical standard is logically required by the assumption that the record presented by the pollen contained in a sediment sample (unless constrained by such errors as long-distance transport of pollen, local overrepresentation, or contamination) constitutes a randomly distributed representative sample of the pollen rain of that time and locale. Given this assumption, analyses with the objective of reconstructing the vegetation producing the pollen record must constrain interpretation to those values which cannot represent the existence of chance events.
Since frequency values which are not significantly larger than 0.0% could be the result of stochastic processes, they are viewed as uninterpretable, anomalous, meaningless values until evidence exists to the contrary.

Pollen analysis, however, is not solely and uniquely a technique of paleobotany. In geology, stratigraphic pollen analysis is an application of biostratigraphy which functions to identify and segregate sedimentary strata. Generally, palynostratigraphic analysis does not employ statistical standards. When utilized as a paleobotanical technique which elucidates the evolutionary relationships and distributions of Tertiary floras, pollen analysis is also not normally statistical in character. Basically, the relevance of statistical standards in pollen analysis depends upon what purposes the technique is used for and the assumptive constructs which underly and justify that particular application.

The question at issue in this case is not floristic but behavioral. The floristic question is whether the maize pollen of Horizon 5 can be identified as representing a significant component of the local flora. The answer to that question is negative. The behavioral question is whether the source of the exotic is the set of uniquely and specifically human behaviors known as maize cultivation. Does archaeological methodology demand application of statistical standards? That it does not is easily demonstrated by considering the situation which would arise if a pattern of human behavior which is evidenced artifactually were at issue, rather than one evidenced by the occurrence of pollen grains.

The Horizon 6 pollen samples represent 29 different provenience units of the Koster site; essentially 29 separate levels from 6 different squares. 11 of these provenience units provided pollen counts large enough to be relevant to project needs. 3 of the 11 incorporated an observation of maize pollen. Let us speculate on the conclusion that would be logically drawn
if the same situation applied to the question of ceramic manufacture, and instead of maize pollen grains one observed potsherds. The horizon is sampled by 29 levels from 6 different squares. 18 levels are artifactually sterile. One potsherd is observed in each of 3 of the 11 artifactually productive levels. Do those 3 potsherds evidence the existence of ceramic manufacture at the time of Horizon 6 occupancy?

The answer to this question does not depend on the statistical significance of the frequency value for potsherds in the recovered artifact assemblage. It depends on whether or not the potsherds observed in fact are the results of ceramic manufacture (i.e. are properly identified) and whether or not they in fact derive from their putative provenience locations (i.e. are not contaminants). If the items observed are indeed the products of ceramic manufacture and indeed were in situ in Horizon 6 contexts it makes no difference whatever whether they accounted for a statistically significant proportion of the artifact record. Any number of potsherds in such a situation would constitute evidence in favor of the existence of the behavioral pattern in question. And there is a very sound logical reason why statistical standards are not necessary for the resolution of questions of behavioral reconstruction. Stated simply, it is because there are no assumed constraints on the nature of pre-existing behavioral patterns which are justified by anthropological theory.

Did Australopithecines speak languages? Did Pithecanthropines fly about in hang gliders? Did Neanderthals exchange information across continental distances? Anthropological theory does not constrain our consideration of any of these as possible patterns of behavior. The methodology of scientific archaeology, however, demands the existence of positive evidence in support of behavioral reconstructions that document the prior occurrence of anthropological patterns. Yet archaeological theory recognizes
the constraints imposed upon accumulation of positive evidence. Specifically, it recognizes that the material record is normally depauperate as the result of an inverse relationship of the degree of material preservation and the passage of time. Archaeological theory also recognizes that the organization of loci of human behavior is not random, so the spatial distribution of material remains reflecting patterns of human behavior at such loci (i.e. sites) also is not random. Further, the degree to which a particular body of material evidence approximates or diverges from randomness in its distribution may or may not be a relevant datum when identifying the behavior reflected by that particular material record. Because positive evidence is so constrained, the amount of positive evidence demanded to demonstrate the existence of a behavioral pattern is not required to meet a statistical standard unless the anthropological significance of the behavior is similarly constrained. That is, statistical standards are only necessary where they are documentably relevant.

The anthropological significance of a behavior pattern (for example decorating pottery in a particular style) may be dependent upon a statistical standard, rather than a matter of presence or absence. The occurrence of a particular art motif or style on 25% of the pottery of a site might, for example, allow anthropological classification of that component as a member of Phase X while its occurrence in any significantly lower frequency would argue that its recognition as a member of the Phase X group is inappropriate. But any amount of positive evidence of the occurrence of the particular motif or style is adequate to draw the inference that it existed as a member of the inventory of art forms.

The maize pollen grains of Horizon 6 are material. Though microscopic, they are not less real than potsherds, projectile points, fire-cracked rocks, or any other material items observed in association with them in the provenience
units identified. They are not misidentified and they are not field or laboratory contaminants. There is literally no botanical question that maize plants could not germinate or survive to the point of reproductive maturity in the context of any Holocene temperate latitude vegetation pattern without the encouragement and assistance of human cultivation behavior. The existence of any number of in situ maize pollen grains in such a context thus fulfills all of the requirements of evidence which are demanded to resolve the question at issue by either botanical or archaeological standards. The statistical properties of the pollen records of Horizon 6 are not relevant data in assessing the verity of this conclusion.

This is not to say that the numbers of maize pollen grains observed are totally irrelevant to any sort of behavioral questions we may have about the cultivation of maize during Horizon 6 times. One question is the degree to which the available palynological evidence indicates the intensity of maize cultivation at Koster at different times. This question is mathematically framed, and requires an answer which is either quantitative or relative. Briefly, the quantity of maize pollen observed in the entire population of Horizon 6 pollen grains (0.306%) is approximately equal to that observed in the populations of Horizon 2 pollen grains (0.264%) or Horizon 1 pollen grains (0.313%). If the proportion of maize pollen grains in the population of an occupational horizon serves as an accurate index, the palynological evidence is that maize cultivation was equally intense at Koster at any time it was practised.

Asch and Asch (1980) argue that maize cultivation was significantly more intense at Koster during Late Woodland occupation, and that the macrofossil evidence for maize cultivation in any degree of intensity during earlier occupations of Koster (or elsewhere in the Illinois Valley) is yet
of debatable credibility. The conflict with available palynological evidence, however, may be more apparent than real. Asch and Asch note specifically (1980:94) that there is evidence for the occurrence of two temporally distinct Late Woodland occupations at Koster, and the evidence for intensive maize cultivation is not equally distributed between them. As they say,

"the transition to intensive maize production may have occurred during a continuous span of Late Woodland occupation."

None of the 4 Late Woodland (Horizon 1) sediment samples analyzed is, in fact, very likely to date to the time of the younger Late Woodland occupation of Koster.

4. The maize pollen of Horizon 6 does not constitute adequate evidence of Archaic Period maize cultivation because no direct or indirect association of this pollen and maize macrofossils has been observed.

This objection recognizes the fact that a serious and systematic search for carbonized maize macrofossils in Horizon 6 contexts has been performed (Asch and Asch 1980). Absolutely no carbonized maize remains were observed in the examination of 30 kg of charcoal derived from 212 standardized flotation samples from Horizon 6 features. This is not a function of poor preservation, for 76.66% of the 891 seeds recovered were identifiable. By contrast, in the Late Woodland component at the Healy site one-half of one kg of charcoal representing 87 samples from features yielded 58 maize macrofossils out of 914 seeds (77.90% identifiable). In other words, the search for independent corroborating evidence was thorough and comprehensive. The fact that it proved fruitless should be given serious consideration.
However, the issue is not what it appears. Demands that the credibility of one form of evidence be justified by the co-occurrence of another form of evidence are not unheard of. But this standard does not apply in cases of behavioral reconstruction. The inference that the residents of a site practised basketmaking does not require both the recovery of baskets and basket-making tools. The latter satisfies the demands of evidence sufficiently even though such tools may be used for other purposes. There is no question that the co-occurrence of both forms of evidence strengthens the conclusion substantially. Nor is it fair to say that the conclusion is totally justified solely by the existence of nothing other than the minimally sufficient evidence. But, as discussed above, archaeological theory lays great weight upon positive material evidence. Independent corroboration is not demanded if positive material evidence exists in defense of a behavioral reconstruction.

However, this argument only blunts the behavioral objection. Though corroboratory evidence may not be demanded in order to argue that cultivation was practised, it may be demanded for acceptance of the conclusion that the pollen has been properly identified as maize. In fact, corroboration is demanded and that is why observation by an independent observer was considered necessary. It is also why the opportunities to replicate the observation by analysis of a very large number of Horizon 6 samples were exploited. In any case, the observation of maize macrofossils could not corroborate the identification of the pollen as maize. That is, the occurrence of maize macrofossils in a deposit does not make the occurrence of maize microfossils any more or less likely, or vice versa. This matter is worth closer examination, since it seems to fly in the face of common sense. Also, it illustrates a methodological difference between the evaluation and interpretation of artifactual and non-artifactual remains.
Evaluation of the significance and meaning of artifactual materials in part depends upon acceptance of two principles. First, that recovery of different forms of artifacts which served the same cultural function will reinforce a reconstruction of that function. This is true because archaeological theory grants two assumptions: (1) the cultural functions (or functions and uses, to use Linton's (1936) precise terminology) of all materials recovered from a component context are interconnected in some way, and (2) each item recovered (or represented by a recovered fragment) fulfilled some cultural function. There are necessary assumptions to the practice of archaeological interpretation. If it were necessary to demonstrate in each and every case that the material items recovered fulfilled one or more cultural functions, most recovered items would be unanalyzable. The factual evidence and logical argument required to scientifically demonstrate that a given ceramic vessel or a particular chipped stone tool was actually utilized in the past is staggering. The archaeologist does not present it; instead the archaeologist grants the verity of the anthropological theory which supports use of these assumptions unless evidence exists to the contrary.

The biologist employs the assumption of a uniformitarian universe. However, the uniformity expressed is not in correspondence of relationship. Rather, it is in correspondence of the mechanisms which result in the apparent similarity between existing and pre-existing biological conditions. The co-occurrence of different types of fossils in a given context, then, is not assumed to be the result of a functional relationship between the different types of organisms observed. The pots, tools and structures of an archaeological component can be assumed to be members of a single functionally interrelated group simply because they are observed directly associated with
each other. The equids, camelids and proboscidiens of the Rancho LaBrean fauna or the taxa of a pollen spectrum cannot be assumed to be related to one another because they are directly associated. An argument of relationship must be presented. The co-occurrence of the biological remains, however, can be assumed to have resulted from taphonomic processes that may be understood and reconstructed from analysis of mechanisms which may be observed to act upon modern biological materials.

The co-occurrence of two different kinds of biological materials representing the same taxon in a fossil record, e.g. leaves and flowers or skin fragments and bone fragments, is not considered independent forms of evidence documenting the existence of the taxon or the credibility of the biological identification. Co-occurrence documents only that the processes by which the assemblage of fossils was formed, preserved and recovered were such that both kinds of materials survived from the past to be observable in the present. The lack of maize macrofossils in the Horizon 6 deposits does not, therefore, argue that the maize pollen is misidentified or intrusive to that context. It argues only that the processes which might have led to the observation of maize macrofossils in this context are not, in fact, evidenced. Human use of maize at the time of the deposition of the Horizon 6 flotation samples analyzed is one of those processes. But there are many, many others and some are interactive. What it comes down to is that the production, dispersal and preservation mechanisms by which carbonized maize macrofossils occur in archaeological contexts are not, on the whole, even grossly similar to those by which maize pollen microfossils occur. Even though both forms of this biological material might be subject to the same patterns of human behavioral
manipulation in some way - e.g. deliberate burial in trash-filled pits - such manipulation need not be responsible for the preserved material which was actually recovered.

Taphonomic processes being what they are, the empirical evidence accumulated by Asch and Asch suggests that the most reliable and productive archaeological context proveniences for recovery of carbonized plant macrofossils in Illinois Valley sites are the deposits of middens and the deposits in-filling features. The empirical evidence obtained from the Macoupin (Schoenwetter 1975), Apple Creek (Schoenwetter 1966) and Koster sites is that taphonomic processes affect the preservation of pollen in all types of cultural and non-cultural deposits at Illinois Valley archaeological sites about equally. The productivity and reliability of a sediment sample for pollen analysis seems far more related to its antiquity than any other single factor. In the Illinois Valley, the older a site context sediment sample is, the less likely it is to contain quantities of pollen or a large variety of pollen types.

5. The occurrence of maize pollen at Koster cannot be credibly interpreted as reflecting maize cultivation unless similarly ancient evidence of such cultivation occurs at other sites.

This objection can be treated superficially or seriously. On the superficial level one may counter with demands that the criteria of evidence adequate to overcome the objection should be identified. What sorts of "similarly ancient evidence of such cultivation" would prove acceptable? Is the existence of evidence of the similarly ancient cultivation of cucurbits - another exotic floristic element - adequate? Is palynological evidence of similarly ancient occurrence of maize adequate
if it derives from sites in other districts of the same culture area or other culture areas? Would similarly ancient maize pollen constitute evidence of cultivation of maize in an area if it came from a bog or lake deposit rather than site?

More seriously, one may recognize that the objection is founded on a legitimate problem of archaeological methodology. Anthropologically, the behavior pattern of the cultivation of exotic organisms tends to be linked on the one hand to processes of communication and information flow between communities, and on the other hand to economic systems which incorporate many diverse component variables and interrelationships. Insofar as archaeological records reflect anthropological realities, we have reason to expect that cultivation behavior which occurred in the past would express similarly complex cultural processes and systems. We would thus expect that archaeological evidence expressing a pattern of cultivation of exotic organisms would exist in a variety of forms, and that its distribution would not be limited to one locale. Further, the forms of archaeological evidence which express the occurrence of a behavior pattern of cultivation might reasonably be expected to express other characteristics of the processes of information flow existing between communities and the nature of an economic system in which the cultigens played some role.

The reasonableness and value of such expectations, however, derives from the fact that they are applicable to the "normal" (i.e. artifactual) evidence archaeologists evaluate. As I have mentioned earlier, one of the assumptive constructs upon which such evaluations are based is the idea that all the materials recovered from an archaeological component context are directly or indirectly related. Here the "component context" is not that of a single site; it is a pre-existing human population responsible for the existence of an archaeological record of a certain antiquity in a
certain territory. In the present case, the record involved is named Late Middle Archaic and the territory is the culture area named the Eastern Woodlands of North America. This territory is relevant because typological and stylistic artifactual evidence from this territory is conventionally identified as adequate to indicate that processes of information flow among communities did actually occur.

This assumptive construct allows the archaeologist to generate one or more anthropologically credible models of information flow processes and related economic systems which may be tested through analysis of the character and distribution of archaeological evidence. However, such a method cannot apply to a form of evidence which does not meet the demands of the assumptive construct. Artifactual evidence meets the requirement. Non-artifactual evidence does not. Maize pollen is not an artifact. Though its distribution in an archaeological context may depend on human behavior, its existence is a direct function of biological processes.

Man certainly conditioned the evolution of the organism we observe today as maize, and man must certainly have been responsible for the occurrence of maize in Illinois during the Helton phase of the Archaic. But maize pollen is not thereby identifiable as an artifactual record. Totally biological processes are responsible for its existence. Those processes must have occurred at or before the time of deposition of the Helton phase deposits at Koster. If the maize pollen observed in Horizon 6 is not intrusive those biological processes are unlikely to have occurred anywhere other than in the immediate environs of the Koster site at the time the Helton phase deposits were laid down.

The objection would be legitimate if the methods of interpretation applicable to biological fossils were exactly comperable to those applicable to the normal records of behavioral events, i.e. artifacts. Unfor-
tunately, this is not the case. The requirements of evidence in support of a plausible argument are different in the two instances, so what is reasonably demanded of artifactual evidence of human behavior is not reasonably demanded of non-artifactual evidence.

Non-artifactual evidence of human behavior is not required to meet the demands of anthropologically credible models. The maize pollen of Horizon 6 constitutes evidence of maize cultivation during the Helton phase at Kos- ter because (1) its identification as maize pollen has been adequately determined, (2) its occurrence in Horizon 6 samples has been replicated, (3) maize pollen contamination is demonstrably unlikely as a result of intrusion from some younger source, and (4) the maize plants that must have been responsible for the existence of this pollen could not have been other than cultivated exotics. The occurrence of as many pieces of quarried obsidian in the same component context would similarly reflect the existence of a behavioral pattern of long-distance exchange, if it could be shown that the material was properly identified and was not intrusive. It is the character of non-artifactual evidence which is basic to its behavioral evaluation, not the nature of its probable relationship to other evidence. It is the nature of its relationship to other evidence which is basic to the behavioral interpretation of artifactual evidence, not its character. A piece of chipped stone that has all of the morphological and physical attributes of a gun flint may or may not be interpretable as a gun flint. It depends on where it is found, how old it is, what is associated with it, etc. That is to say, it depends upon the anthropological credibility of the inferential model of behavior that supports such an assessment.
6. The maize pollen cannot represent cultivation at Koster during the Kelton phase, since such a behavior pattern cannot be reconciled with any credible subsistence-settlement system reconstructions provided for Archaic populations of the Illinois Valley or elsewhere in the Eastern Woodlands culture area.

Clearly, this objection is also based upon the characteristics of anthropologically credible models. But it is also based upon the availability of positive evidence supporting specific models of the significance of cultigens (e.g., Ford, 1981). Those models reconstruct the activities of Archaic cultures as almost wholly independent of cultivated plants. Fruits of the exotic plants *Cucurbita* (squash) and *Lagenaria* (bottle gourd) almost certainly functioned as containers rather than food resources for the ostensibly few populations which grew them. The native plants which may have been cultivated for food at the time (*Iva, Chenopodium, Polygonum, Helianthus* and *Phylaris*) clearly do not constitute a large enough fraction of the carbonized macrofossil plant remains record to evidence subsistence dependency, nor could they have made up a nutritionally crucial aspect of the normal Archaic diet (Asch and Asch 1980). Nor does any distributional analysis of the spatial relationship of Archaic habitation or limited activity sites to agriculturally productive lands indicate that maize production was a probable significant component variable in the system conditioning the character of residency or territorial patterns of Archaic populations.

What must be recognized here is that the subsistence-settlement reconstructions specifically reference models of the economic subsystems of Archaic horizon cultural models. Maize cultivation may have had far less economic significance than other cultigens; indeed it need not even have been cultivated because it was a food source at all. Anthropologically
credible models of Archaic cultural systems may be generated in which maize — whether it was or was not consumed as a foodstuff — was primarily significant as a symbol, a ritual material, a material requirement of sociological interactions (e.g. bride price or gifting behaviors), a decorative material, etc. That existing positively evidenced reconstructions of Archaic subsistence-settlement relationships and economic systems neither require the occurrence of maize nor suggest that any cultigens were critical economic resources is one matter. That the fossil record demonstrates that the Archaic population of Koster cultivated maize is a totally different matter. Neither serves either to confirm or deny the other.

SUMMARY

Various objections have been raised to the conclusion that the maize pollen of the Horizon 6 (Helton Phase) deposits of the Koster site document maize cultivation behavior. Basically, the objections fall into two classes: those which are based on assessments of the credibility of biological evidence and those which are based upon assessments of the evidence archaeologists normally employ when reconstructing behavioral patterns. Objections of the latter class seem relevant and reasonable to those not familiar with the methods of paleobotanical reconstruction or the assumptive constructs upon which they are based. The logic such reconstructions employ, however, even for reconstructions of a behavioral character — is not like that archaeologists employ when dealing with artifactual evidence. Relationships among the units of data recovered, for example, cannot and are not assumed to exist simply because the units are directly associated. What is at issue in such reconstructions is the interactive model of biological and behavioral processes that could credibly account for the
observations. Whether those behavioral processes are positively evidenced by other forms of data in archaeological record or are even anthropologically credible is not important. Objections of this class are, then, logically irrelevant. They literally do not competently address the problem.

Objections of the former class boil down to questions of the competency of the investigator in identifying the taxa involved and in obviating interpretation of contaminated samples. Questions of these sorts cannot be totally excluded, since human error is a constant possibility. Normally, such questions are resolved by identifying the precautions and controls used to reduce the probability of error and used to identify the occurrence of errors. Controls were employed to reduce identification error, precautions were employed to reduce field contamination, and the character of replicate observations of the maize pollen argues that errors of these sorts have a low probability of accounting for the record obtained.

The objection of statistical inadequacy falls into neither of these classes. But it fails the test of relevancy because it assumes a false premise: specifically, that all interpretation of palynological records must be evaluated by statistical standards. The applications of palynology with which archaeologists are most familiar (identification of prior vegetation patterns, identification of Holocene temporal divisions, and intra- and inter-site correlations) must be argued on a statistical basis because palynological theory requires a statistical standard to ensure credibility. But this does not exhaust the list of archaeological applications of pollen analysis. Behavioral reconstructions based upon palynological evidence may utilize statistical standards to enhance confidence (e.g. Pippin 1979:257ff), but is more commonly based upon an awareness of the contrasting probabilities of biological and behavioral processes to account for the presence of the taxon in the frequency observed. Thus
certain pollen taxa are normally identified as ethnobotanic in character (Bohrer 1972). When pursuing such traditional goals of pollen analysis as paleoclimatic reconstruction, some palynologists working with site-context samples exclude the statistical properties of ethnobotanic taxa from the analysis, much as the palynologist working with peat bog-context samples may exclude the statistical properties of pollen taxa uniquely adapted to bog habitats.

CONCLUSIONS

When the objections to the Helton Phase maize pollen of Koster are analyzed, a recognizable underlying issue is whether distinctive canons of evidence apply in demonstrating a behavioral conclusion on the basis of artifactual and non-artifactual data observations. Some canons of evidence seem to apply equally to non-artifactual and artifactual data which derives from an archaeological context. Observations (data) need not constitute evidence for the demonstration of a behavioral conclusion in either instance if the provenience attributed to them is questionable. Discounting deliberate or inadvertant human error, the issue is one of contamination in the case of pollen grains. However, recognition that the issue is labelled "contamination" constitutes only a very general appreciation of the specific canons that apply. The biological nature of pollen production and dispersal mechanisms are such that arguments presented to accept or reject the proposition that a palynological observation is a function of contamination are very much more relevant than is the normal case for artifactual observations. Contamination is one of the classic forms of error in pollen analytic research, and thus is one of the bases for establishment of exact procedures of work designed to allow identification of and compensation for error.
There is no particular reason why a non-palynologist should be expected to know how to evaluate the efficacy of those procedures, or even to know which should be used in a particular case. I have attempted to show that in the case of the maize pollen of Koster they may not be procedures that are richly endowed by "common sense" appreciations of how pollen study is accomplished. The most telling argument that the Koster maize pollen does not derive from local sources that are younger than the Helton Phase, for example, proceeds through comparisons of the general characteristics of the Helton Phase, older and younger pollen spectra. Such a comparison would be meaningless if a particular type of artifact were proposed as a contaminant, or at least would carry little logical weight. Analysis of the general contrasts between the Helton Phase and the Late Woodland occupations of Koster, for example, would not be a convincing way of demonstrating that a type of scraper which was common in the latter but rare in the former was a contaminant of the Helton Phase assemblage.

In the case of both artifactual and non-artifactual observations, data need not constitute evidence for the demonstration of a behavioral conclusion if the data have been misidentified. But identification of artifactual remains does not demand application of the uniformitarian assumption, and identification of biological materials does. What independent, knowledgeable, observers agree looks like a basketry fragment cannot be assumed to in fact be a basketry fragment in the absence of evidence to the contrary. Positive argument must be presented that the attributes of the item are analogous to those of basketry fragments. If independent knowledgeable observers agree that the thing observed looks like a maize pollen grain, it must be assumed to be one in the absence of arguments that explain why it looks like one but is not.
The numerical qualities of observations of non-artifacts which constitute canons of evidence may also be different from those relevant to artifacts. There is a far greater demand for replicability in the former, for example. This is particularly true for pollen grains, since it has long been acknowledged that stochastic variation in the processes of pollen production, dispersal and preservation can produce very unexpected observations. Colloquially, one swallow does not a summer make. But

This does not mean that statistical standards must thus be accepted as the basis for judging non-artifactual observations as reliable data for drawing behavioral conclusions. 20 or 2,000 swallows need not make a summer either. Statistical standards are properly identified and applied in relation to problems. Some problems demand them, others do not.

The canons of corroboratory evidence are also different in some significant ways. With artifactual data, the direct or indirect association of observations of a different type indicating the same sort of behavior constitutes a strong, independent, basis for evaluating the first observations as evidence for a behavioral conclusion. A very great proportion of archaeological procedures of data recovery and analysis, in fact, are designed to discern corroboratory data of this sort. The degree of relevance of independent but co-occurring corroboratory evidence is different in the case of non-artifactual materials. What tends to be of concern is co-occurring observations that corroborate the identification and provenience of non-artifactual data, rather than those which corroborate the behavioral inference.

In part, this distinction seems to be a function of assumptions which
are made in archaeological theory about the roles of artifacts in culture and society. Biological theory makes no such assumptions, so the co-occurrence of two types of fossils of the same taxon does not necessarily indicate any behavioral processes. It documents only that the processes responsible for the recovery of the two types of fossils were of a certain order.

Finally, there are very sensible reasons why artifactual data may be rejected as evidence for a specific reconstruction of human behavior if that reconstruction is not concordant with the requirements of anthropologically credible models. Lack of concordance with an anthropologically credible model is not an acceptable basis for rejection of a non-artifactual data record as evidence of a behavioral pattern, though lack of concordance with a biological model or physical model is. If it could be argued on biological grounds that maize plants could have grown and reproduced at Koster without any human intervention, the behavioral conclusion that the maize pollen observed evidences cultivation would fail to meet a legitimate canon of evidence. The conclusion would not necessarily be dismissable, but it would be recognizably weaker than it is at present.

Not too long ago, I concluded that archaeologists interested in Eastern Woodlands prehistory had ignored the palynological evidence of Archaic maize cultivation at Koster because they were basically unacquainted with the nature of such evidence and the requirements of its interpretation.

This conclusion was rebutted by two arguments: (1) that the palynological evidence involved had not been satisfactorily presented to those who desired to evaluate it independently, and (2) the evidence had evidently been discounted by ethnobiologists (Yarnell 1976, Struever and Vickery 1973). Not to put too fine a point on it, both my conclusion and the arguments
of rebuttal are probably correct. In this essay I have tried to both satisfy the demand for pertinent factual details and to identify the reasons for refuting objections I have heard to the inference that the Helton Phase occupants of the Koster site cultivated maize. The point of the work, though, has not been either counter-rebuttal or demonstration that the objections are invalid. The point has been to illustrate methodological and theoretical contrasts between use of palynological and artifactual evidence as the scientific grounds of archaeological conclusions. Instruction in such matters must be more common in archaeological literature if the discipline is to fulfill its promise of multidisciplinary investigation and explanation of the character of prior cultural systems. Comprehension of the potentials and limitations of the techniques of other disciplines which have demonstrable archaeological value is crucially important, but it is not totally adequate. Evaluation of the verity of conclusions developed by application of those techniques also demands some degree of methodological and theoretical sophistication.

Why archaeologists who emphasize the necessity of comprehension of anthropological theory and methodology for proper application of archaeological technology should resist recognition of this general principle is a question I have never been able to answer. Yet archaeologists who diligently demand methodological sophistication of themselves and of students in the areas of statistics, computer science, spatial analytic techniques adopted from human geography, petrography, and museum conservation will make no similar demands where geomorphic - stratigraphic analyses, ethnobiological analyses, paleoecological analyses or chronometric analyses are involved. I am, of course, fully aware that archaeologists cannot be equally sophisticated and proficient in the application of all,
or even the majority, of the technology relevant to archaeological interpretation. This situation will predictably worsen significantly in the near future. Even excavation and survey technology is developing at such a rapid pace, and is becoming so fine-tuned to the logistical demands of different geographical districts, that the average practising professional is now expected to require the expertise of locally proficient personnel even to initiate competent effort. One way to counter the disadvantages of this trend is to provide archaeologists with instruction in the methodological structures underlying a wider variety of applicable technologies, while reducing emphasis on instruction of the applications the techniques can and have served in archaeology. This will provide the average practitioner grounds for evaluation of the results of the effort and a medium of legitimate communication with the specialist who undertakes it. The disadvantage of this strategy is that the average practitioner will have to accept the technical proficiency of the specialist as an article of faith unless he has himself undergone technical training. But that disadvantage is already evident. There is a totally different plausible approach to resolution of this problem. Though I have given some public consideration to its character (Schoenwetter 1981), it remains theoretically immature as yet.

Reviewers of early drafts of this essay have advised me that it is legitimate to view the distinctions in canons of evidence I have discussed as matters of degree rather than kind. I agree; such a stance is logically legitimate. On the one hand, this acknowledgement in no way requires that consideration of the distinctions as matters of kind is thus to be viewed as illegitimate. I'll allow you your philosophy of science if you'll allow me mine. On the other hand, and more importantly, how we decide to view the distinction is not a significant concern of this work. My discourse
has been designed to document the proposition that the canons of evidence are distinctive, to instruct workers on how those distinctions affect inference in this case, and to examine what I understand to be the methodological structures which create those distinctions. Those are the matters upon which I encourage and invite debate by publishing this work.

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1. Acute readers may note a discrepancy between the information of Figure 1 and that provided for Square 8 by Asch and Asch (1980:26). The depth of Square 8 indicated on Figure 1 is 28.25 feet (9.3 m) below the base of the plow zone, while Asch and Asch note the depth of Square 8 as 9.8 meters (30 feet) below surface. Asch and Asch recognize the existence of Horizons 3, 7A, 12 and 13 at Square 8 which are not recognized on Figure 1. Alternatively, Figure 1 recognizes the existence of Horizons 7B, Sub 9 and divisions of Horizon 1 which are not recognized by Asch and Asch.

While real, the discrepancies are not relevant to this analysis. Asch and Asch may have failed to accommodate the fact that Houart removed the plow zone of Square 8 as a unit before establishing the first of her arbitrary excavation levels (Houart 1971:3). The cultural horizons identified in Square 8, then, proceed from the base of the plowed zone and not from the original surface. The depth at which Asch and Asch identify the upper limit of Horizon 6 (2.0 m below surface) is equivalent to the depth of the uppermost sediment sample assigned to that horizon on Figure 1 (120 inches, or 10.0 feet); however, is not equivalent to the depth recognized by Asch and Asch (3.5 m, roughly 127.5 inches or 10.63 feet). This apparently occurs because Asch and Asch evaluate the density of debris from Houart's test differently from the way in which Brown evaluated it when preparing the stratigraphic column of Figure 1 in 1978. The absolute difference in depth (9.8 m versus 9.3 m) probably represents a distinction between the depth of deposits excavated and the depth of deposits sampled for pollen analysis.

3. Numerical relationships are also botanically significant. The fact that the identification of the pollen grains in question could be replicated independently is crucially important to the argument that misidentification of some other palynomorph as maize did not occur. Replication in segregate and independent samples is also a crucial aspect of the argument that the maize observations are not the result of contamination. However, the numerical relationship of replication not required to meet statistical standards.

4. Asch and Asch (1980:61ff) report the recovery of carbonized cucurbit rind fragments in 5 excavation units at Koster which pre-date Horizon 6, as well as in Horizon 6 itself and in Helton Phase deposits at the nearby Napoleon Hollow site. They consider the identification secure, but are unwilling to conclude that the available evidence is adequate to exclude the possibility of contamination or the possibility that the cucurbit observed is not an exotic cultigen. Archaic camp occupation, dated by the occurrence of concave base points similar to the Jakie stemmed type, in the Ozark district of Missouri (Anderson, 1980).

5. Both maize pollen and Cucurbita pollen have been observed in sediment samples collected from an extension of test pit E in the vestibule deposits of Salts Cave, KY (Schoenwetter 1980). A single grain of each taxon was observed in each of two samples. No radiocarbon dates or temporally diagnostic artifact types were directly associated with either sample. The natural stratigraphy of the sampled strata, however, is correlated by Watson (pers. comm.) with strata which underlie those representing the Early Woodland occupancy of the site. Correlative strata at another test
are radiocarbon dated to the Archaic period (Watson 1974).

6. Maize pollen has been observed in the lacustrine context of Dismal Swamp, VA in a pollen spectrum which dates approximately 2000 BP (Whitehead 1965, 1972). The absolute antiquity of this record post-dates the Helton phase, but it pre-dates the local establishment of Early Woodland occupations. Sediment samples from Early Woodland site context deposits in Georgia have produced maize pollen associated with maize macrofossils (Wood 1979). A single grain of maize pollen has also been recovered from the non-site context deposits of Phillips Spring (King 1978) in a stratum which correlates geologically with the stratum that produced Archaic squash rind and seeds (Chomko and Crawford 1978).