BO-550 - PALYNOLOGY

"Pollen Analysis of Quaternary Sediments, Particularly in the American Southwest"

by

Dr. James Schoenwetter,
Dept. of Anthropology, ASU

1. Pollen analysis was invented for purposes of comprehension of Quaternary events: paleoclimatic reconstruction, and establishment of horizon markers--fossil indices--to allow dating. With the advent of C_{14}, the latter purpose has become essentially undermined, as correlation can never be as adequate as absolute dating. However, it is critically important to realize the original dual function of pollen analysis, because the method that became traditional was designed to fulfill both of these functions.

Paleoclimatic reconstructions of the conditions succeeding the Würm (European glacial stage) maximum had already been developed in the 1st decade of this century on the basis of interpretation of geomorphology and lithology. This was the Blytt-Sernander hypothesis of a 3-stage series: cool wet-warm dry-present. Von Post literally invented pollen analysis to determine if this hypothesis was valid and if it needed modification. Pollen had been known to be preserved in organic sediments, particularly peat. The problem was to determine a means of evaluating the preserved pollen in paleoclimatic terms. The method Von Post championed was a statistical one. Rather than use one or two pollen types as index fossils, he used the relative proportions of pollen in the sample as a total unit, showing change through time. This is sensible in light of our knowledge of pollen production and dispersal.

But statistics is a tricky game--and one far less comprehended at that time. How should the proportions of pollen be evaluated? What does a proportion mean? What is the universe of analysis?

Because they were concerned with changes in climate as a test of the Blytt-Sernander hypothesis, Von Post and his students (including Erdtman) chose to deal with the universe of analysis called the FOREST TYPE. If the climate had changed as radically as the hypothesis stated, then there should have been changes in the gross forest composition. An area now clothed in pine forest should have been clothed in spruce forest at some colder time or oak forest at some warmer time. This reconstruction would only be true if some critical degree of greater cold or warmness had actually occurred. Von Post knew full well that it takes a relatively major temperature change to change the character of a whole forest--- but this was exactly the point. The Blytt-Sernander hypothesis was arguing that such large temperature changes had taken place in post-Würm time, and this was the point at which doubt arose.

Knowing that his problem was the forest type, Von Post designed his method to suit that problem. Specifically, he emphasized the value of arboreal pollen by statistical means and de-emphasized the non-arboreal pollen. He simply dropped the NAP out of the pollen sum on which the percentages were based. Anyway, it worked. Von Post was able to show that temperature changes on the order of those presumed by the Blytt-Sernander hypothesis were reflected in the pollen record trapped in European peat bogs. Beyond that, pollen analysis of the pre-existing European forests illustrated that the B-S reconstruction was a gross one. More subtle changes in forest composition could be defined and interpreted in terms of more subtle climatic changes.

Pollen analysis gave the geologist of the Quaternary a very valuable stratigraphic tool. When the forest sequence was recognized to be consistent, pollen spectra were obviously usable as index fossils of the same order as vertebrate and invertebrate fossils. There was one complication—-because of the differing environmental conditions existing in e.g., southern France and Denmark by virtue of their different soils and latitudes, it could not be expected that pollen spectra of the same age in the two regions would be identical. When it was cold enough to produce spruce forests in Denmark, it was only cold enough to produce pine forests in So. France. It took awhile to realize this because ecological knowledge was not very secure before 1940, but it was finally worked out by Harry Godwin of Great Britain.

Pollen analysis became essentially a tool of those interested in the Quaternary. This was because: (a) it worked best with angiosperm microspores, (b) it worked best with organic deposits, which are rarely preserved from Tertiary contexts (in fact rarely preserved from Pre-Würm-Wisconsin contexts) and (c) one could more reasonably assume that the ecological meaning of a taxon-say pine or oak-from the Quaternary was essentially similar to that taxon's ecological meaning today. If you found walnut pollen in the Tertiary, there was always reasonable doubt that the ecology of a possibly extinct species of walnut could be meaningfully understood.

2. Pollen analysis in the New World trailed behind European work until the early '50's. It was applied to the same kind of sediments and used the same methods for the same purposes as those of European scholars. Paul Sears is the pollen analysts' American "man-of-distinction;" the man who did for US pollen work what Von Post and Faegri did for European work. Sear's stimulus came from a desire to determine when the prairie peninsula reached Ohio and became established there. Through the work of Sears and Deevey and their students it was found that the Eastern US pollen sequence roughly paralleled that of Europe. There were differences, but relatively minor ones.

U.S. work in the early 40's began to deviate from the traditional pollen analytical programs of Europe in 3 directions: (a) more work on less organic matrices, (b) more stress on forms of statistical operation with the data, (c) more work on older Quaternary and Tertiary sediments. The war disrupted pollen studies everywhere until the early '50's.

3. Pollen analysis in the semi-arid lands of the New World began with the 1936 publication by Sears which demonstrated that pollen was preserved in alluvium at Kayenta, Arizona. The samples were collected by Antevs, who at that time (along with Kirk Bryan) was applying geological knowledge to the problems of paleoclimatic reconstruction in arid lands. It is Bryan and Antevs who established the Pluvial-Anathermal-Altithermal-Medithermal sequence--essentially the Blytt-Sernander hypothesis as applied to the American Southwest. In 1955 Anderson published a paper on the existence of pollen in S.W. cave sediments. In that year and in 1955 Sears and Deevey published on pollen cores from highland Mexico; in 1956 Kurtz and Turner of the Univ. of Arizona proposed the oil floatation extraction technique for inorganic sediments.

In 1957 Dr. Paul S. Martin of the U. of Ariz. began his investigations which would ultimately lead to the 1963 publication: "The Last 10,000 Years." I was lucky enough to be part of that research program in its early days. Martin's interests were distinct from those of traditional pollen analysis. Radiocarbon dating had superceded pollen analysis as a dating tool, and had already been used to establish the ages of alluvial units in the S.W. Martin had worked in the Cloud Forests of Tamaulipas, Mexico, investigating reptiles and amphibians. He knew that the genera and some species of Cloud Forest trees were identical to those of the eastern US woodlands, but the genera and species of most of the fauna were distinctive. His interest in pollen analysis was as a means of discovering when the Tamaulipan flora and the Eastern US flora might last have been incontact. He thought it would have been late in the Quaternary. While SE Arizona might not have been the route of plant contact, the allurvial sediments of the SW were known to be datable and to contain pollen. He started to work on them, figuring that any climatic change capable of joining the Tamaulipan and US floras would be reflected in the desert pollen record.

As we started to investigate the post-pluvial record, we were at first surprised to find very little arboreal pollen. Traditional pollen analysis depended on AP and we just didn't have it available to us. But we did find plenty of AP in the plurvial records--particularly at the Wilcox playa.

What did the lack of AP mean? To determine this Martin instituted a program of surface sampling. We were able to demonstrate on the basis of a number of different sorts of samples of the modern pollen rain that low AP values were commensurate with an ecological environment lacking trees. At high elevations we had trees and AP. The conclusion was obvious: since the end of the last plurvial, the arid SW has been arid and essentially treeless, except at higher elevations. Unlike Europe and the Eastern US, there has been no essential climatic change.

Our position was immediately attacked by Antevs, who argues that our palynological records were not representative of paleoclimatic conditions because they did not agree with the reconstruction offered on geological grounds. Martin and his students, essentially myself, R. H. Hevly, P. J. Mehringer and D. Adams have not formally argued the matter further. We state our evidence and draw our conclusions. We, and also C.V. Haynes (a geologist) feel that the data are not in conflict, only the interpretations. The point here is that if we are correct and there has been no post-glacial climatic change in the American Southwest as there has been in other areas, the principle might very well apply to other time horizons. The stratigrapher trying to correlate across great expanses of the globe will be in trouble if climate in one area does not change when climate in another area does. Thus, the Southwestern pollen analysts' conclusions strike deep into the theoretical position underlying traditional pollen analysis. We question the value of pollen analysis in all cases for index fossils, and we rely on C_{14} rather than correlation for dating purposes. What have we found out? We haven't been at it very long, and there are few of us—but look at the available record:

1) By use of well-controlled surface samples, we have become able to recognize variations in the pollen record which are not due to climate change, but are due to distinctions in moisture balance through time. Mehringer's article in the Feb. 1967 issue of Kiv shows these for the post-Wisconsin period. It turns out

that these moisture fluctuations can be correlated over broad reaches of geography. A moisture increase found at Tule Springs, Nevada occurs at the same time as one recorded in SE Arizona and one recorded in the Parnamint Valley. The correlation is supported by radiocarbon dating, and we see this in a number of cases in the post-Wisconsin record.

2) The question of climatic fluctuation has been thus shown to be something of a moot point. We can demonstrate that environmental changes affecting broad areas of the desert lands occurred---who cares if we call them "climatic" so long as we learn what they are!

3) The records of the rest of the Quaternary are beginning to come in, and we can't realy make head nor tail out of them. It is apparent--see Martin (1963, and 1965)--from comparison with surface samples that since the Illinoian the flora of the SW has been an essentially Southwestern semi-arid flora. There is no Eastern Woodland or California Coast pollen spectrum in the record. But what the vegetation reconstructions mean, i.e., what climatic factors have kept the SW flora stable, is a question we really can't answer.

4) Southwestern work has demonstrated the practicability of analysis of highly inorganic sediments. It takes special techniques and it is necessary to accept more gross taxonomic identification, but it can be done and you can get consistant results.

5) Southwestern work has demonstrated the extreme value of surface samples as controls on vegetation reconstruction. We've learned that is is very hard to outguess a pollen spectrum. The center of the Wilcox Playa--miles from any pine and absolutely devoid of vegetation--gives a surface pollen record of

about 20% pine. The edge of the playa, yielding a sparse growth of the chenopod Suedia gives only 10% pine (or less) even though it is closer to the sources. We've learned that one of the best indices of dry conditions in N. New Mexico is grass pollen. A cactus-studded landscape here in Southern Arizona does not deposit much cactus or creosote bush pollen on the surface of the ground. Mostly you get Amaranthus (pigweed) and Fransaria (burro bush) pollen and grass pollen, because these are the local wind-pollinated producers.