

Botany and the Archaeologist: A Critical Assessment

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I'm afraid that I'm going to disappoint a large segment of this audience today, because I'm not going to speak on the topic expected of me. In the letters sent me by the organizers of this symposium, the clear intent was that I would "handle" some statement on the value limitations of the use of paleocological reconstructions offered the archaeologist by Botany. As one who calls himself an archaeologist, who teaches in a department of anthropology, and who holds a graduate degree in Botany, I am very much aware of the need archaeologists have for such a statement. I know first-hand how confusing the reports of the dendroclimatologist, the specialist who deals with diatoms, the pollen analyst or the man who identifies seeds and other floral remains are to the someone trained in the social sciences.

Those archaeologists who are not confused by such reports, those who feel confident that the specialists' work can be neatly plugged into the data bank upon which archaeologists draw for information to support their theories and hypotheses, are either geniuses or incredibly naive. Paleoecological reconstruction by virtue of botanical data is no more simple than cultural reconstruction by virtue of artifactual data. The paleobotanist ordinarily has about as many biases affecting his work, and about as large a void in his matrix of reliable data, as does the archaeologist. Both face similar problems in regard to the theory and methodology of comparative analysis, and both have to contend with questions of parallelism - which the paleobotanist handles under shelter of the concept of uniformitarianism and the archaeologist struggles with under the rubric of ethnographic analogy. Beyond this, the paleoecologist, like the cultural historian, is concerned with relationships rather than

with data units themselves. This concern leads the paleobotanist into just as murky waters as it leads the archaeologist.

I have chosen not to speak to the direct topic of this symposium precisely because the majority of us are neither geniuses nor naive but simply confused. The point I wish to make is that this confusion has arisen out of a misplaced confidence that if we just learn a little more about the topic enlightenment will surely follow. Perhaps I judge too much by my own experience, but I haven't found that an understanding of the results of paleobotanical research, or even an understanding of the potentials and limitations of such research, has really been of that much help to me as an archaeologist. It has been very important to me to understand the botanical principles upon which interpretations are based and upon which the techniques of paleobotanical study are based. But I'm not prepared to list and document those here.

The most profitable problems to attempt to cope with, I believe, are methodological ones. I prefer to ask (in essence) not "what do we know," but "how do we find out?" So I will not speak to the primary topic of this symposium, which is a way to answer the question "what do we know." Instead I will ask "how do we find out" and address myself principally to this matter in respect to botanical techniques of paleoecological study.

Most archaeologists find out about past plant ecological matters by calling in a specialist and allowing him to research the matter. This is proper because archaeologists are trained as social scientists, to whom the laboratory experiments and technology of the botanist are mysteries as arcane as those of the astrologer or warlock. But is it a good idea? The archaeologist - at least the majority of archaeologists

in this country - says he is interested in culture. The botanical specialist, for the most part, is no more trained in analysis of cultural problems than the archaeologist is trained in analysis of botanical ones. Is it judicious for an archaeologist to place his cultural problem in the hands of one untrained in such matters?

And I am convinced that though the work of research must ordinarily be accomplished by one trained in the techniques and methodology of the natural sciences, the problems that the archaeologist wishes studied are not biological but social. And this is necessary because the point of archaeological study is the investigation of only certain of the phenomena and conditions of Nature in the past: those which have demonstrable effect on culture and which are causes, indices of, or effects of cultural processes and activities.

The archaeologist is not a universal historian of the past who is challenged to chronicle every natural event. He is a student of the cultural conditions of the past. He does not dig where there are no sites, but at loci where people have left evidence of their actions; he does not analyze non-artifacts, except insofar as such analysis will shed light on the nature of artifacts; he does not synthesize reconstructions of the world as it formerly was, but only those aspects of the past which critically relate to the story of mankind and groups of men. And by these lights, the archaeologist is not much concerned with the nature of pre-existing climate except insofar as it might be a matter of some critical importance to his cultural reconstructions; he is not overly involved with the nature of ancient patterns of floral variation unless these ancient patterns illustrate and illuminate cultural questions

or shed light upon matters which will help resolve cultural questions.

The paleobotanist is (a) not overly interested in ancient culture, (b) not trained to recognize any but the most obvious and least subtle of cultural matters when he faces them. The paleobotanist must undertake the task of research on cultural problems when botanical evidence is the stuff which must be analyzed. But the problems as such are not those of his discipline and in large part he is probably not trained to the task of their recognition, evaluation, or resolution. No more, say, than the archaeologist is trained to the task of recognition, evaluation, and resolution of problems in the field of phytosociological mapping in the Late-glacial period.

The archaeologist who relies on the paleobotanist to handle the entirety of the job of phytogeographic reconstruction will not be disappointed. He will ultimately receive a report which incorporates much information of archaeological value. But it is not the kind of report that might have been obtained if a truly interdisciplinary study had been undertaken. One way to do this is to begin with a clear formulation of the nature of the cultural questions which the archaeologist wishes to research. In a recent study in which I was engaged this question was: what technological responses might primitive agriculturalists make to environmental fluctuations affecting the productivity of their fields?

As stated, this problem quickly broke down into a series of related research questions:

(1) What environmental fluctuations do affect agricultural productivity in the region today, and in what ways may we recover evidence of such fluctuations for different times in the past?

(2) What are the minimal and maximal effects on agricultural productivity expectable from use of the technological equipment the record shows was actually used? It was critical that this question be resolved, since we might otherwise have confused the evidence for an environmental fluctuation with that for a technological change.

(3) What spatial and temporal restrictions and limitations should be imposed on the processes of data gathering, and what limitations be imposed by our current knowledge? In effect, we were asking if this problem was capable of resolution given existing and foreseeable logistic difficulties. We were also asking ourselves just what sorts of data we needed to assess that had already been worked with or needed to be garnered.

(4) To what degree - quantitatively or qualitatively assessed - would simple correlation of technological and environmental variation serve as an index to resolution of the problem? If we found a negative correlation between technological change and environmental change would it be more meaningful than a positive correlation or a correlation that was neither positive nor negative? In effect, we were asking whether or not our original problem was a sufficiently sophisticated one to be worth all the effort we were willing to invest. All too often, problems are clearly formulated but not very much worth the effort. Grahame Clark found this out at Starr Carr. He had a lot of pollen study done which was designed to resolve the question of the nature of the forest environment occurring in the site area at the time of occupation. The answer came back: this was birch forest. So what?

Now breaking the original problem down into a series of research

questions helped us clarify what kinds of information we were looking for and edged us a bit further from a strictly cultural perspective to one which more fully outlined the biological matters of direct relevance. At this juncture, we began to rephrase the research questions in a biological frame of reference which was amenable to investigation through the techniques of pollen analysis. Where we had asked "what environmental fluctuations affect agricultural productivity in the area today" we now asked:

(a) what phytogeographic variations that occur in the area today are expectably due to environmental factors which would affect agricultural productivity

(b) what sort of pollen spectrum identifies these phytogeographic variations today and might expectably identify them in the past

(c) can the identifying pollen spectrum be affected by environmental factors which do not affect agricultural productivity in fashions similar to those which do affect such productivity.

These last questions could be researched and resolved through application of the paleobotanica] technique of pollen analysis. They are not questions towards which this technique had been applied previously, because the traditional purpose of pollen analysis is to obtain information regarding climatic change, and these questions do not require information about climatic change for their resolution. A biologically-trained pollen analyst would not normally have asked these questions, and would not normally have systematized his study to resolve them. He would thus not have arrived at the collection of data necessary for the resolution of the stated cultural problem.

Though it may sound that way, I have not exemplified the methodological problem by drawing on my own experience in order to toot my own horn. Nor do I do so simply to illustrate to you that an archaeologist who himself is trained in paleobotany can hope to accomplish more in regard to interdisciplinary research than one who is not, for I don't really believe that is true. I'm attempting to point out that before a trained paleobotanist can be expected to work profitably on cultural problems, the archaeologist - working with or without the benefit of his specialist experience - must be expected to transpose those cultural problems into a meaningful set of biological research questions that can be expectably resolved by the paleobotanist's techniques. I put the burden of work in this regard squarely on the archaeologist's shoulders because the problems, after all, are cultural ones. It is the archaeologist who is trained in the recognition, evaluation, and resolution of such problems. The fact that he needs the specialist's help does not allow him to shirk his own responsibilities.

I must wearily admit that the hardest part of the job is neither the development of the cultural problem nor the task of answering the research questions - a task which in this case falls to the paleobotanist but which may just as well fall to the geologist or the zoologist. The toughest part is the job of interdisciplinary communication that is necessary for translating the cultural problem into meaningful paleobotanical terms. The difficulty is not that the botanist and the archaeologist do not speak the same language, but that they do not mean the same things by the terms they use in speaking with one another. Amongst themselves, archaeologists are well aware of the problems of typological

classification which subtend the generalizations of definition of archaeological cultures. By virtue of extensive training and experience we have learned the proper dose of salt to apply to the meal served up by the synthesist of culture history. The botanist has not had this training and experience. He has no reason to expect that a scientifically defined culture does not have the same straight forwardness as a scientifically defined physical particle or chemical element. He will not know that any existing archaeological cultural taxonomy is as full of holes as is a Swiss Cheese unless he is informed. Alternatively, few archaeologists are aware of the equally shaky underpinnings of botanical studies in ecology and phytogeography, no less botanical taxonomy. The feeling is widespread amongst social scientists that natural science is a good deal firmer bed to lie in. I have been in that bed, gentlemen, and it is not. To speak momentarily to the title of this symposium, one of the pitfalls in archaeological paleoecology revolves around the communications gap. This is evidently a matter of education - a matter which works to the detriment of students on both sides of the disciplinary boundary.

To return to my own topic "How do we find out about archaeological paleobotany?" I answer first, we learn to communicate in a really significant fashion with our specialist colleagues and second, we develop sufficiently vigorous methods of study. By significantly vigorous, I mean methods which are appropriately designed to the task. We presently have all too few such methods. Rather, we generally are content to accept the crumbs which fall from the paleobotanical table that happen to be edible. That repast was prepared by methods designed to resolve biological problems.

If we devise methods which actually are of archaeological paleoecology and which resolve cultural problems, we may be able to sit down to a meal of our own.

As a postscript, I might mention that I have little personal confidence that the methods we may design today will allow the setting of a very elaborate bill of fare. I think that little truly worthwhile information has yet been garnered by contrast to that which could be recognized once we begin refining our methodology. But I foresee a number of decades of highly exploratory work before any real level of sophistication will be ordinarily reached. For example, our most sophisticated work in this field is yet descriptive rather than explanatory - as the work of Waterbolk - and is still tied rather firmly to descriptions of pre-existing environments rather like those of the modern environment written by 19th and early 20th century Natural Historians. We have a long way to go to achieve the quantitative sophistication of ecological studies of the 1950's or the qualitative sophistication subtending ecological studies of the late 20's to late 40's. Something as high-toned as the principle of vegetative successions to climax seems crude to the modern biological ecologist but far beyond the level of analysis yet reached in archaeological paleoecology. Here is, of course, the exact situation which I find most stimulating. The challenges of developing archaeological research methods in paleoecology are of such massive scope, and yet of such extreme interest, that I find I must rise to them.