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GUIDEPOSTS IN ETHNOBOTANY

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ABSTRACT.—A review of the history of a natural system of maize classification is provided. Detours in the archaeological analysis of maize abound. Tests suggest the extrapolation of cob row number from kernel angles lacks predictive value. Flotation analysis is forging ahead. Current trends in regional studies might be better integrated with anthropological model construction or destruction. Charcoal identification merits improvement through use of metallurgical microscopes coupled with more rigorous use of plant anatomy. Analysis of leaf epidermal fragments in coprolites broadens the range of information recovered. The biotic factor, including the anthropogenic one, needs to be considered as seriously as climate and soil in the formation of prehistoric and modern plant assemblages. Ethnobotanical interpretation of prehistoric plant remains benefits by insights into the kaleidescopic patterns formed by modern and historic interrelationships between plants and man. It is never too late for contemporary studies. Museums rather than colleges or universities continue to provide workspace for ethnobotanists. Suggestions are given in regard to academic education in ethnobotany and exemplary books and reports in the field.

INTRODUCTION

GUIDEPOSTS IN ETHNOBOTANY

Ethnobotany is flying high in the 1980s. The first issue of the *Journal of Ethnobiology* was published in 1981; the first general session on ethnobotany at the annual meetings of the Society of American Archaeology was in 1983; and in May, 1985, after settling into my seat on Southwest Airlines Flight 192, I was pleased to notice that the cover story of their flight magazine featured the contemporary ethnobotanical work of Dr. Richard Felger in northwestern Mexico and southern Arizona. That same August, *Forbes* magazine featured an article on the applications of pollen analysis (Teitelman 1985). Apparently ethnobotany has emerged from the low visibility of a pioneer discipline into a new phase of prominence.

Because your good editor suggested a historical perspective might be valuable, I wrote the first two sections on Maize and Flotation chronologically, but my initial resolve to follow this approach lessened by the time I reached the next section on Charcoal and Fecal Analysis and evaporated by the time I wrote Changes in Plant Life. Do not be misled by these initial digressions into history, for the pages that follow have taken another more personal orientation. I have participated in ethnobotany from the time when it comprised a handful of people, and witnessed with mixed emotions a series of changes in the field. The following pages typify my rejoicing over examples of fine work and brooding about areas that seem deficient. I regret that I can not know and reference all the best efforts in ethnobotany. On the other hand, I find it judicious to avoid citing what is less than satisfactory. The recent increased prominence of ethnobotany carries an increased responsibility toward producing a quality product; some sort of guide seems especially necessary right now. Lest you misunderstand, be assured that I admire the achievements of those who have pioneered in learning about maize, flotation, charcoal, fecal analysis, environmental reconstruction and ethnobotanical interpretation. Nor are you to assume that I am setting myself up as an oracle for ethnobotany. I expect some disagreement with my judgment. A discourse such as this will perhaps stimulate a dialogue for improvement within the profession so that we can avoid, in spite of the meteoric growth of our discipline, a hollow center leaving us too vulnerable to withstand criticism from outside.

MAIZE

Southwestern Pueblo Indian maize was among the first to be carefully sampled and classified according to a natural system (Anderson and Cutler 1942). A natural system relies on the ability to establish genetic relationships and trace origins. In the course of Anderson's self-directed study of maize he learned to depict variability important in the analysis of introgressive hybridization by means of a pictorialized scatter diagram (Anderson 1946:175). In the first version developed for southwestern maize (Carter and Anderson 1945) each point on the scatter diagram registered index numbers that summed data rather than recorded the primary measurements on each plant. In Anderson's subsequent publication (1946) the system was perfected by employing row number on the *x*- and kernel width on the *y* axis. The small circle or dot representing the intersection of values was proportionately elongated to illustrate the amount of kernel pointing, and shaded to correspond to the degree of kernel denting. Both row number and kernel width were used because they were end products of many genes acting in concert. For example high row number, narrow kernels and pointed kernels tended to be inherited together. By choosing single characteristics that were the expression of multiple genes, Anderson could sit in a farm yard and measure the salient features of a cob by making a single mark on the appropriate intersection of his scatter diagram which represented all three observations simultaneously. He could average the data for all maize ears in a village. Later, one could derive average values for all villages belonging to a tribe, and for all tribes belonging to a linguistic group. With such economy of effort not only Pueblo Indian maize but all maize of Mexico could be classified in the hope of tracing its northward migration.

In the 1930s long before Anderson developed an interest in maize, Carl Sauer, Professor of Geography, University of California at Berkeley, became interested in documenting the varieties of corn grown by Indian tribes in remote locations in northwestern Mexico. Isabel Kelly, one of Sauer's students, provided Anderson with critical collections of chapalote corn from Culiacan and Sinaloa and of the related maize reventador (Anderson 1944) from Jalisco and Zacatecas. Both races proved an essential modern link in understanding prehistoric maize in the southwest. Anderson combined his keen perceptions of morphology with his special interest in multiple gene complexes to provide informed speculation that maize reventador represented an old and primitive race (Anderson 1944:309). Later, the related chapalote race was thought more important (Wellhausen *et al.* 1952:57). The magnitude of Anderson's contribution to the understanding of maize in Mexico is apparent when we realize that ten of the races he delineated were incorporated into a total of 25 races recognized for Mexico (Wellhausen 1952:13).

When the descriptions of Mexican maize races were published in 1952 their correlation with certain patterns of distribution of chromosome knobs were known. For example, all those called "ancient indigenous races" had low chromosome knob numbers. Since then an outstanding team of maize cytologists undertook a study of nearly twenty years in mapping the distribution of chromosome knobs of most maize races in the Western Hemisphere as well as the majority of races of teosinte, the closest relative of maize (McKlintock *et al.* 1981). They were able to demonstrate that some races considered relatively ancient from the Pacific coast are related to each other with the 7s large knob: nal-tel, zapalote chico, chapalote, reventador and harinoso de ocho (McKlintock *et al.* 1981:40). Through many pages of carefully documented analysis the team was able to deduce two routes of entry of maize into the Southwestern United States—one along the Pacific Coast and a second that apparently ran from central Mexico northward to the Rio Grande. Although Barbara McClintock won the 1983 Nobel Prize in medicine for discovering transposable elements based on the mode of inheritance of color in corn kernels and leaves, ethnobotanists will remember her leadership in independent cytological corroboration of the integrity of the races that Anderson first deduced from maize morphology. The finer relationships between races revealed in the study she headed should sustain an interest in racial classification for some time.

Given the statistical advances applied to maize variability since Anderson's initial work (eg., Goodman and Paterniani 1969, Bird and Goodman 1977), and given that cupule width represents the nearest measure of kernel width when the cob alone is considered, it is a tribute to Anderson's keen perception for the authors of a recent technical report on maize from the Peruvian coast to remark that of the seven characters used, row number and cupule width were the two most consistently useful traits in distinguishing maize types (Bird and Bird 1980:330). Studies in the 1980s have corroborated the early methods used to identify races, singled out the most ancient races of maize, and given the scientific world a fresh explanation of the seemingly abrupt transition from teosinte to maize as a catastrophic sexual transmutation (Iltis 1983; Gould 1984). The theory has not gone unchallenged (see Galinat 1985) but the persistent storms over maize evolution that have raged for decades have lessened in intensity.

In the Southwestern United States the foot and leg work necessary to provide information on the prehistoric distribution of maize types was done primarily by Hugh C. Cutler in cooperation with field archaeologists. In Cutler's many appendices to archaeological site reports he provided data on cob row number and cupule width. What his colleague Anderson learned of modern races in Mexico and what Cutler himself learned about southwestern corn could by synthesized to interpret prehistory. Two appendices are particularly rich in insights to the linkage of history and prehistory (Cutler 1964 and Cutler 1966). In the earlier report Cutler relates the maize from Carter Ranch Pueblo to the Pima-Papago race that he and Anderson first defined, to more recent racial definitions, and to his observations on Hopi maize. In the later report he provided a greatly expanded overview of southwestern maize as part of his Glen Canyon, Arizona research (Cutler 1966:12-15). He presents views on the progression of races of maize in the southwest and the role of Mexican races like ***onaveño*** and ***harinoso*** de ocho in prehistory.

Maize has provided us with an incredible number of detours in our thinking before we have found the main road. Differences of opinion about which is actually the correct road disappear only in retrospect. Cobs, kernels, pollen, phytoliths and radioactive isotopes have all been the focus of considerable debate, and not without reason. While cobs are the most efficient way to gather data about prehistoric corn, the process is far from perfect. Burned cobs seldom represent more than a random cross-section of a cob devoid of kernels. Although adjustments for cob shrinkage can be made, the lack of attached kernels deprives the investigator of ancillary criteria to assess the racial affiliations of the population formerly present. In addition, corn grown under stress may have reduced row number (Emerson and Smith 1950 in Cutler 1966:11). Recently Mackey (1985) has advocated the use of maize to demonstrate stress in the Gallina Llaves Valley, New Mexico archaeological sites. A necessary prerequisite of such a study is establishing the existence of a uniform race through time. Real problems exist in establishing the former existence of any single race (Benz 1985).

A modern statistical treatment of the races of maize in Mexico and the southwest need development to serve as a template against which prehistoric populations can be fitted. The loss of a host of criteria in archaeological maize makes it more difficult to accurately fit the narrow racial categories formed from intact modern maize (Wellhausen *et al.* 1952) rather than to the more broadly defined races first delimited (Anderson and Cutler 1942, Anderson 1946). We still do not know to what degree changes in southwestern corn result from cultural preferences or selection and which are products of diffusion of new races of maize. Ford (1981:13) has argued that from the genetic variability present in southwestern corn by 300 B.C. one could derive the morphological variability seen in subsequent strains through natural and cultural selection. Ford's ideas in conjunction with the evidence for prehistoric diffusion of harinoso de ocho to the mid-west (Galinat and Gunnerson 1963) might well be considered once the boundaries of our racial taxonomy are in better order and the accuracy of our dating verified with direct radiocarbon dating.

To further complicate matters the process of extrapolating row number from prehistoric maize kernels (Cutler 1956) appears to have serious problems, despite seemingly impeccable theoretical grounds. The sides of the kernel on a fully developed ear should be compressed by the limitations of the space available on the nearly circular cob (360°). The circle could be divided into 8, 10, 12 or 14 parts corresponding to the number of rows on the cob. A cob of 8 rows should have kernel side angles of 45°, 10 rows 36°, 12 rows 30°, etc. If these assumptions were correct and one had only kernels to measure, the angles of the kernel sides could give an estimate of the row number. The accuracy of the kernel angle method of determining row number was tested recently by Pearsall (1980) when a series of 25 kernels from cobs of known row number were measured. The tendency of kernels from 8 and 10 row cobs to be confused and the inaccuracies in the assessment of cobs of 12 and 14 rows were noted.

In a 1984 workshop on ethnobotany in Tucson I directed a similar test on the predictive value of maize kernel angles for row number. Two modern kernel covered ears were selected, one of 12 rows called "A" and one of 14 rows called "B". Each was checked to verify that the ear had the same number of rows at the base and the tip, and the kernels were then removed from the cob. Without knowing the original row number, each student took 30 grains, measured the angles formed by the kernel sides, then classified the kernels as to 8, 10, 12, or 14 rows. Our results (Table 1) show little relationship to actual row number. A class member more experienced in measuring maize kernel angles who used a metal template instead of ruled angles produced similar results. Our obvious conclusion was since we could not estimate the true row number when we could verify the accuracy of our work, there was no point in applying the method to archaeological kernels which were apt to have suffered additional modification of the kernel angle through carbonization.

Current techniques on how to recognize maize pollen in the United States now seem in need of revision. North of Mexico palynologists rely upon the large diameter of maize pollen as a trait sufficiently unique to separate it from other grass pollen. In contrast palynologists in Mexico use features other than size to distinguish maize from the closely related teosinte. In the late 1970s when little barley grass *(Hordeum pusillum)* caryopses

TABLE 1.—*Row number determined from kernel angles on two ears of maize.*

Ear*	Row No. 8	10	12	14	Total kernels
A	36	76	54	6	172
B	13	72	69	11	165

*A was 12 rowed and B 14 rowed.

were identified in the Southwest (Gasser 1981), palynologists showed legitimate curiosity as to the diameter of this New World cereal pollen. Early literature (Jones and Newell 1948:141) and independent measurements by southwestern palynologists confirmed that the diameter of little barley grass resembled most grasses. Recently, however, while doing background research on native barley I noticed that *Hordeum pusillum* from Granite Reef Dam, Arizona, produces larger pollen (Covas 1949:14). In addition, *H. arizonicum* bears pollen grains with modal pollen diameters of 60-68 micrometers (Covas 1949:17), which are very similar to chapalote maize pollen (Irwin and Barghoorn 1965). If the hexaploid nature of *H. arizonicum* (Rajhathy *et al.* 1964:196) gives it certain competitive advantages in growing in modern Papago fields as Nabhan has observed (1983:200) it may have done so in the past. Arizona barley grass has been reported in Maricopa, Pinal and Pima countries (Gould 1951:108) in the Hohokam culture area. We have chanced upon one species that locally negates the assumption that the size of maize pollen produces a unique signature that differentiates it from other grasses. Perhaps systematic research into the size of pollen of all polyploid grasses should be undertaken.

The acquisition of the perception that there is no unique pollen size signature for maize except where local plant geography allows, lets me view with concern another struggle now underway to revise the unique morphological attributes by which maize phytoliths can be recognized (reviewed by Rovner 1983:249-251). As much as we might desire to establish a universal "truth" about maize phytoliths, it might be more instructive to concede that truth is relative. Like maize pollen identification, the validity of maize phytolith recognition may be relative to the amount of ambiguity provided by phytoliths of other plant species growing in the area. Schoenwetter (1974:298-301) has supplied some thought provoking remarks on how one grapples with uncertainties in the maize pollen record in the heart of Mexico. His discussion might as easily apply to related problems with phytoliths.

Although radiocarbon dating began around 1949, it was not until 1967 that the specialists realized maize dated younger than predicted (Bender 1981) because it was one of a number of grasses (Walker and Lewis 1979) and herbs which had a photosynthetic path (C-4 instead of C-3) that accumulated radioactive isotopes differently. Since then the knowledge has been used to advantage in the analysis of the $^{13}C/^{14}C$ ratios in human bone collagen. In animals the isotope values clearly reflect the isotope ratios in their diet, that is, the proportion of C-4 to C-3 plants. Where succulent plants like cholla cactus *(Opuntia)* are eaten, a crassulacean acid metabolism (CAM) mimics the C-4 isotope ratios (Troughton *et al.* 1974). Where maize is likely to be the only C-4 plant consumed, the ratio provides an indication of the importance of maize in the diet, as in Hopewell agriculture (Bender 1981). In the southwest the consumption of C-4 plants such as *Amaranthus, Panicum, Portulaca,* members of the Capparidaceae *(Cleome)* and Chenopodiaceae *(Chenopodium)* contribute to obscuring the vexing question of the former importance of maize agriculture. Studies of its antiquity will benefit from accelerator radiocarbon dating, recently used to advantage in dating individual kernels.

FLOTATION

Although in the 1930s Volney Jones used flotation to recover vegetal remains from adobe bricks from historic Awatovi (Griffin 1978:17) as did Hendry and Bellue (1936) from different historic buildings, the method lay unappreciated by others. In the 1950s Dr. Hugh Cutler visited the Field Museum excavations at Tularosa Cave and Higgins Flat in New Mexico and demonstrated that plant remains could be recovered by throwing screened dirt in a bucket of water and skimming off the floating charcoal and seed (Watson 1976:79). Approximately a year later, when Dr. Cutler visited the University of Arizona field school at Point of Pines (Director: Dr. Emil Haury), a similar demonstration had portent for future ethnobotanical research. Ten years later, in 1964, Dr. Haury creatively applied Cutler's flotation to the second excavation of Snaketown. There flotation was metamorphosed from a dirt sample scattered on a bucket of water to a fully conceived sampling plan for trash mounds and pits to obtain a chronological series for flotation analysis. Haury developed and directed the method of concentrating the charcoal (Bohrer 1970). As an ethnobotanist my job began with the receipt of the bags of charcoal and seed.

Since the early work on flotation at Snaketown, our understanding of the formation of the carbonized vegetal record and the fraction that is apt to be recovered by flotation has undergone considerable evolution both in North America and abroad. Flotation techniques in vogue in the mid 1970s have been reviewed (Watson 1976) and numerous variants have been reported since then. Research designs compatible with research objectives have been promulgated by Adams and Gasser (1980), Bohrer and Adams (1977), and Toll (1984). Schaaf (1981) has advocated the use of a sample splitter among other techniques, and methods have evolved to check the accuracy of flotation (Wagner 1982). A concommitant development has been the acknowledgement that carbonized seeds differ in their buoyancy and that certain conditions or properties of seeds may cause them to sink (Pendleton 1983). Such problems may eventually lead ethnobotanists who haven't yet done so to the examination of the heavy fraction or to adopt a modified form of water screening, as has Schaaf.

The recent publications of the results of two large archaeological projects at Dolores, Colorado and in Chaco Canyon, New Mexico incorporate several innovations helpful in flotation analysis. At Dolores, archaeologists have formulated criteria for field recognition of four site abandonment modes (Breternitz 1984:166) (1) leisurely abandonment implying minimal preservation of artifactual botanical material; (2) deliberate abandonment, burning within 5-10 years afterward, providing moderate preservation potential; (3) ritual abandonment; (4) catastrophic abandonment allowing optimal preservation of materials. A number of advantages are apparent from this procedure. The segregation of sites in terms of potential for plant preservation helps set priorities in the analysis of pollen and flotation samples. Furthermore the method provides a distinct aid to interpretation, for the manner in which case 2 and case 4 are treated, are vastly different (Halley 1981; Minnis 1981). Finally, in a project of long duration, the prospect remains of increasing the accuracy of judgments made in the field from the results in the laboratory.

At Chaco Canyon, Mollie Toll (1984) undertook flotation from two sites using different sampling designs. With the help of a third site, she has been able to bring into focus underlying patterns obscured by different seeds densities in flotation samples. Her innovations in dealing with unburned seeds may assist those working in other arid areas or historic sites where uncarbonized seed is seemingly still preserved. Two were shallow village sites subject both to erosion and alluvial deposition. A third site (Pueblo Alto) had living surface over a meter deep. Pueblo Alto was sampled like one of the shallow village sites and used to evaluate the possibility that some of the unburned seed might be prehistoric. The pattern of seed recovery from habitation room to habitation room at Pueblo Alto varied little. Weedy *Chenopodium*, *Amaranthus* and *Portulaca* were carbonized in heating features and uncarbonized on floors in decreasing density with increasing distance from food processing features. The distribution suggests prehistoric primary deposition of unburned seeds of Pueblo Alto. In the shallow sites the conclusions drawn by using only burned seeds or burned plus unburned seeds are similar—a pattern of using immediately available disturbed ground species in company with agricultural products.

There are several qualifiers in the above study. The first is that the difference between burned and unburned naturally black seeds can be subtle. Even slight parching of a seed coat may deter degradation. The second is that high standards of sample selection need to be exercised rigorously. Sample locations that were cleaned out prehistorically and filled with alluvium, that were disturbed by rodents, that were ambiguous in terms of the source of cultural versus natural deposition (like post holes) had to be eliminated from consideration (Toll 1981:2). Unless non-cultural factors active in archaeological deposits are recognized and systematically excluded before flotation samples are analyzed, Mollie (1984:249) warns that "Non-cultural factors may introduce more variability into the archaeological record than the ones that can be related to past behavior". Cultural and non-cultural factors represents two sides of the same coin—have we always examined each side with equal care? When the ground rules are carefully set forth and the cultural and non-cultural facets of seed deposition vigilantly explored, there seems to be a possibility that shallow, open sites as well as those over a meter deep may preserve a prehistoric seed fraction that appears unburned.

Regional analyses of subsistence, particularly prominent in the last five years, allow us to better visualize the utilization of cultivated and domesticated crops, the nature of the fields and the encouraged plant pioneers. Probably no editor had a tougher scramble to keep North American papers current than Richard Ford (1985). The emerging evidence for prehistoric cultivation of agave and barley in the west and the domesticated *Chenopodium* and *Amaranthus* in the Eastern United States tremendously complicated the publication of his timely volume. Regional summaries by persons with the knack of distinguishing true subsistence variability in site assemblages from differences induced by variation in methods of data collection, analysis or degree of preservation are needed continuously.

Ethnobotanists have been passive bystanders while hunter gatherer foraging strategy proliferates along with numerous other models for the preconditions for the spread of agriculture. Often models are not stated in such a way that they can be tested with archaeological or ethnobotanical data. The missionary zeal of the theoretically oriented would benefit by struggling with the uncertainties of organic preservation and related realities. Ethnobotanists in turn, could assist more with theory and with its translation into practical, testable hypotheses.

CHARCOAL AND FECAL ANALYSIS

The questions asked in 1963 of wood charcoal analysis (Western 1963:151) resemble those considered in site reports today. What kinds of trees and shrubs have been used for firewood, in construction and in the making of objects for daily and ritual use? Does the analysis of wood types reveal anything in regard to: (1) climatic differences; (2) nature of local vegetation (forest, scrub or grassland); (3) the importation of timber for special purposes; (4) the extent to which the qualities of different woods were appreciated? However, if the question in regards to whether or not cultural selection of woods is practiced fails to receive priority, then all three of the preceding questions prove particularly difficult to answer. If for example only ponderosa pine and fir are used as beams in a site which is today surrounded by pinyon and juniper, are we looking at a change in vegetation or the importation of timbers for structural purposes. If hickory charcoal predominates in hearths, can we conclude hickory was the dominant forest tree? To what extent is cultural selection active in each case?

Only when mild post-excavational curiosity about the charcoal content of a site is replaced by problem oriented provenience sampling can we discard weak interpretations based on often explicit but questionable assumptions. At some sites it would be possible to determine a lack of bias on the part of the inhabitants in choice of wood if the type used for beams is the same as used in a quick hot fire, in the baking pit, and in the smelting furnace. It would also be possible to compare floated charcoal splinters to larger pieces from the same provenience to investigate differential destruction of charcoal types.

Many ethnobotanists, instead of employing cross sectional views of charcoal as a preliminary step in identification, use it as the only source of evidence. Now if specialists who identify modern wood regard the study of a cross section as only an initial step and secure additional information from radial and tangential sections, I fail to see how those among us who do so much less than that with prehistoric charcoal can hope to achieve a comparable level of reliability. We are dealing with an undetermined range of anatomical patterning in wood created by juvenile twigs as well as some shrubs and trees whose anatomy remains undescribed in texts. Current work could strive to match the standards set over 40 years ago in the identification of fish weir stakes and wattles from Boylston Street, Boston (Bailey and Barghoorn 1942). To recapture this standard requires both proper tools and training.

Using the right tool helps get any job done quickly and well. In 1972 the Forest Product Laboratory recommended a microscope with incident light (vs. transmitted) to examine wood. In today's technology an ordinary dissection microscope of 30x has been replaced by metalurgical microscopes that have magnifications between 50 and 150 power and a working distance of 3 or 4 cm. or better. Such microscopes may already be employed in anthropology departments for lithic wear and pottery temper analysis. In a freshly broken piece of charcoal, critical features like spiral thickening, pitting on individual cell walls, or sieve plate details are apparent with no further preparation. It is true the same job can be accomplished with a Scanning Electron Microscope, but critical details of dicotyledon anatomy can be obtained more efficiently with a metallurgical microscope.

Courses in plant anatomy and the technology of microslide preparation are rarely offered these days, and unless they are offered by someone around long enough to reach the rank of full professor, teachers are apt to be under qualified. Ethnobotanists younger than age 40 have reduced access to formal training in plant anatomy and many are unaware of how beneficial the acquisition of an educated eye and the ability to read the literature with true understanding can be.

From the 1970s onward archaeologists and ethnobotanists have good naturedly accepted studies of the seed and pollen fraction of human feces without questioning the possibility of directly documenting the inevitable greens that are part of omnivorous dining. I know of two exceptions: a thesis from the University of Colorado (Stiger 1977) that deals with coprolites from Mesa Verde National Park and one from Texas A&M. University (Williams-Dean 1978) that concerns archaic coprolites from SW Texas. Throughout the same period the *Journal of Range Management* has regularly carried articles on determining range herbivore diets through fecal and fistula analysis (reviewed by Holechek, *et al.* 1982). Many articles detail problems relating to the quantification of results and their validity, a matter of interest to all. Identification of plant epidermal fragments resembles pollen and phytolith analysis in that it is labor intensive, requires expertise and an extensive plant reference collection.

CHANGES IN PLANT LIFE

Finding a new species as a result of archaeological investigation would be an event equivalent to a paleontologist recovering a new fossil form. In as much as our modern flora has essentially been in place 8,000 years (VanDevender and Spaulding 1979) such a discovery is unlikely. Our nation has been so bombarded by introduced plants that what is truly native can remain unrecognized. At such times the archaeological record makes a genuine contribution to our understanding. The presence of bugseed *(Corispermum)* in hearths, in a Mimbres jar from Swartz ruin, New Mexico and in human coprolites from Cowboy Cave, Utah can be helpful finds when their age has been confirmed by accelerator radiocarbon dating (Betancourt 1984). Furthermore, the "useful weed" status of some annuals now rare or locally extinct can only be appreciated from their broad distribution in archaeological contexts (Bohrer 1978). Modern more aggressive introduced plants have altered their role in disturbed habitats.

Local changes in southwestern vegetation have been of such a magnitude that a contrast is apparent within the lifetime of one person. Historic changes in dominant vegetation have been acknowledged recently in what may become a standard reference of the biotic communities of the American Southwest (Brown 1982). Our semi-desert grassland is almost obscured by scrubby trees, brush and cacti. Two native shrubs, burroweed *(Isocoma tenuisecta)* and snakeweed *(Gutierrezia sarothrae)* have replaced grass understory over millions of acres and serve as indicators of former grasslands (Brown 1982:131). In west central Arizona the grassland has shifted from perennials to introduced annuals (Brown 1982:129). In conifer woodland of the Great Basin, junipers have invaded large areas of former grassland (Brown 1982:53). Arizona climax cienegas and marshlands have decreased greatly since 1890 (Hendrickson and Minckley 1984).

The tendency has been to account for modern plant assemblages by considering insufficient limiting factors such as either soil or climate. All too seldom is the biotic factor brought to attention, e.g. in terms of competition from other plants, grazing or human intervention. The grazing industry has vested interests in not recognizing the role of overgrazing in the loss of grassland, just as the lumber industry does not speak of changing the composition of the primieval forest.

Two organizations in the Southwest have evolved different approaches to environmental reconstruction. SARG, the Southwest Anthropological Research Group, discovered one plant ecologist (Küchler 1964) who takes the anthropogenic factor seriously into account when mapping vegetation (Effland 1978). Küchler predicts what a modern plant assemblage would become if suddenly today human influence were removed. Unfortunately for archaeologists, Küchler's potential vegetation maps represent predictions that lack the supposition that landscapes will revert to pre-conquest vegetation. The link to the past is missing. In contrast, environmental reconstruction on the Gila Salt Aqueduct Survey in central Arizona (Fish 1985) began by obtaining numerous (N = 400 +) pollen samples from a variety f sites. The importance of redundant information and duplication of effort became apparent by the internal patterning between suites of pollen samples. A valuable data bank of cultural and ecological information was created. Suzanne Fish could recognize anthropogenic pollen floras because of their characteristic signature in archaeological sites (an abundance of insect pollinated Arizona poppy *Kallstroemia*, spiderling *Boerhaavia*, and globemallow *Sphaeralcea)* contrasted so sharply with pollen spectra from locations away from intense human activity. Modern analogs to the prehistoric disturbance flora proved difficult to locate. Biotic factors active in the creation of a disturbance flora have changed in intensity and direction. Suzanne warns us (1985:84) "If disturbance taxa which disperse relatively little pollen can influence archaeological spectra so greatly, interpretation of environment and climate from more prolific producers should proceed with great caution." Our limitations are increasingly apparent.

No neat, easy formula helps one detour around the dangers in the reconstruction of vegetation. The uncertainty (nay, disagreement) in attributing vegetational change to cultural or natural causes in the archaeological record has been pointed out recently by several authors (Bryant and Holloway 1983; King and Graham 1981). Overly confident attempts to quantify past food resources based on the presumed distribution of vegetation, carrying capacity and calories have been numerous in recent years (see Moran 1984). A more conservative view of environmental reconstruction is expressed by King and Graham (1981:139) . . . "it should be possible to develop the knowledge of a few key taxa necessary to fairly accurately quantify those resources and to project variations in their relative abundance and distributions back through time as they may have responded to environmental change."

CONTEMPORARY STUDIES

It is never too late to conduct contemporary ethnobiological studies. I think many modern studies are better than years ago because of the ecological orientation. Even though previous studies existed on southern California Cahuilla ethnobotany, the work of Bean and Saubel (1972) provided important insights into the manipulation of plant life. One can seek examples from other parts of the world, but those nearest one's own territory seem to carry more impact. For example, when I read how fan palms are planted from oasis to oasis by the Cahuilla, it was easier to understand how agaves might have found their place on terraced slopes in southern Arizona (Fish *et al.* 1985).

No matter what our task is in ethnobotany, no matter how supposedly tedious, it needs to be treated as important and worthy of our best efforts. The seemingly insignificant record that Gary Nabhan made of Arizona barley grass in Papago fields took on a new dimension when viewed from the perspective of a palynolgist working with the prehistoric pollen rain. Research by David Clawsen (1985) inadvertently helped us understand the color coding of maize that corresponds to the symbolic colors of the sacred directions among the pueblos. Although ritual observances frequently reinforce practices (once) vital to the economic survival of a people, the particular advantage bestowed by preservation of strains of colored maize has been obscure. David Clawsen tells us color coding of crops such as potatoes, yams and many legumes helps a cultivator plant according to given maturity periods. The farmer who maintains a greater diversity of colored varieties of any one crop carries a kind of physiological crop insurance. Despite how the rains distribute themselves, or whether or not the frost is early or late, the chances are good that there will be some crop to harvest. Clawsen has provided ethnobotanists working with the archaeological record a lost dimension, for by the time we examine the physical remains of any crop, all the varieties are the same color, *black*.

ETHNOBOTANISTS AND ETHNOBOTANICAL INTERPRETATION

Ethnobotanical interpretation of prehistoric plant remains should consider factors like the method of excavation, sampling design, context of recovery, and post-depositional factors until all pieces of the puzzle fit in harmony (Fig. 1). But there are aspects of interpretation that transcend site specific boundaries. One must provide perspective derived from a broad range of experience in dealing with the archaeological plant record (directly or via literature) and from the rich, deep kaleidescopic patterns formed by modern and historic interrelationships of plants and man. Archaeological botanical evidence can be likened to a crumb surviving a whole loaf of bread. Or envision it as a stanza of a longer ballad. One needs to provide the refrain that brings unity, lest such botanical stanzas land in the scrap heap of our intellect. A supra-tribal, supra-national viewpoint combined with a competent ecological perspective can supply vitality to any otherwise drab scholarly discussion. Am I asking too much? The way our society organizes itself makes preparation for the task more difficult.

The compartmentalization of knowledge into fixed disciplines works against the creation of an ethnobotanist. The time needed to acquire a dual background often proves excessive unless one begins as a undergraduate. It is none-the-less essential to develop the interdisciplinary vocabulary and theoretical understanding that allows one to read the literature directly, for therein lies a rich reservoir of ideas and a bridge to alleviate misunderstanding.

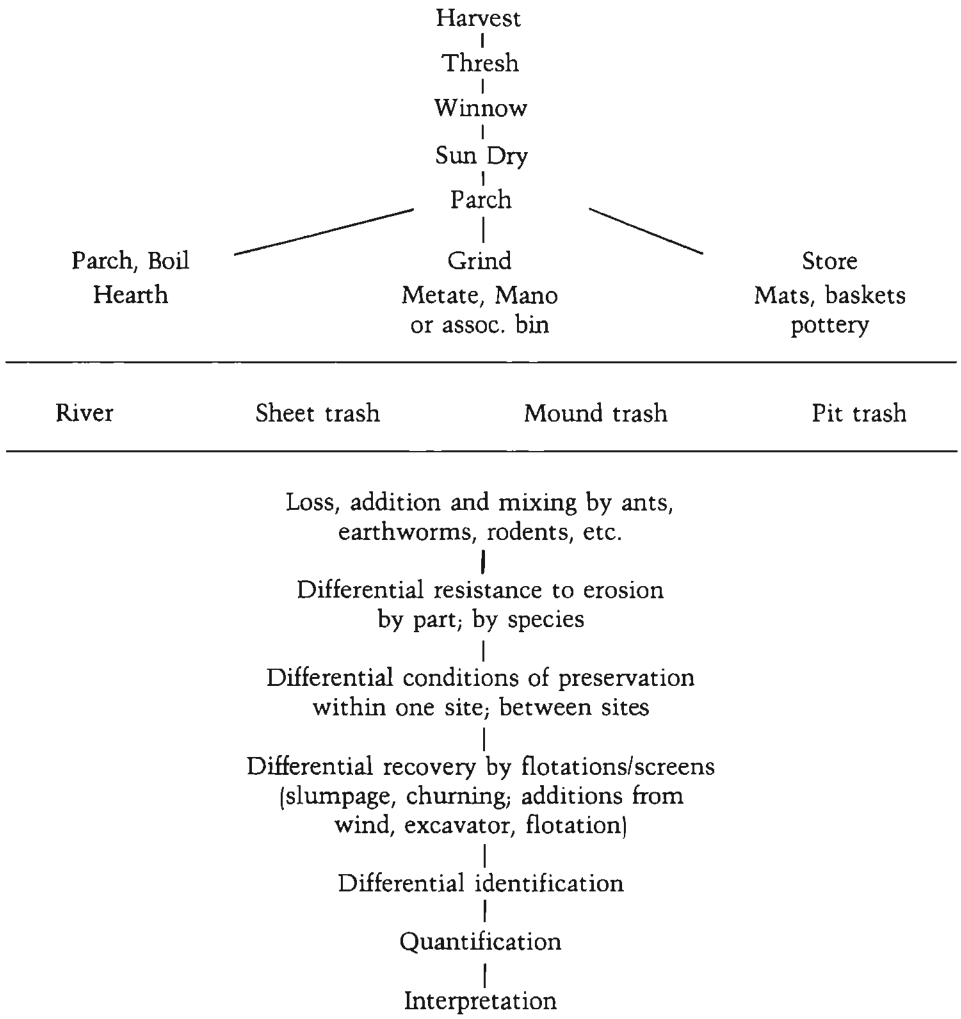

FIG. 1.—The formation of the archaeological seed record.

Neophyte ethnobotanists greatly benefit by being shown how the abstractions of the textbook apply to the field. In subjects like plant anatomy, taxonomy, ecology, evolution and core courses in anthropology, content is commonly emphasized over developing skills in problem solving. To resolve a baffling question a more experienced researcher can supply the impetus for a younger colleague to try a new approach or re-evaluate an old one. Many practicing ethnobotanists have incorporated these skills (Toffler 1971:414) in their own approach to problem solving: (1) classifying and reclassifying information; (2) determining its veracity; (3) shifting categories when necessary; (4) moving from the concrete to the abstract and back again; (5) looking at a problem from a new direction; (6) shifting from the inductive to deductive methods and back again.

As ethnobotanists exist in increasing numbers as independent consultants, a form of ancillary or off-campus research internship would seem desirable for students. If problem solving skills are not formally taught, opportunities should be available where skills can be developed. In addition, the research topics that intrigue professionals who are directly and heavily involved in research are apt to be more original than well-massaged topics like the origin of agriculture.

In the past people with ethnobotanical interests have found positions in colleges, universities or museums. Formerly men with doctorates held academic positions exclusively whereas now they are joined by a small group of women. Traditionally (since the 1930s) the museum has nourished and protected ethnobotanical careers of men and women of all levels of education. I am reminded of the support of Melvin R. Gilmore and Volney H. Jones at the Museum of Anthropology at the University of Michigan, of Margaret Towle's association with the Harvard Botanical Museum, of Albert Whiting with the Museum of Northern Arizona, of Hans Helbaek and the National Museum of Copenhagen, Jaques Barrau at the Museum National de Histore Naturelle in Paris, Edgar Anderson and Hugh Cutler of the Missouri Botanical Garden. The trend continues to expand as many additional museums (such as the Bernice P. Bishop Museum in Honolulu and the Arizona State Museum in Tucson) find places for people with ethnobotanical skills.

RESERVOIRS OF IDEAS AND INSPIRATION

Everyone, including the ethnobotanist to be, must ask, "What shall I read?" "What is worth reading again and again?" "What are the classics?" Sometimes we read to know the level of understanding achieved in times past, but the real value is in capturing "the visions and the styles of the scientists who preceded us" (Thompson 1984:187). Short articles seldom contain such an essence and short articles indeed predominate the ethnobotanical literature. Really good autobiographies or biographies of ethnobotanists are all too difficult to find: their lack impoverishes us. My own library contains some favorite books: Edgar Anderson's (1954) *Plants, Man, and Life*, Frank Cushing's (1920) *Zuni Breadstuff*, M.P. Harrington's (1932) *Tobacco Among the Karuk*, W.W. Hill's (1938) *The Agricultural and Hunting Methods of the Navajo Indians* and Paul Weatherwaxe's (1954) *Indian Corn in Old America*. More recently I have added Gary Nabhan's (1985) *Gathering the Desert*.

The above list contains no volumes, chapters or appendices in archaeological site reports because so few are models of "how to know, how to think, and how to write" (Thomson 1984:187). Few contain the flavor of imaginative thought, clean, stylish prose and front-line science (Thomson 1984). But there are some. I have already discussed one recent example (Toll 1984) under flotation. Toll has distilled voluminous observations into a single paper with breathtaking conciseness. The facts at her disposal do not overwhelm her. Another example is Goodyear's (1975) Hecla II and III. Goodyear is an archaeologist that sees no barriers between anthropology and botany. The waters of each intermingle and sustain him as he develops detailed methods of sampling cultural and botanical data and constructs models of prehistoric subsistence activity in remote areas of southwestern Arizona. The strength and weaknesses of the techniques employed are candidly presented while the author places his work in a broad regional perspective. His manner of investigation has been rightfully described as thorough, creative and efficient (Gumerman 1977:12).

Several examples of how to know, think and write date from the 1950s. One of these is V.H. Jones and R.L. Fonner's Appendix C *Plant Materials from Sites in the Durango and La Plata Areas Colorado*. Every shred of information is given the courtesy of thorough treatment and the line of reasoning clearly presented. Findings are placed against a background of knowledge of plant utilization so carefully documented that later finds of cultivated amaranth are anticipated. The discussion of maize is unusually complete and lucid. The long period of evolution of the published report is traced for the reader.

The other example is Hans Helbaek's (1952) *Early Crops in Southern England.* His understanding of what constitutes crops is truly refreshing, for he includes far more than wheat and barley. Flax, rye and crabapples; chess and oats weave in and out of discussions. The role of the *Brassicas, Chenopodium, Polygonum* and *Galeopsis* as plants of cultivated fields and gathered food sources forms a balanced part of his concern for early crops. His tables tell of the increase in grain size with the continued cultivation of several crops and one learns of the waxing and waning of the prevalence of naked barley against the backdrop of other available choices, including rye. Never is southern England treated as an island whose plant history is devoid of connections. The context is European and the scope epic in its coverage of the neolithic, bronze and early iron ages. The keeness of his observation and his critical perception of events prevails in his writing. Would that we could achieve as much in the years ahead.

REFLECTIONS

The history of research on maize is one of looping back over territory once thought to be conquered. Cob and kernel measurements, pollen identification, phytolith recognition, carbon 14 dating and corn evolution—all have been the focus of controversy. Has maize been such a popular band wagon that some people have published prematurely? Or is it that newly developed techniques to deal with maize have been readily accepted without critical evaluation? As we gain enthusiasm for the human capacity to begin to tinker with the evolution of many other plants besides maize, can not this same thing happen with every new species proposed as a cultivated or domesticated plant? The above described trends in the study of maize are ones that I would rather not project into the future. Researchers who worked with ethnographic maize and those who dealt with prehistoric remains sometimes hummed little tunes the other did not hear. In similar cases the meetings of the Society for Ethnobiology can serve as a forum.

It is the larger archaeological projects that seem to have made the most headway with methodological problems concerned with flotation and pollen analysis. This is troubling, for larger projects are dwindling and the burden should be shared more evenly. Enough site specific studies have accumulated that syntheses are being produced, an admirable trend that should continue. Presently we perceive common denominators in subsistence practices without daring to ask "Why?" A greater participation in the theoretical realm of model building and model demolition surely must be near.

Attempts to reconstruct vegetation have varied in approach and results. We have had to learn a lot about our limitations. We may have to be content with estimating abundance of a few species rather than dealing comprehensively with absolute abundance. By analyzing a spectrum of contexts in which prehistoric plants occur can we discover something of their former competitive relationships? The disturbed ground plants of today and of pre-contact times were not always one and the same. The role of soil and climate in influencing the formation of a given plant community has enjoyed exclusive popularity too long. The biotic factor, including the anthropogenic one, also needs careful consideration. Nevertheless, the path is still not all that clear. Our growing humility might be transmitted to archaeologists who still believe in miracles.

Museums have served and probably will continue to serve as a focal point for ethnobotany. Apprenticeships to museums and other independent organizations active in sponsoring research may be the best way for a prospective student to develop skills and apply book learning to concrete issues. It is never too late to conduct contemporary ethnobotanical studies. Field work in the ethnographic and ethnobotanical realm coupled with omnivorous reading sharpens appreciation for the multifaceted patterning of plant-man interrelationships. While it still can be secured, formal training in plant anatomy can reverse a downward trend in the quality of reports concerning charcoal, coprolite content and related applications. If ethnobotanists would first dream that archaeobotanical site reports might serve as reservoirs of ideas and inspiration, it would not be long before we could read them. A viewpoint that transcends both tribe and nation blended with a competent ecological perspective can, with continual grooming, guide others to "New Directions in Ethnobiology".

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