variation has been inconsistently handled. Wilson (1969), for example, distinguishes between types of Alameda Brown Ware on the basis of the relative percentages of different mineral inclusions. This approach is unwieldy for the level of analysis envisioned in this discussion. On the other hand, there are apparently undifferentiated sherd and sand tempered varieties of the Alma series.

6. Corrugated: The same technological problems that characterize current descriptions of plainwares occur in the case of corrugated types. These need to be resolved. Stylistic descriptions have been based on the pattern of surface coils, bands, and indentations. Brunson's statistical analysis has demonstrated so much overlap that relatively few categories will suffice, such as obliteration, partial obliteration, indentation, banding, and patterning (including scoring, punching, etc.). Yet, her analysis is for only a portion of the study area and more detail may be required.

7. Painted Corrugated: Neither stylistic nor technological variation is well described. For example, our collection includes white-on-red, white-on-orange, black-on-red, black-on-orange, red-on-orange and virtually all of the possible polychrome-on-orange combinations. Whether this is a local manifestation that does not require resolution at the regional level, or if a regional resolution is required, is unclear at present.

Improvement of the typological system is only a means to enhance communication among different archeologists working on different research problems within the study area. The case for more detailed analysis is already made. Despite the generally negative attitude toward the pioneering studies of stylistic variation done by Longacre and Hill, the principle on which their work most fundamentally rests is now established; at virutally every level of spatial detail, patterning can and should be sought. Hantman and Lightfoot (1978) have made the same case for temporal patterning. It is dubious that the detail required for any of these analyses, nor for containing the variation that is demonstrated by petrographic studies, should become a part of a typological system as that system would rapidly become too complex and costly to use. However, the useful information that can be obtained from these more detailed studies is now well understood.

The problem with the approach taken by Hill and Longacre is the overly simple interpretation that was placed on the results. Spatial, temporal, and functional patterning were not clearly distinguished. As S. Plog has shown, and subsequent analyses have confirmed, one must clearly distinguish between the three. Nevertheless, Plog's and subsequent analyses have found attributes of ceramic variation that can be used for more detailed inferences when these overall dimensions are adequately treated.

CHIPPED AND GROUND STONE

INTRODUCTION

Chipped stone artifacts have received far less attention in efforts to interpret the prehistory of the unit than have ceramic ones. As late as 1964, one major site report (Martin et al., 1964) grouped chipped stone artifacts into the following categories: projectile points, drills, saws, gravers, knives, scrapers, choppers, and scraper planes. No discussion of the reasoning behind the typology, nor why they used an abbreviated version of a previously far more complex one, was presented. The results of the classification were not incorporated into the interpretation of the site in question. This approach is by no means unique; it is an accurate reflection of the manner in which chipped stone had been handled for decades.

Within the last 15 years, this situation has changed drastically. Chipped stone has become a major focus of analysis as archeologists sought to identify "tool kits" or to describe the variation in the techniques used to manufacture particular artifacts or particular chipped stone assemblages. A bewildering array of attributes are now used in the study of most assemblages. In one recent effort these included: raw material; flake condition; length; width; thickness; length, width, and thickness of the platform; residual striking platform; platform preparation; number of dorsal scars and type of scar; of cortex; flake termination; eraillure; lipping; force lines; bulb of percussion; symmetry; utilization; and edge angle. An even longer list of only partially overlapping variables could be described for recent analyses of projectile points alone.

At the same time, chipped stone has been seen more and more as a source of important information concerning behavior and culture. Exchange and site function are two of the most common patterns that are investigated using this material. At present, there has been no effort to integrate the results of these diverse studies into a single overarching typology. The necessity of such an effort is debatable although it may prove useful. At present, it is possible only to describe the diverse courses that investigations have taken.

TYPOLOGY

A first major area of investigation is typology. While an adequate typology for the study area clearly does not exist, a number of studies on which such a typology might be based have been done in recent years.

The first departure from the use of intuitive typologies in analyzing chipped stone is Longacre's study of materials from Broken K Pueblo (1967). He used metric measurements of flake lengths and widths to argue for selectivity in the use of flakes of different sizes for the manufacture of different tool types. He also analyzed, using a rudimentary typology, the different categories of chipping degree and found differences between some room blocks at the site. Perhaps most importantly, he found that habitation rooms and storage rooms with features had many times more chipping debris than storage rooms without floor features.

Decker (1976) studied roughly 1400 chipped stone tools from the Chevelon drainage in an effort to identify classes within the general category of scrapers and knives. Of a large set of attributes studied, he concluded that edge angle, edge length, and edge contour were the significant variables for defining the classes. He identified two problems in the use of any such system, however. First, the approach cannot be applied to whole artifacts but to each worked edge and potentially to independent use episodes on each edge. Second, even using the variables he mentioned, computer analysis is necessary to group the artifacts in question. On balance, these results suggest a very casual pattern of artifact use in potentially quite different activities.

Perhaps the best attempt to rethink the issue of chipped stone typology is that of DeGarmo (1975). His analysis of the assemblage from Coyote Creek Pueblo included a clear description of each of the categories into which artifacts were sorted. While the effort included only minimal results of statistical analyses of artifact types, it did provide historical background concerning other references to the type, hypotheses concerning the function that each type served, and a discussion of some experimental results that DeGarmo used in attempting to establish function. The distribution of different artifact types in rooms and room blocks at the site was also described. These data, in combination with others, suggest both variation in the use of different parts of the site and specialization in the production of at least some items.

MANUFACTURING PROCESSES

A number of studies have attempted to define patterns of change in the manufacture of projectile points and other bifaces within the overview unit. Traugott (1968, see also Plog 1974) studied the relationship between heat treating and flake form in the case of projectile points from sites in Hay Hollow Valley. His study contributed to understanding the manner in which projectile points are manufactured. Specifically, projectile points made using primary flaking to thin the flake tend to be heat treated. Projectile points made simply by trimming the edge of the flake (secondary retouch) are generally not heat treated. Generally the secondary retouch is used to produce triangular forms on flakes removed from cores so that they are already sufficiently thin. The flakes produced by the second process are far more standardized than those produced by the first. The first process is the earlier and is superseded by the second at about AD 800. Early and later points are shown in Figures 21 and 22.

The complicating factor is the apparent widespread reuse at later sites of flakes made by the older process. While flakes made using the second process are rarely found on early sites, flakes made by the second are widespread on later ones. This pattern occurs on sites from the entire Apache-Sitgreaves National Forests area. Figure 23 illustrates a particularly compelling case. At the same time assemblages from some sites are highly standardized (see Figure 24).

CHRONOLOGY

Further analyses have attempted to identify specific changes in projectile point form sufficient for establishing an areal chronology (Li 1973, Sexton 1976, Rafferty 1977, Coulam and Hutira 1979). Li attempted to generate a usable system using a modification of Whallon's (1972) technique for generating a hierarchical classification system. While a usable typology was developed, it proved to have minimal chronological value. Rafferty's effort was based upon Li's and yielded the same conclusion. Five attributes were used in these two studies: basal width/blade width, primary flake type, basal curvature, length, and width. Sexton employed a greatly expanded set of attributes: distal shoulder angle, proximal shoulder angle, basal indentation ratio, length-width ratio, notch opening, maximum width position both distal and proximal, basal width/maximum width, length/width, longitudinal cross section and transverse cross section.

Rafferty's analysis resulted in the identification of four basic projectile point types: unstemmed with basal indentation, unstemmed without basal indentation, stemmed side-notched, and stemmed corner notched. Before about AD 1100, unstemmed points with no basal notch and stemmed, side notched points predominate. After about AD 1100 basal indentation on stemless points and corner notching on points with stems are characteristic. While he interprets the data in spatial terms, an evaluation of Sexton's results also suggest that prior to AD 1100, points are thicker. This is consistent with Traugott's argument concerning a shift in the manufacturing process. In my experience, the later points are also far more standardized in size and shape.

Coulam and Hutira (1979) used a combination of cluster and discriminant function analysis in an effort to identify temporally sensitive projectile point forms. Their analysis suggested some possible relationships between body and basal width. However, there was no apparent spatial or temporal patterning to the distribution.

FUNCTION

The function of specific tool types has been explored and used in efforts to contrast different activity areas on sites, on different types of sites or in different parts of a particular study area.

Garson (1972) studied the color characteristics of chipped stone artifacts and the

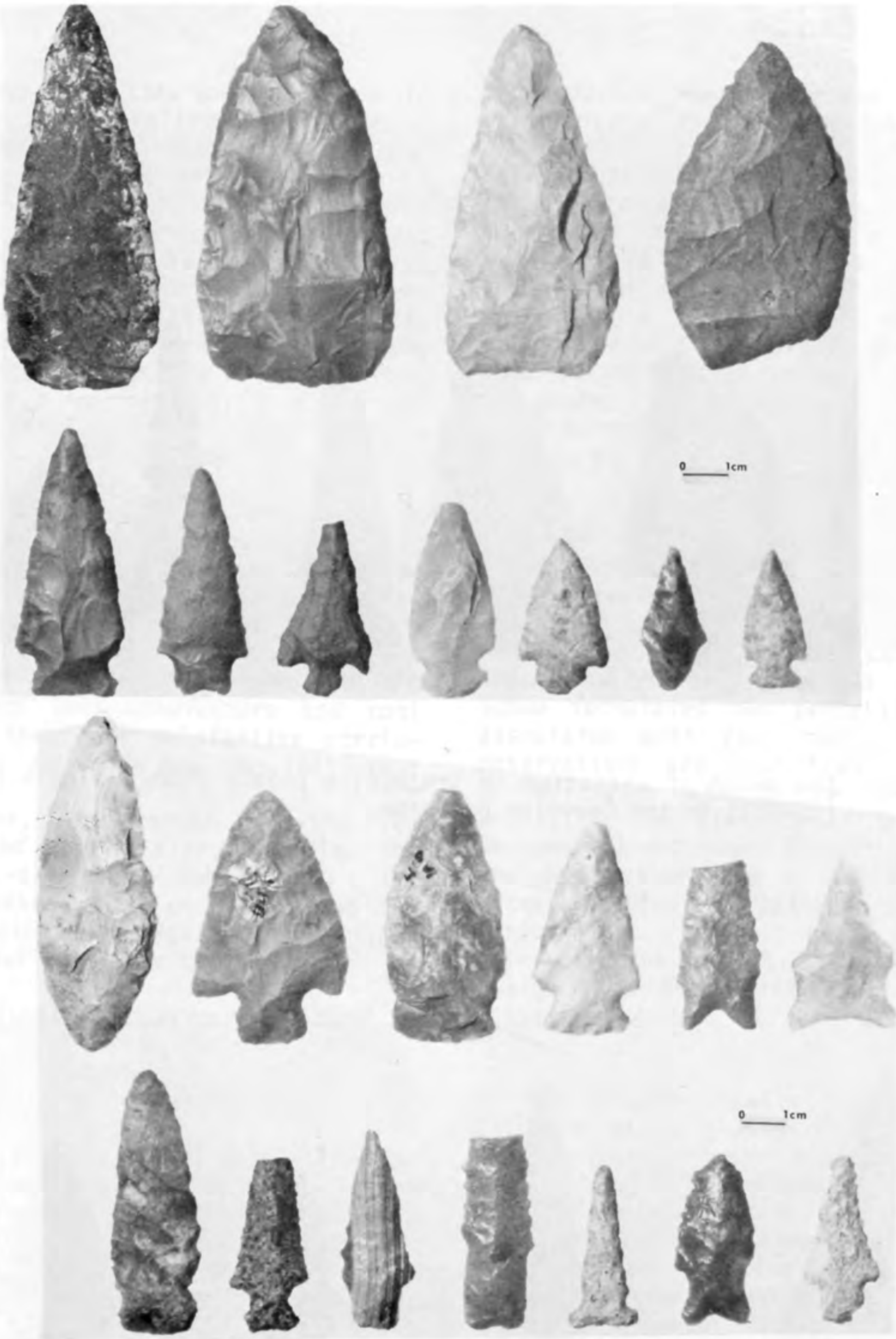

Figure 21. Early projectile points and bifaces from the overview area.

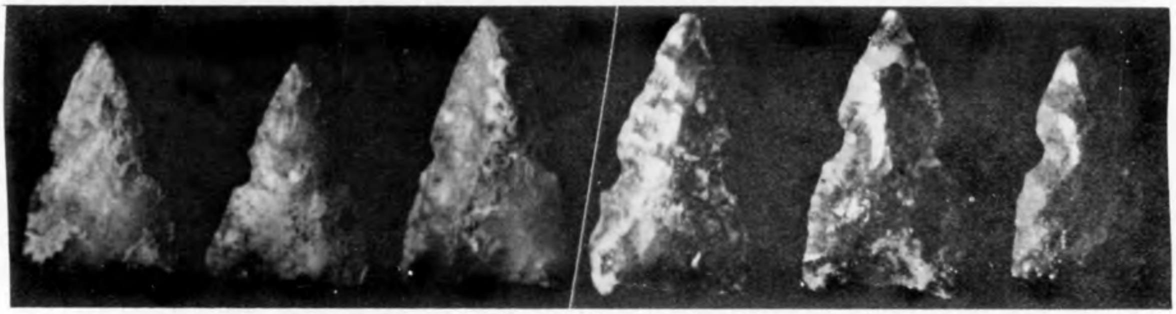

Figure 22. Highly standardized points from Navaqueotaka, a large late pueblo just outside the Little Colorado overview unit. The average point is 1.5 cm long.

Figure 23. The range of variation in projectile points from a single pithouse (CS-553) in the Chevelon drainage.

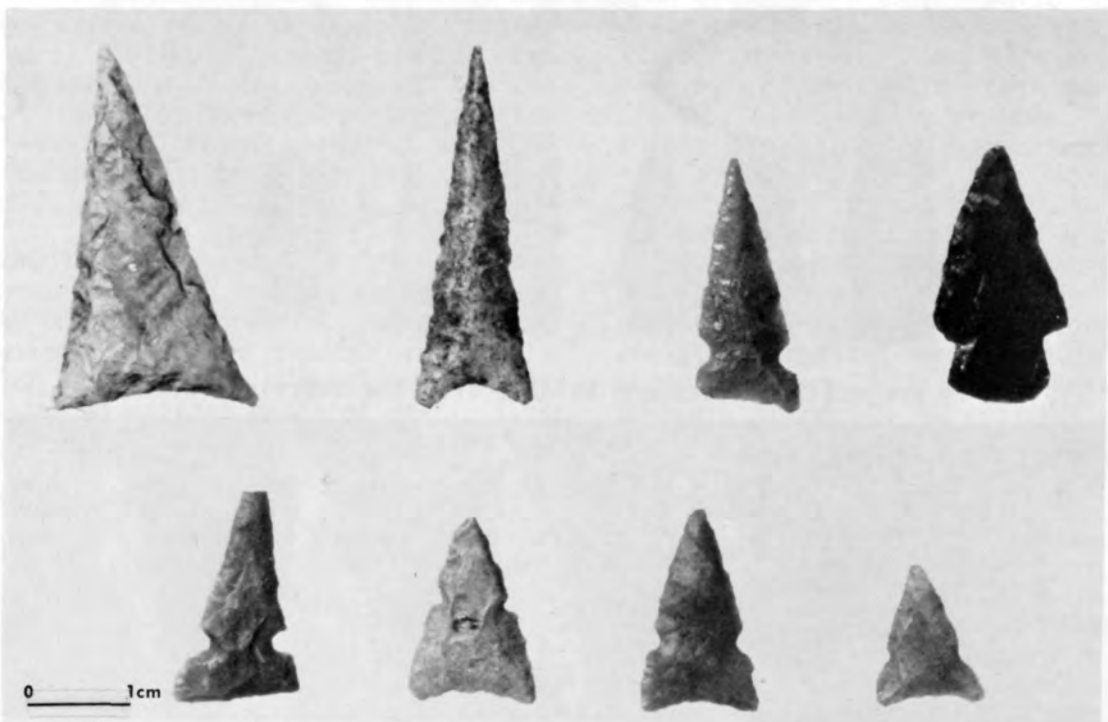

Figure 24. Later projectile points from the overview unit.

materials from which they were made at site NS28 in Hay Hollow Valley. A sample of cherts and chalcedonies were collected from source areas in the immediate vicinity of the site. Garson postulated that there might have been selection of different colors of raw materials by different social units or for manufacturing different types of artifacts. His results suggested that no such selectivity occurred. Only in the case of chalcedony was there any apparent selectivity and the possibility could not be rejected that this pattern reflected a selection of a higher quality raw material.

Schiffer's (1976) analysis of chipped stone artifacts from the Joint Site is probably the most comprehensive analysis of variation in chipped stone artifacts undertaken in the study area. His effort includes a lengthy discussion of the strengths and weaknesses of various approaches to the study of both tool manufacture and tool use. Among the major substantive conclusions of his research are the following:

1. Chalcedony, the rarest of the raw materials used at the site, was also the most intensively used and reused. The number of worked edges on flakes and the amount of shatter are significantly higher than for either quartzite or chert.

2. Quartzite was most frequently used in the manufacture of large tools and chalcedony in the manufacture of smaller and especially bifacially retouched tools.

3. When the chipped stone artifacts from secondary refuse are analyzed using factor analysis, three factors can be isolated. The first consists of utilized flakes and utilized shatter with all but very steep edges. With the exception of quartzite artifacts, all had been used. These tools were most probably used in a wide range of cutting activities and for processing a wide range of raw materials throughout the site. Factor two is defined by some unutilized chert and all unutilized chalcedony waste flakes, although a hammerstone and several formally made tools are included. This factor is probably associated with the manufacture of chipped stone tools, primarily in the rooms and on the roof of the pueblo. Factor three is defined by chert and quartzite waste flakes. Again, chipped stone tool manufacture is indicated but in the areas away from the room block where the raw materials occur. I suspect that this factor may represent the initial processing of the raw materials.

Stone (1975) analyzed a large collection of chipped stone artifacts recovered during survey of the Chevelon drainage. Her approach was based upon "fuzzy set theory" a technique that is useful when the association of a particular observation with a particular activity is only probable rather than certain. The investigator specifies the probability in question and this inference becomes a part of the analytical procedure.

Stone's analysis required identifying the probable activities with which unutilized flakes, formal tools, and casual stone tools were associated. For each artifact category, the existing literature on stone tool use, consultation with colleagues, and inspection of the materials were used as Stone formulated the probabilities to be associated with each tool type. Actual observations are then transformed by the probabilities and become the data for analysis. The strength of this procedure is that the inference becomes a part of the analysis rather than a guess that is made after particular patterns of artifact association have been identified. Moreover, the result of the analysis is stated as a probability that a particular activity occurred at a particular spatial locus.

Using excavated materials, Stone found evidence of a high probability of stone tool manufacture and food preparation at all sites. The probabilities of hide preparation, butchering, hunting, and wood working were both generally lower and highly varied from site to site. A cluster analysis performed on the data formed two groups, one with a high certainty of all activities and the other with a high certainty of only a subset of all activities. Specialization in both room function and site function are suggested by the data.

Using the survey data, groups were again formed indicating different combinations of activities at different sites. A discriminant function analysis of the cluster results indicated an error of only 2.74% in assigning cases to the clusters. Butchering and food preparation were relatively useless in cluster formation; variation in these activities is so great from site to site that their presence at some sites and absence at others is likely to be unrelated to the other variables. Most of the variation is associated with differential evidence of chipped stone tool manufacture on sites where hunting also occured. Hide preparation, and the processing of hard materials are the next most important activities separating the clusters. As was the case with excavation data, the largest group is formed of sites where the probability is high of all activities being present. The remainder of the groups are defined on the basis of associations between chipped stone tool manufacture and various of the other activities.

The study provides little evidence of any linear variation in the degree of site specialization through time. Instead, the degree of specialization seems to vary as populations disperse and contract through the drainage or move into areas that were previously nearly uninhabited. Also, the more generalized site types are characteristic of periods of relatively more abundant rainfall. Among later and larger sites there is good evidence for the performance of specialized activities in specific rooms.

Gibson (1975) investigated the relationship between edge orientation, edge angle and raw material type using a sample of 3000 artifacts from the Chevelon drainage. Her study indicated that the use of a lateral edge was the most common pattern for all material types and that there was little difference between the end and lateral edge angles. She found a trimodal distribution of edge angles roughly paralleling those described by Wilmsen (1970). While all raw materials showed working edges corresponding to these peaks there was a marked tendency to use vitreous cherts for tasks that involved lower edge angles, inferably slicing and cutting tasks. Quartzite showed some tendency to be used in tasks requiring the steepest edge angles. Basalt and chalky cherts were most abundant in the intermediate category of edge angles although no argument can be made that they were selected for tasks requiring edge angles of this category.

Dobbins (1977) studied a sample of 1800 chipped stone tools from 28 sites in the Chevelon and Pinedale areas, dating to 1250 to 1325 AD. He attempted to assess the extent to which limited activity and habitation sites in the juniper pinyon and ponderosa communities had different lithic assemblages. He examined the size of limited activity sites and the percentages of tools, types of decortication flakes, and cores found on limited activity sites found within the two zones. He was able to show that they were homogenous. Habitation sites in the two zones are, however, different. Sites of the pinyon-juniper community have more tertiary decortication flakes and fewer primary ones, and relatively fewer cores. Dobbins argued that the relatively greater numbers of cores and primary flakes found on sites in the ponderosa zone suggest only occasional manufacture of stone tools at these loci. Artifacts reflecting later stages of the manufacturing process occur in greater abundance in the juniper pinyon area because these sites were occupied on a more permanent basis, he concluded.

Saunders (1976) studied 275 chipped stone artifacts from four contemporaneous sites in the Chevelon drainage. One habitation site and one limited activity site in the grassland and pinyon-juniper woodland zones were selected for the study. He found evidence that, while all stages of chipped stone manufacture are indicated at habitation sites, only the thinning of already decortified flakes and cores occurred at the limited activity sites. The characteristics of the flakes used at the sites proved to be generally similar. There was no evidence of patterning in the use of different raw materials at the sites.

Briuer (1976) used techniques borrowed from the police laboratory in an effort to understand the materials on which chipped stone artifacts were used. He examined organic residues on the edges of chipped stone tools, 37 from O'Haco Rock Shelter, a Desert Culture site, and several from Coyote Creek Pueblo, an open air site. Residues were present on the Coyote Creek artifacts despite their having been washed. Chemical tests indicated that the vast majority of the artifacts were used for plant processing, although blood was indicated on the edge of one of them. Briuer was also able to show that the organic residue on the stone tools was unlike that found on natural objects in the rock shelter.

M. Donaldson (1977) studied a collection of nearly 800 chipped stone tools from 5 limited activity and 5 habitation sites in the Chevelon drainage. The main difference she found between them related to the quantity and types of chipping debris. There is much more chipping debris at habitation sites than at limited activity sites although there is more evidence of immediate reduction of flakes from cores at the latter. There is also much greater evidence of selectivity of flakes for utilization at habitation sites. Differences in the length, width, and thickness of utilized, as opposed to non-utilized, flakes is significant at habitation sites but not significant at limited activity sites.

Grove (1977) studied a large number of characteristics of the raw material, tool type, and flake morphology of sites from the Little Colorado Planning Unit, west of Lakeside on the Apache-Sitgreaves National Forests. She attempted to distinguish between the lithic assemblages of artifact scatters, one or two room sites, and larger sites in this area. Few of the characteristics proved to be significant and those that did were questionable. These results stand in marked contrast to those obtained for the western portion of the Apache-Sitgreaves National Forests. The result is probably best understood in regard to the argument developed by Wood and others (Plog 1978). Basically, the evidence suggests that occupation of the eastern area was late and lasted for a relatively brief time. Given that the area is one of greater geomorphological activity than the more westerly areas, the structures on the "limited activity sites" may simply be buried. Alternatively, what may be represented is a series of large to small sites, with and without structures, all of which reflect colonization or temporary use of the area. This issue cannot be resolved at present.

Little effort has been spent in attempting to define types of ground stone artifacts in the study area. Generally, investigators have used an intuitive typology consisting of the following elements: one-hand manos, two-hand manos, basin metates, trough memtates, slab metates, palettes, mortars, and pestles. In some instances, manos have been further divided on the basis of bevelling. Mundie (1973) used statistical procedures in creating a more sophisticated typology for manos. Her results suggest that a relative few categories are necessary in order to classify

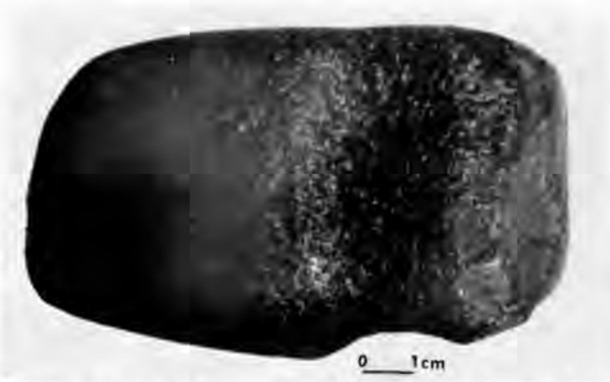

Figure 25. A ground stone axe from the overview area. This example is 10 cm long.

artifacts of this category. (See Figure 25 for an example of local groundstone.)

Slawson (1978b) examined a number of variables in an effort to separate aceramic from preceramic sites. Her basic strategy involved comparison of the lithic assemblages from ceramic sites with those from aceramic ones. Thus, the aceramic group must be assumed to include preceramic sites as well as sites contemporaneous with the ceramic ones on which ceramic artifacts were simply not used. As a result, the conclusions identified below could probably be strengthened by using them as a basis for separating preceramic and later sites and basing an analysis exclusively on those sites that have the apparently earlier characteristics. Major conclusions of the study were as follows:

1. Non-flakes (cores, shatter, and tools) are almost three times as abundant on aceramic sites as on ceramic ones.

2. Non-ceramic sites have a much higher frequency of secondary flakes.

3. Ceramic sites have a much greater abundance of tertiary flakes.

4. Quartz, orthoquartzite, and petrified wood only occur in non-ceramic sites and flints and limestone are far more abundant on them.

5. Andesite, jasper, calcite, and obsidian only occur on ceramic sites.

6. High percentages of edge angles greater that 76 degrees and less than 18 degrees are found on aceramic sites.

7. An abundance of edge angles in the 36 to 45 degree range occurs on ceramic sites.

8. More formally made cores and tools are found on aceramic sites and more different kinds of tools. There are some tool types that may be unique to these sites.

9. Utilized and unutilized flakes occur in greater abundance on ceramic sites.

Unfortuntely, the analysis also suggested considerable spatial variation in the results over the area that Slawson studied. Nevertheless, this suggestive evidence provides a substantial basis for beginning an effort to clearly distinguish between aceramic and preceramic lithic sites.

Most (1978b, 1979) has attempted to distinguish between various site types in the Pinedale area using lithic artifacts. The variables she considered include raw material, characteristics of the striking platform, cortex, and characteristics of the bulb of percussion. Using these variables pithouse, limited activity, and pueblo sites are generally similar. There are, however, two important areas of difference. First, raw materials are differentially distributed. Specifically, basalt tends to occur on the largest sites and to be more typical of later sites. This nonlocal raw material was imported into the area and possibly was processed at large habitation sites prior to distribution to inhabitants of smaller ones. Second, a number of variables that are associated with the production of tool blanks suggests that these artifacts may have been manufactured on limited activity sites.

In the Pinedale area, clear distinctions between habitation and nonhabitation sites are not possible. There are clearly some sites that would have been recorded as limited activity sites were it not for potholes that revealed walls. In combination with Most's data, most of the genuine limited activity sites in the Pinedale area may be loci at which raw materials were quarried from the Rim Gravels and initially processed. Given the highly ecotonal situation of the area, it is unlikely that separate resource extraction loci would have been necessary for most floral and faunal resources.

EXCHANGE

A final major line of analysis focused on the exchange of chipped stone artifacts or the raw materials from which they were made. Until recently, most efforts to study exchange have used exotic resources such as obsidian. In recent years, a number of studies done using data from the overview unit suggest that there is great potential in studies of the distribution of more mundane resources.

Rick and Gritzmacher (1970) analyzed roughly 800 pieces of chipped stone from 10 sites in Hay Hollow Valley. The materials they used were surface collected from the sites. Their investigation did, however, test the relationship between surface and subsurface deposits at one excavated site and they concluded that surface materials were a good reflection of subsurface deposits. They divided chipped stone raw materials into five categories: chert, agate, petrified wood, basalt, and quartzite. Sources for each of the raw materials were located with only basalt and agate having restricted sources. This argues for a linear decrease in abundance of material as distance from the source increases.

The sites studied were occupied between AD 300 and 1300. While the percentage of chert, the most frequently used raw material, remained relatively constant over time, utilization of agate decreased and utilization of quartzite increased. Since agate is a higher quality material, I suspect that the replacement of it by quartzite may indicate exhaustion of the source material. While unwillingness to undertake the quarrying trip is a possibility, the source is only a few miles away from the most distant site used in the study.

One aspect of the study on which the authors did not comment is the extreme variation in lithic density on the sites which they studied. The range is from 1.1 to 21.5 per square meter. The coefficient of variation is .59 and the chances are less than 1 in 1000 that such a set of observations would be drawn from a population with the same mean. This suggests that efforts to distinguish site types might well focus on variation in the overall quantity of chipped stone materials on sites and that there may be substantial variation in the quantity of material used at different points in time.

Green (1975) studied the distribution of chipped stone raw materials on sites of all time periods from the Chevelon drainage. She grouped raw materials into four categories: vitreous chert, chalky chert, volcanic, and quartzite. Throughout the sequence, vitreous chert was the most commonly used raw material, although chalky chert was most important in the southwestern corner.

Green found that different localities within the drainage, when evaluated using both SYMAP and cluster analysis, were relatively different from one another at all time periods. The raw materials used, with the exception of volcanic, were typically the most abundant locally available raw materials. Volcanic materials appear to have been traded into the drainage by peoples living to the west with the material entering the drainage at the confluences of Brookbank and Potato Wash with Chevelon Canyon. The distribution of the raw material follows the easternmost drainages within the Brookbank system. The period between AD 1175 and 1250 is marked by exceptionally high diversity in the use of raw materials. Nevertheless, the basic patterning is apparently spatial with much less indication of change in use patterns through time.

LePere (1979a, 1979b) has undertaken a thorough statistical study of the different raw material types used in the manufacture of chipped stone tools on the Apache-Sitgreaves National Forests. Her analysis indicates that chert and chalcedony were the most widely used resources with basalt and quartzite next in importance. Many less widely used resources (e.g., andesite, jasper, and siltstone) are used within relatively smaller spatial isolates. Obsidian occurs at low levels over the entire forest area.

A cluster analysis of the data, one that groups sites on the basis of different raw materials found on them, suggested several interesting conclusions. A three cluster solution basically segregated the western, central and eastern sectors of the forests. In an eight cluster solution, this division was still clear although the eastern sector was best defined on the basis of the high degree of variation in comparison with the other two clusters. In a 16 cluster solution, the eastern sector and the eastern third of the central sector show high variation in comparison with the remaining portion of the central sector and the western sector, both of which remain relatively consistent.

LePere also investigated the potential sources of the raw materials and in many cases was able to identify possible sources by simply using geological maps of the area and fall-off rates. Both linear and exponential rates were identified. Of course, the results of this analysis can be improved with field investigations to more specifically identify the sources. Some possible associations with site size and site type were also identified.

Findlow (n.d.) has studied obsidian from several hundred sites on the Apache-Sitgreaves National Forests. The technique that he has used to identify obsidian sources has been described previously (Findlow 1976). The major result of this effort is the discovery that, as far east as Pinetop, obsidian is predominantly from sources in the Flagstaff area. In the vicinity of Springerville and Eagar, Red Hill obsidian, from a source of that name in New Mexico, occurs in the greatest abundance, although Flagstaff obsidian still occurs in some quantity. The westernmost extent of exchange of Red Hill obsidian is the Pinedale area.

FUTURE RESEARCH

The studies described above reflect substantial progress in obtaining an initial understanding of the nature of variation in chipped stone assemblages. While there has been considerable growth of understanding of the information that can be derived from particular variables, there is also still considerable guessing concerning the variables that are most likely to yield information and the manner in which these should be defined. A number of important questions now exist.

1. What is the source of materials used in manufacturing chipped stone tools? While a number of studies are reported that describe variation in chipped stone raw materials with tool-type, site type,

or time, the studies are exclusively based on data from sites. To date, with the sole exception of obsidian, no effort has been made to identify the actual sources of particular raw material types through field work. Provenience postulates must be established if we are to understand the nature and extent of exchange that brought the raw materials to the loci where they were used.

2. What productive/extractive processes are reflected in formally made stone tools? Virtually all of the studies reported herein clearly indicate that the assemblages in the overview area are relatively casual assemblages: used and minimally modified flakes are the most typical tool. Behaviorally, a very eclectic pattern in the use of chipped stone tools is suggested. The relatively infrequent occurrence of formally manufactured stone tools leaves open the questions of why, and under what circumstances, prehistoric peoples chose to invest additional energy in manufacturing more specific tool types.

3. Can preceramic and non-ceramic sites be distinguished? The study reported above is preliminary in nature and in no way resolves the question of whether this distinction can be clarified.

4. What is the nature of spatial and temporal variation in lithic assemblages found on sites of different types? Some of the studies discussed above indicate one or more variables that can be used to differentiate the assemblages of habitation sites as opposed to limited activity sites. In other cases, however, no such differences were noted. Whether this information reflects differences in the organization of productive activities from one portion of the overview unit to another, or at different points in time, cannot be stated with any security at present. It does seem clear that, in at least some areas, limited activity sites are characterized by relatively less chipped stone manufacture; this suggests that they were occupied for relatively short periods of time.

5. Is there a typology that is useful for comparing chipped stone from different projects? The development of such a typology would be a major undertaking were one to assume that it should proceed from statistical analyses of different artifacts to the establishment of formal types. Something less than this may be useful, however, for communication at the same level of detail envisioned in the discussion of ceramic variation. Were a system based on the use of a relatively few attributes in existence, [such a typology would be perhaps not too different from that used by DeGarmo (1975)] variation in functional and manufacturing patterns of a magnitude that would imply major cultural or organizational differences might be evident.

SETTLEMENT PATTERNS: INTRA-SITE

During the last decade much of the intellectual investment in studies within the overview unit has been in studies of locational and settlement patterning. There is probably more detailed information on settlement related issues for various localities within the study area than any other area of comparable size.

In the case of both intra-site and inter-site patterning there are two general areas of analytical concern: the definition of the elements of settlement patterns and the analysis of the manner in which these elements are articulated and change in their articulation through space and through time.

ELEMENTS OF INTRA-SITE PATTERNS

While efforts to identify room types have a long history in studies of southwestern archeology, it is only within the last 15 years that systematic efforts to identify attributes of different room types have been common. Prior to this time obvious distinctions (between kivas and secular rooms, between pithouses and surface storage units) were commonly made. Recent efforts have focused on the identification of more subtle differences between rooms that served different functions.

Efforts to systematically define and verify the existence of different room types begin with the work of Hill (Hill 1966, 1968, 1970; Martin, Hill, and Longacre 1967). The goal of this effort was to identify differences in room sizes and features associated with habitation as opposed to storage rooms. Kivas and "clan rooms" are also recognized, although their definition was more problematical. Subsequently, Hill and Hevly (1968) described palynological data that also coincide with the habitation/storage distinction made earlier.

Zilen (1968) and Plog (1969, 1970) used data from the overview unit and surrounding areas in an effort to test the utility of Hill's distinction. Their analysis suggested that, while the size boundary was not absolute, a general distinction between two size modalities was associated with variation in room features at most of the sites in the area. Thus, the habitation/storage contrast seemed secure.

In a subsequent analysis of the relationship between room size and room function, Johnson (1970) found evidence of inter-site variation in average room size in Hay Hollow Valley. Because the average size of rooms on sites varied from site to site, no single quantitative criterion was clearly useful in distinguishing between habitation and storage rooms. However, Johnson did discover two size modalities on the sites he considered and argued that the habitation/storage dichotomy is valid even if there is no single quantitative distinction that is viable for all sites and/or time periods.

Ott (1970) returned to the question of ceremonial vs. secular rooms and ventured into the relatively difficult area of prehistoric ceremonialism in analyzing a series of excavated sites in the Upper Little Colorado and the Pine Lawn area of New Mexico. She compared changing characteristics of the features and artifact assemblages that differentiate kivas and houses.

Her analysis indicated limited evidence of any consistent differences in the artifact assemblages found in kivas as opposed to other structures, prior to about AD 1000. There is also limited evidence of features that differentiate the two classes before this date. The argument is not that there is no basis on which kivas can be defined prior to this period, but only that there is little evidence of strongly developed activity specialization. Interestingly, the change occurred at a time when there is evidence of a substantial increase in the overall degree of specialization in site artifact assemblages and an increase in the ratio of structures on a site to kivas.

Blank, Fischel, and Wild (1974) attempted to identify room types using material from excavated sites in the Purcell-Larson locality. Since there is little variation in room size in the area that is suggestive of a clear habitation/storage dichotomy, architecture could not be used to inform the analysis. They found little evidence of meaningful variation in quantities or kinds of ceramic artifacts. The most important distinctions concerned the presence/absence of significant quantities of ground stone and the presence/absence of evidence of tool manufacture (as opposed to tool use). All four possible combinations (e.g., with ground stone, without tool use) were discovered. It seems unlikely that so complex a pattern of specialization would have characterized intra-site patterns in the area. The distinctions that were found more probably reflect the last activities that were carried out in the various rooms and/or the nature of trash that was dumped in them.

Acciavatti (1974) provides some notion of the distributional patterns that occur on living surfaces when conditions of preservation are ideal. She analyzed material from rooms 5 and 6 of site 731 in the Purcell Larson locality. These two rooms, apparently occupied at about AD 1275 to 1300, were both burned.

Acciavatti separated the ceramics in the rooms into eating (bowls, ladles, pitchers), cooking (smudged jars), and storage (unsmudged jars) vessels. Limited neighborhood classification analysis was then used to group 1 by 1 meter excavation units on the basis of similarities in the proportions of these different functional categories. There was a tendency in room 5 for storage jars to occur along the walls and cooking and eating vessels in the vicinity of hearths, although there were some storage vessels near the hearths also. Room 6 was somewhat more complex, with a high density of storage vessels throughout the room. Nevertheless, cooking and eating vessels predominated in the vicinity of hearths. Interestingly, there was also evidence that black-on-white, black-on-red, and polychrome vessels are distributed differently, within the room.

Room 6 is somewhat of a puzzle in that it is 60 square meters, atypically large for the overview unit. Acciavatti (1974) observes that there may have been impermanent partitions dividing the room as the hearths are dispersed over it. Vessels may also have been on the roof rather than on the room floor, which was unconsolidated sand. As a result, the charcoal from the burned room simply graded into the sand; there was no clear floor surface. Finally, Acciavatti (1974) notes that sherds from a vessel that was clearly sitting on the floor of one room were found in the other room providing some idea of the magnitude of transformation processes that effect distributions at even the most pristine sites.

INTRA-SITE SPATIAL PATTERNS

There have been two major analytical traditions in the effort to understand intra-site spatial patterning. The first of these focuses on inferences concerning organizational patterns as these can be inferred from the distribution of artifacts among rooms. The major impact of this tradition has been the development of evidence suggesting increasing specialization of room function and activity structures through time. The second tradition has focused on the "construction cycle" or at least the construction process at sites (see Figures 26 and 27). The major impact of this tradition has been the suggestion that there are regularities in the manner in which sites grow, although these are affected by events that are occurring in the natural and social environment at the time construction is on-going.

Longacre (1966) has argued for differentiation of productive processes at Broken K Pueblo. He notes that while finished products are found throughout the pueblo, tools used in artifact manufacture are, in some instances, highly localized. Graving tools that might have been used in manufacturing bone artifacts, for example, occur largely in the northwestern corner of the Pueblo while arrowshaft tools are localized in the southern portion.

Anderson (1971) attempted to measure variation in the extent of social stratification from the time of Christ until about AD 1400. Her effort is based upon a distinction between "necessary and unnecessary" artifacts and their distribution in the rooms of the Hay Hollow, Gurley, Carter Ranch, Joint and Broken K sites in Hay Hollow Valley. While the distinctions that she made in analyzing the artifact assemblages are somewhat questionable, she describes a way of approaching the problem that is worth pursuing. Her analysis suggests that one cannot exclude the possibility of differential access to resources at any time during the valley's

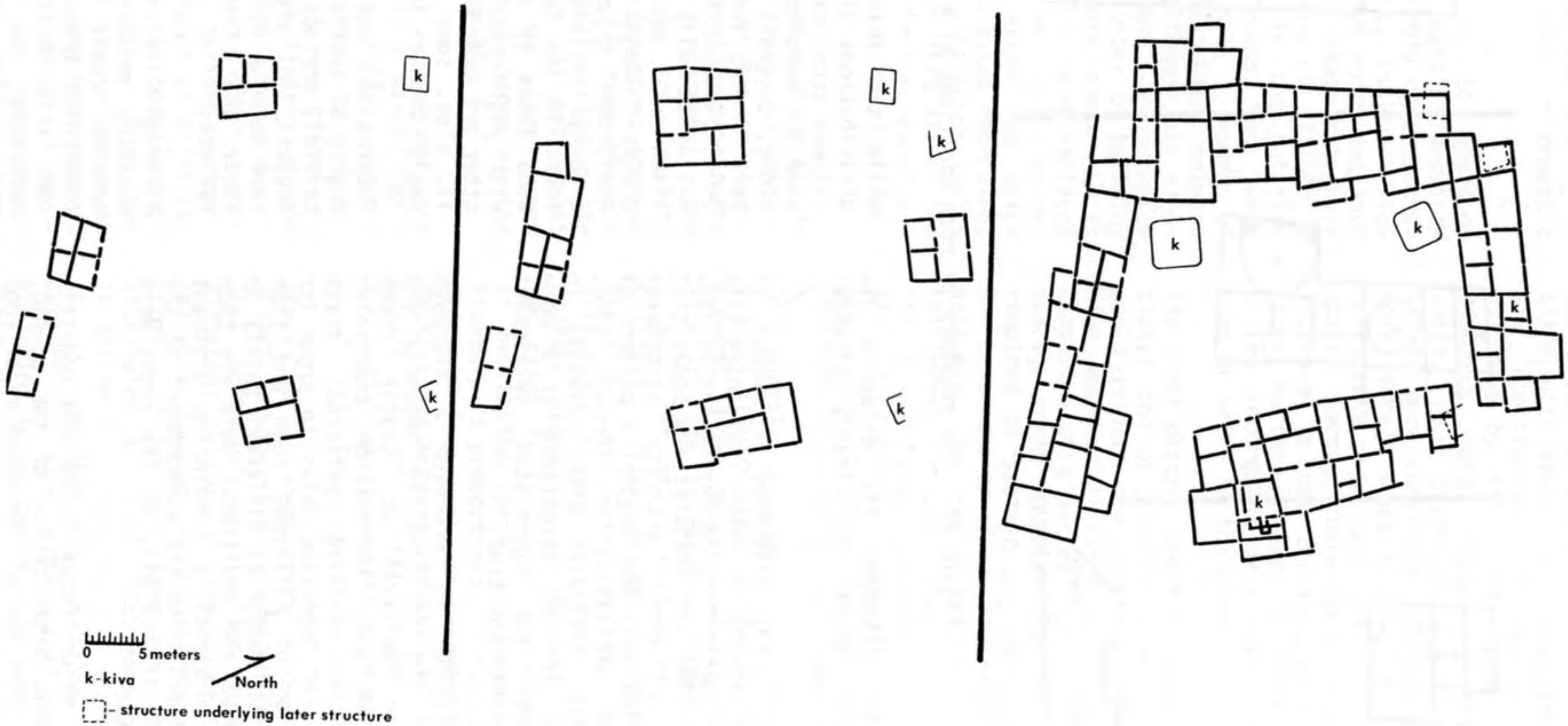

Figure 26. One interpretation of the construction sequence at Broken K.

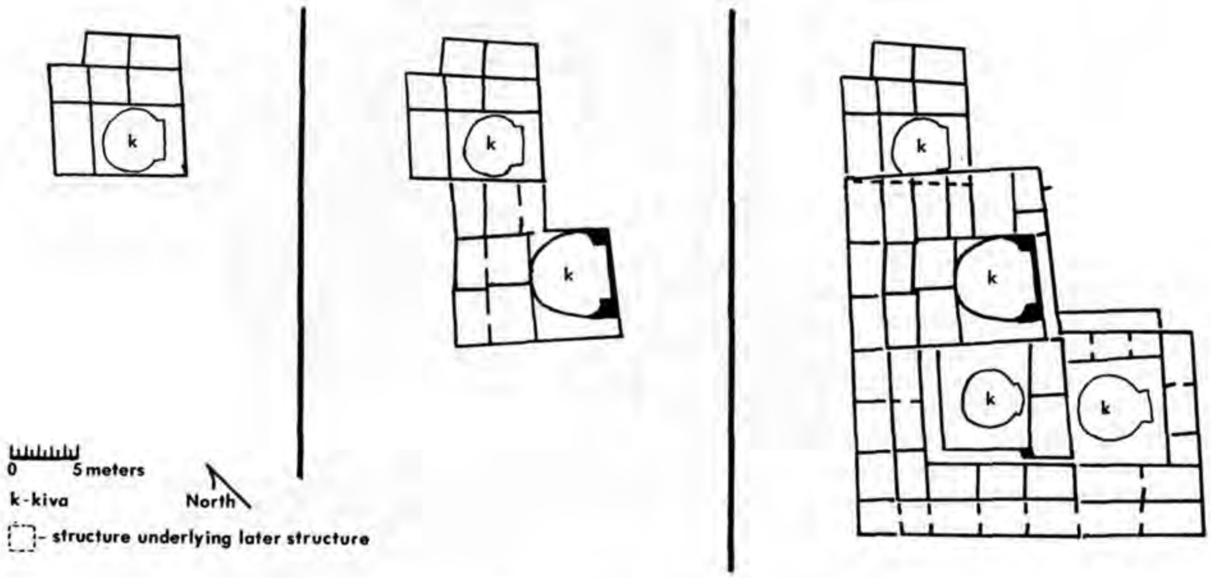

Figure 27. The sequence of room additions at Riatuthlanna.

occupation, although the evidence for differential access is clearly stronger toward the end.

Plog (1974) has discussed a number of different aspects of inter- and intra-site settlement patterning in Hay Hollow Valley. He argues that the features and tools that identify different activity structures within sites vary and suggest a pattern of increasing specialization through time. This change correlates with increases in population and the appearance of a more hierarchical and aggregated settlement pattern within the area.

DeGarmo (1975) has provided a thorough review of the construction sequence and interactive patterns at Coyote Creek Pueblo. He uses information concerning bonding and abutting patterns, trash deposits, and tree-ring dates to argue for construction of different room and room sets at the pueblo at different points in time. These and additional data are then used to construct a convincing argument that just prior to the abandonment of the site most, if not all, of the rooms were still in use.

DeGarmo's major focus is on the domestic and economic organization of the pueblo. He argued that two of the three major room clusters were the abode of domestic groups while the third was a work area. The distributions of tools used in ceramic and chipped stone manufacture, especially those used in manufacturing arrows and chipped stone, suggests some specialization on the part of the two groups. It is possible, DeGarmo suggests, that arrow-manufacture was a specialty for the inhabitants of the sites with members of the two domestic groups subspecialized in the process. Additional suggestions of specialization/exchange include: (a) parts of two bison found in the two domestic areas, and (b) bone tools in one domestic area are made with right-side bones while those in the other are made with left-side bones. There is, also, some differentiation of ceramic design between the two areas.

DeGarmo was not arguing for a substantial degree of specialization. Nevertheless, his careful, perhaps overly cautious, arguments suggest that productive specialization may have been a more pervasive pattern in the study area than the current literature reflects.

Although the recognition that bonding-abutting patterns and other architectural details could yield important insights concerning prehistoric behavior is an old one, this insight has only recently been harnessed to current anthropological theory. The relationships of construction patterns to stress, to the availability of resources, and to household or domestic cycles have been studied.

Driskell (1969) attempted to discover relationships between population decline/ subsistence stress and pueblo construction patterns. He argued that, in Hay Hollow Valley, the average size of rooms declined with population decrease. (Note elsewhere in this section that this is likely a product of an increase in the relative proportion of storage rooms that were constructed.) He does note a number of changes in construction techniques between about AD 900 and 1400, although his argument is based on only four. There was a general decrease in the number of building stones of which walls were composed, as banded or semi-banded walls were replaced by rubble-adobe construction. Walls become generally thinner, the extent to which construction stones were dressed decreased, and the practice of plastering walls decreased. Driskell argued that all of the above represent more economical construction practices as a result of decreased labor availability.

Schaefer (1970) also provided an analysis of the growth curve at Broken K. He was particularly concerned with the relationship between private and public space. The public space in question is essentially the plaza which is bounded by the dwelling units. Even the earliest few rooms seem to have bounded a plaza. While some of this "implicit plaza" was eventually used as the pueblo grew, the basic growth pattern at the site is outward from the plaza around which the first few room clusters at the site were built (see Figure 26).

Autry and Vaughan (1972) analyzed the settlement patterns of Hooper Ranch, Table Rock, Mineral Creek, Rim Valley, Carter Ranch, Broken K and the Joint Site pueblos. Their analysis began with an effort to describe the architectural history of the sites through a detailed analysis of bonding-abutting patterns. Having completed this analysis, they defined residential clusters, groups of rooms of different types that seemed to represent additions to an initial core group of rooms. They then attempted to determine whether variation in the nature of these clusters from site to site was best explained in terms of environmental stress or domestic group dynamics. They note two trends. First, there was a regular construction sequence at all sites with an initial heavy investment in the construction of habitation rooms, followed by the construction of proportionally more storage than habitation space. Then there was a return to relatively more construction of proportionally more storage space followed by a return to relatively more construction of habitation space. Second, there was a general overall increase in the construction of storage rooms during periods of time when the pollen records suggest that stress might have occurred in the area.

The first pattern might be either a generational one or related to the stress. It could reflect the construction of habitation rooms sufficient to underwrite the expansion of a nuclear family, followed by storage rooms necessary for the foodstuffs required to sustain children, followed by the construction of new households. Alternatively, it could reflect an initial settlement the inhabitants of which construct additional storage space during a time of stress and then ultimately resolve the problem by enticing more inhabitants/ laborers to the settlement.

One equally important point on which the authors (Autry and Vaughan 1972) do not comment is the substantial variation in the size of the residential clusters at all sites. The ratio of the largest to the smallest cluster at each site is between about 3:1 and 4:1. In one instance the difference is due to a greater quantity of storage space in the largest cluster, in two cases it is due to a greater quantity of habitation space, and in one the relationship is proportional. Thus, while both economic (storage) and social (habitation) factors can account for the difference, the possibility of a status-based explanation for variation in cluster size seems likely in at least two cases.

Tracz (1970) used formal analysis in an effort to understand variation in the rules that described construction techniques in different sectors of the Joint Site. Her analysis focused on 15 attributes of the rooms, ranging from area to such detailed attributes as presence/absence of pottery in the walls. Her analysis suggested that it is possible to describe different sectors of the pueblo that were built according to different rule sets. Whether these are ascribable to temporal or social variation is unclear. Nevertheless, the study suggests the potential of efforts to define residential clusters, or at least clusters representing an epoch of construction, on the basis of detailed attributes of the rooms.

Wilcox (1975) has summarized the architectural history of the Joint Site in Hay Hollow Valley. His analysis rested on a careful treatment of bonding-abutting patterns, the surface on which particular rooms rested, and tree-ring dates. He argued that the site was initially occupied by three social groups and that the later history of the site reflects expansion of these groups. Although the rate cannot be determined, the site grew as a result of aggregation and not a massive and planned labor investment. This essay probably provides the best available model for the analysis of the architectural history of relatively large puebloan sites.

Wilcox (1975) has also analyzed the architectural history of Broken K. His work supports the notion that each of the four main room blocks grew from a small core of rooms. Thus, the "plaza" was defined from the very beginning of the site.

A regular alternation in the construction of habitation, storage, and/or habitation/storage combinations is evident in the various room blocks. The number of rooms constructed does vary between room blocks. Contemporaneity of construction is uncertain. At precisely the same points in the construction sequence (but again not necessarily contemporaneously) large clan rooms are added in each wing. This pattern is strongly suggestive of one that would result from the relatively regular growth of family units in each of the four room blocks. Habitation rooms were added when children left their natal residence. Storage rooms were added to accommodate more children. After some critical density was reached, a clan room was added for meetings. There is, of course, the alternative possibility that some of the storage rooms were added to meet environmental problems, and that the addition of clan rooms reflects organizational change with little relationship to the household dynamics of the pueblo.

Hanson (1975) has argued that a major change in occupation pattern occurred near the end of occupation at the Joint Site in Hay Hollow Valley. Early in the history of the site, there was a very typical dwelling-living unit that consisted of a habitation room (firepit), preparation room (mealing bin and/or mano-metate complex), and a storage room (featureless). The last rooms constructed at the site were larger, multi-functional rooms. Hanson suggested that the experimentation and economy reflected in these rooms are a response to stress. It is worth noting, however, that the clear-cut room function pattern that seems to be common in the Upper Little Colorado is less common in surrounding mountainous areas. Thus, this change could represent the adoption of a different architectural style.

FUTURE RESEARCH: ROOM TYPE

While the ability to distinguish between habitation, storage, and ceremonial rooms provides an important analytical basis for studies of intra-site settlement patterning in the study area, there are a number of areas in which our understanding is less precise than it should be.

The room size/room function equation seems to work best in the plains portion of the overview area. In the more mountainous areas, a bimodal distribution of room sizes is not apparent. Thus, activity structure in sites that are composed of these multifunctional rooms is relatively poorly understood.

While the habitation/storage dichotomy is now relatively well understood, the secular/ceremonial distinction is not. Watson Smith's thorough discussion of "kiva-ness" (1952) has made it clear the "kiva" features are not exclusively associated with this architectural form. No study of variation in the presence of kiva features with respect to architectural types, not to mention an assessment of the strength of covariation, has occurred in the overview unit. Thus, the degree of distinctiveness of ceremonial units is problematical.

Little effort has been invested in recent years in the study of construction techniques. It is clear that Chaco, Mesa Verde, and Kayenta "styles" are all present within the area and that more than a single style can occur at one site. Jacal and possible adobe architecture are also found.

Because little effort has been expended in a badly needed study of the overlap of these different styles, it is not possible to use them in studies of either spatial or temporal patterning.

Clan rooms remain somewhat of a mystery. Exceptionally large rooms occur on even quite small sites and seem to contain relatively distinctive artifactual assemblages, a point to be discussed later. Nevertheless, there is at present no good definition of such rooms, not to mention an evaluation of their function. The same comment holds for "mealing rooms," rooms with stone boxes and/or metates set into the floor. These are evidence of the first clearly specialized processing space in Southwestern sites. Yet, no study to date has compared them with respect to size, attributes of architecture, or space-time distribution.

PATTERNS AND PRINCIPLES OF SETTLEMENT LAYOUT

There is no easy way to summarize these specific studies in terms of a few predominant sites types. However, for the pithouse and pueblo periods some synthetic statements are possible. Because pithouse data are more limited, these will be considered in some detail with a focus on broader principles for the Puebloan sites.

Gladwin's (1945) description of the White Mound type provides evidence of a very typical settlement pattern and architectural form that occurs throughout the overview unit, although not at all sites. White Mound Village was a series of clusters consisting of a variety of combinations of pithouses and surface structures. The pithouses varied considerably in shape, from circular to subrectangular. While the surface rooms were generally rectangular, there is considerable variation in size and the presence/absence of "basins" inside the structures. Somewhat larger houses with benches that may have been kivas are also present. Structures with and without hearths and with and without V-shaped walls emanating from the hearths were recorded. The general pattern of the settlements was a line or arc of surface, probably storage, structures around a cluster of pithouses and a single possible kiva.

Sites that are virtual mirror images of this one occur elsewhere in the overview unit, as far south and west as Wild Cat Canyon near the Forest Development Road 504. While excavation has been done at only a few other sites, surface indications suggest a White Mound pattern. Invariably the sites, or perhaps better, multi-site communities, are among the largest in the areas where they occur. Thus, there is a strong suggestion that these may have been ritual, economic, or political centers and that their distinctive characteristics are a reflection of functional patterns rather than temporal patterns.

The Flattop Site (Wendorf 1950, 1953) is a pithouse village located in the Petrified Forest. The site consists of 25 structures on top of a mesa. Eight of the houses at the site were excavated. Only one of these had a hearth and only one a firepit. The houses were generally about 35 centimeters deep and 2.5 to 3 meters in diameter. All were slab-lined and were circular to oval in shape with an inclined entry way. Some corn was recovered from the site. A relatively large ceramic collection from the site was entirely Adamana Brown.

The Twin Butte Site, also in the Petrified Forest, was described by Wendorf (1951, 1953). The site is probably best viewed as one settlement in a multi-site community because there are several other nearby sites as well as suggestions of water control features. Eight, of a total of twelve, structures were excavated. Both surface, semi-subterranean, and subterranean structures occurred on the site. These are variable in size, shape, and depth. The living structures and storage cists occur both individually and in crescentic groups around "kivas". Eight burials at the site contained shell ornaments, turquoise, and argyllite. There is variation suggestive of status differences. Corn was recovered from some of the storage cists. Lino Grey, Woodruff Brown, Lino Black-on-Gray, and La Plata and White Mound Black-on-White were the major components of the ceramic assemblage, suggesting a Basketmaker III occupation of the site.

Three, of a possible total of four, houses were excavated at the Tumbleweed Canyon Site (Martin et. al, 1962). The structures varied from circular to D-shaped. All of the houses were lined with basalt boulders and all had hearths, although only one had a storage pit. No ceramic artifacts were recovered from the site, but a single radiocarbon date suggests occupation at about AD 300.

Gumerman (1966) excavated two pithouse sites near Houck. NA 8937 consists of oval pithouses that average 60 centimeters in depth and 4.5 meters in diameter. The houses all had hearths on a gently sloping, unprepared floor. The absence of interior postholes suggests a sloping self-supported roof. The house contained no internal storage pits. A single Cibola White Ware sherd, believed to be intrusive, was recovered from one of the houses. Four pithouses were excavated at NA 8971. While these were also oval in shape they were on the average smaller (3.75 meters) than those at NA 8937. The structures contained hearths, postholes, and, in two cases, meter deep storage pits. Pueblo II ceramics were recovered from the fill and again identified as intrusive. Corn pollen was recovered from sediments at the site. Radiocarbon determinations were deemed "worthless" at the time and were not reported. Given subsequent evidence of the late occurrence of pithouses in the area, it is entirely possible that these sites may represent late aceramic manifestations, although the basis for arguing a Basketmaker II association is evident.

The Finger Rock Site (Gumerman 1979) is north of Winslow, actually outside of the overview unit proper. Both rectangular and circular structures are present on the site with a diversity of floor features. The pottery is predominantly Lino Gray, but both Lino Black-on-gray and White Mound Black-on-white are present. Some later ceramics suggest that there may be more than a single occupation at the site. In this regard, the site is a classic illustration of the problem of determining whether one is dealing with a single multifamily settlement or a series of sequential occupations of a particular location.

The Connie Site (Thompson and Longacre 1977, Rogge, in prep.) is located on a point of a mesa in Hay Hollow Valley. The site consists of 35 pithouses, 11 "smaller structures," 6 features bounded by a cobble arc, and 1 probable kiva. Seven houses excavated at the site were relatively uniform. They were 4 to 5 meters in diameter and about 30 centimeters deep. All were rocklined. Hearths were generally present along with a vertical deflector slab. In a general sense, the houses are arrayed in an arc around the probable kiva. The site is radiocarbon dated to about AD 225, although there is an archeomagnetic date of AD 650 and 690. Pottery from the site is uniformly Adamana Brown. The Connie Site has a companion site, NS 243, on the same mesa top. The architectural characteristics of the site are similar, and the ceramics identical. In general, there are many similarities to the Flattop Site, although there appears to be somewhat more of a village pattern.

This list by no means exhausts the total of excavated pithouse sites in the overview unit. These do represent the larger and more complete projects. Reports on the excavation of a single pithouse allow little opportunity for the discussion of diversity. Some pithouses and pithouse sites are shown in Figures 28 and 29.

A number of "patterns" can be described on the basis of the data that have been reviewed. I use the term pattern because I do not wish to leap to the conclusion that these represent either temporal or spatial units. First, architecturally, there are four patterns that we may refer to as the Adamana, White Mound, Finger Rock, and Mogollon patterns. The Adamana pattern is that expressed at sites such as Connie, Flattop, and Tumbleweed Canyon. All of these are mesa top sites and the structures are rocklined. This is not to say that there are no differences between the sites. The houses at Connie and Tumbleweed, for example, have hearths while those at Flattop do not. Connie and Flattop have Adamana or Woodruff Brown pottery while Tumbleweed has none. But, there appears to be a basic similarity in the location and architecture of these sites. Their spatial distribution appears to be to the east of Silver Creek (which is interesting in light of the preceeding discussion of the distribution of Desert Culture projectile point styles). There are no records of such sites further south than Hay Hollow Valley. They may well occur further northward and eastward than the boundaries of the overview unit. Temporarily, their placement is a problem. Connie may date to either about AD 200 or AD 600. Tumbleweed Canyon dates to about AD 300. Flattop has been dated to AD 300 on the basis of the occurrence of Adamana pottery in Hilltop phase contexts at the Bluff Site, a chronological argument

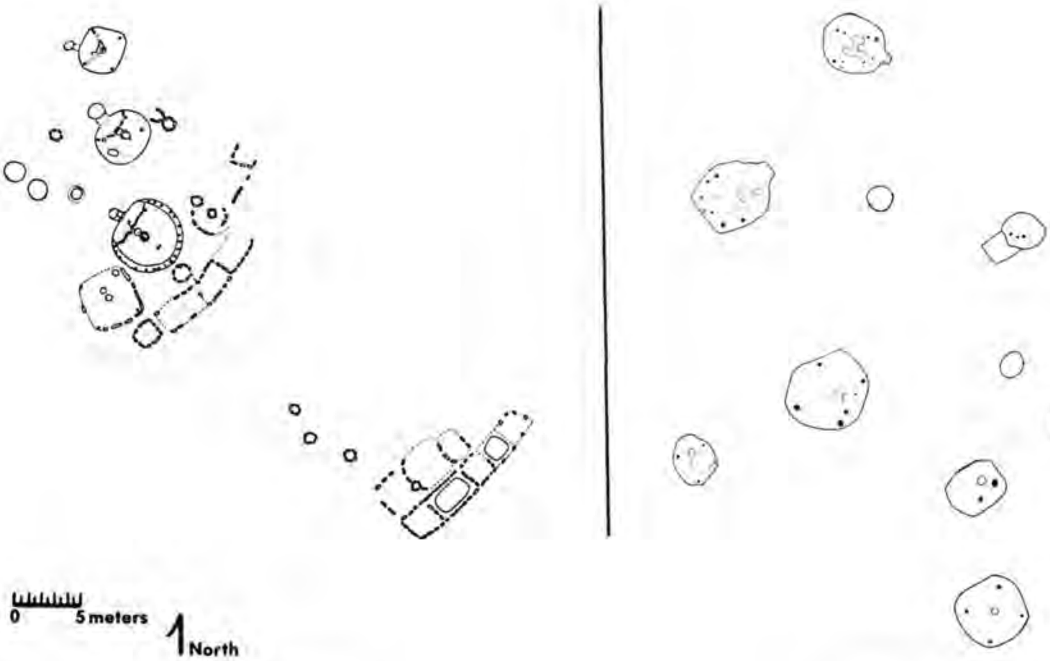

Figure 28. Variation in the plan of pithouse villages in the Little Colorado overview unit.

that Gladwin (1948) has questioned in rather telling fashion.

The White Mound pattern is expressed at White Mound, the Whitewater Sites, and Twin Buttes. It consists of relatively deep pithouses with rather consistent floor features along with surface living and storage rooms. Again, while sites are by no means identical, the homogenity is striking. In fact, I would suggest that there is more similarity among White Mound sites than any other widespread architectural tradition in Southwestern prehistory. Spatially, such sites barely cross the rim at Walnut Creek (Morris 1970), clearly extend northward and eastward outside of the overview unit and may occur beyond the westernmost boundary also. Well-dated sites seem to suggest that this pattern was characteristic between AD 700 and 1000.

The Finger Rock pattern is defined by the absence of a pattern. These sites are characterized by enormous architectural variability, but usually appear to be small, on the order of four or five rooms. There is no clear evidence that the structures are contemporaneous. In this sense, the sites might represent no more than the periodic occupation of a desirable hunting, gathering, or farming location by a family-size group or the coming together of peoples with distinctive technological traditions in a small village. These sites occur throughout the overview unit. Temporal placement is difficult, although the Gurley Sites in Hay Hollow Valley, a classic example of the pattern, date to AD 500 and AD 1000.

Pueblo period village patterns are far more difficult to synthesize (see Figures 26 and 27). First, there are many more sites that have been excavated or mapped. Similarly, it is far easier to record the pattern of Puebloan sites on survey than is the case with pithouse villages. Second, once above ground masonry construction had become common, the prehistoric engineer-architects had available an almost unending array of different combinations that could be employed in constructing sites. Most of these options appear to have been used in one circumstance or another. For this reason, it is far easier to identify the principles that underlie the variation than any set patterns. Apart from obvious variation in size, there are four major principles: aggregation of rooms; association of kinds of rooms; focus; and planning.

Puebloan sites within the overview area vary incredibly in regard to the extent of aggregation of rooms (Figures 30 and 31).

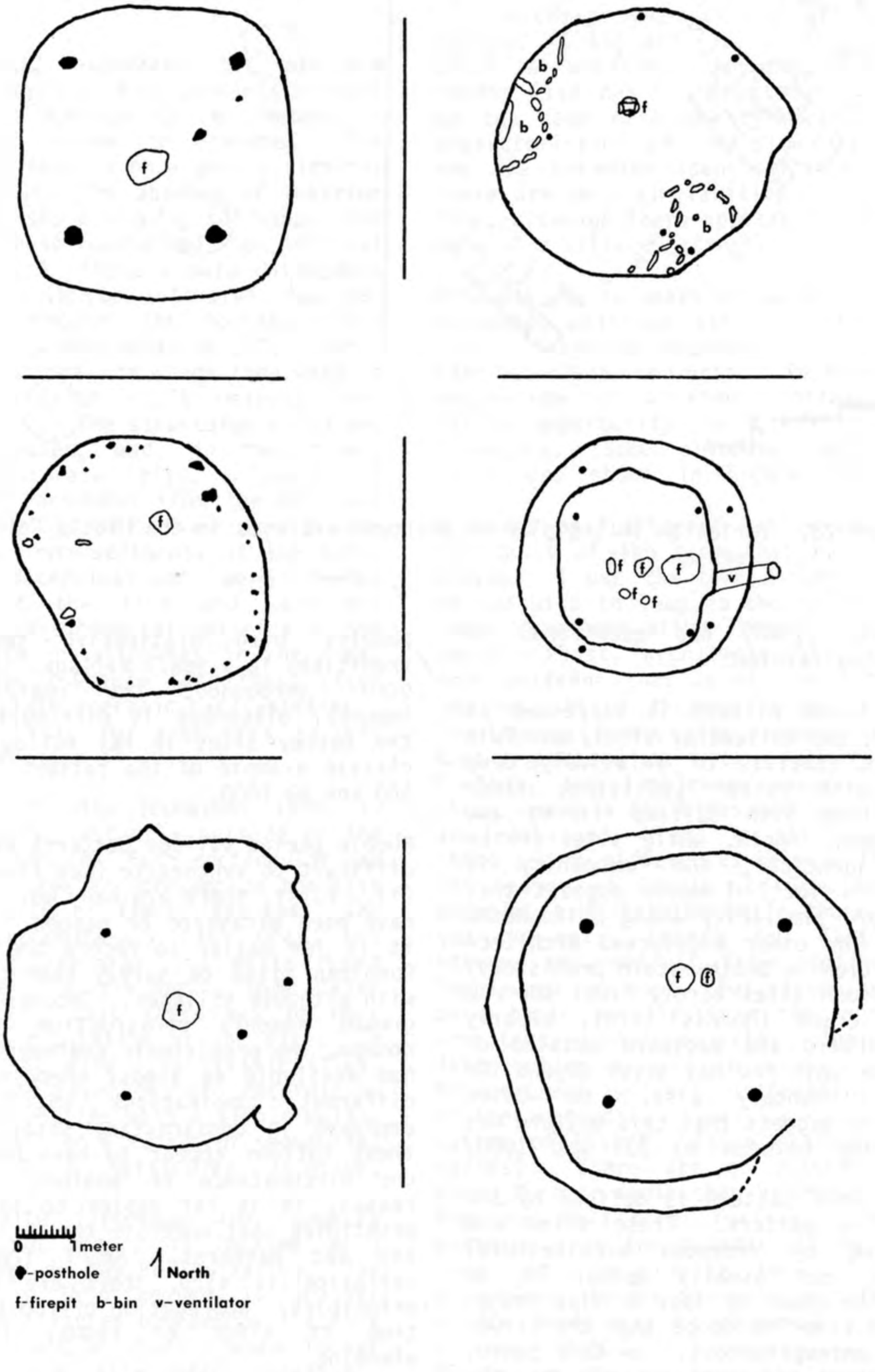

Figure 29. Variation in pithouses in the Little Colorado overview unit.

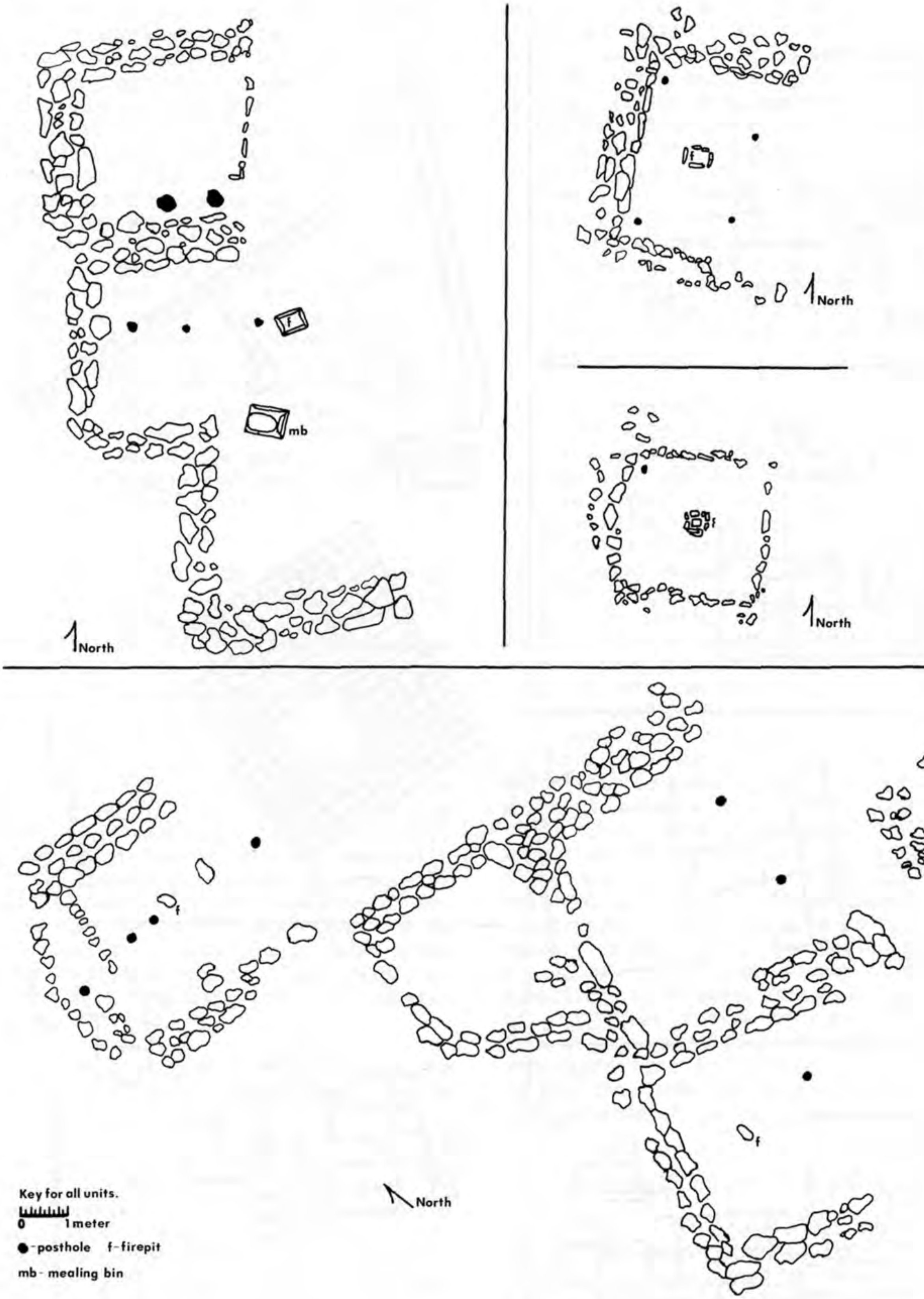

Figure 30. Variations in the plans of small pueblo sites in the Little Colorado overview unit.

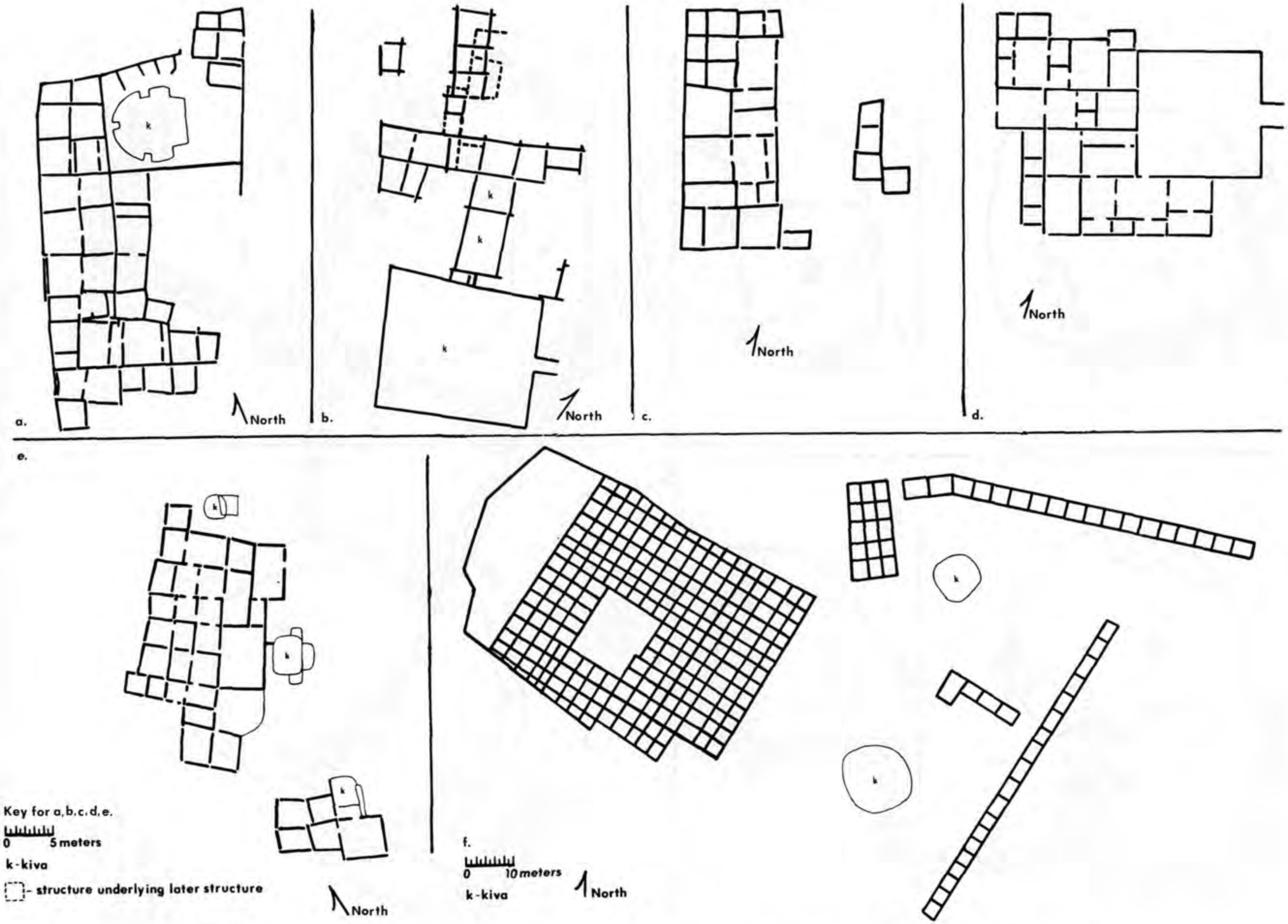

Figure 31. Variations in the plans of larger pueblo sites in the Little Colorado overview unit.

Aggregated sites are ones on which the rooms occur in one or several contiguous blocks. Disaggregated sites are ones on which the rooms occur as isolates. I know of very few completely disagregated sites that have more than just a few rooms. The largest disaggregated sites are several in the Purcell-Larson drainage that range in size from one dozen to two dozen rooms. Aggregated sites may be quite small. There are many examples in the overview area of "unit pueblos," or at least sites that resemble unit pueblos. These are composed of blocks of four rooms. The very largest sites in the study area are generally aggregated. Of course, there are many examples of sites that vary between these two poles. Even the largest aggregated sites often have a few outlying rooms. And, there are some sites, site 689 in the Chevelon drainage is an example, that are composed of a single aggregated room block with an equal number of individual outlying rooms.

On aggregated sites, the manner in which particular rooms are associated also varies. The predominant pattern is one in which habitation, storage, and kiva rooms are associated in a complex fashion. Broken K (Hill 1970) and Carter Ranch (Longacre 1970) are examples of this pattern. Another pattern is one in which storage rooms occur in association with kivas. Site 201 (Zubrow 1975) in Hay Hollow Valley is an example of this pattern. Kiathuthlanna (Roberts 1931) shows an association of storage rooms with kivas, the former surrounded by a ring of habitation rooms. In the Chevelon drainage, the definition of room types is an almost impossible task since room sizes vary little within sites. Thus, exploration of this principle is not always possible.

The focus of Puebloan sites refers to the feature around which rooms appear to be aggregated or dispersed. It is of course difficult to discuss this issue in the case of very small sites. In the case of larger ones, it is meaningful to distinguish between plaza, kiva, front, and compound focused sites. Plaza-focused sites are ones such as Broken K (Hill 1979) where the construction of rooms appears to have occured around a plaza. At Kiatuthlanna, a kiva is the focus of each of the major construction units (Roberts 1931) and a series of kivas are, thus, the foci of the site. Front oriented sites are ones on which entrances all appear to face in a single direction. In a sense, the space outside such units is an unbounded plaza. Finally, sites such as those described by Gumerman (1960) and Gumerman and Skinner (1968) near Holbrook are constructed within a compound. In the case of very large and complex sites, multiple foci may be present. At Pinedale Ruin (Haury and Hargrave 1931) there are both compound and front focused components to the site. To a minimal extent, one can talk about focus in the case of disaggregated sites. In the Chevelon drainage, for example, great kivas appear to occur in the center of clusters of smaller sites.

Finally, Puebloan sites vary in the degree to which they are planned. In general, planning refers to the presence of a focus for the site and to some evidence of cooperative effort in erecting at least portions of the site. However, the specific evidence of planning is highly variable. As we saw at Broken K, planning could have involved little more than an implicit notion of public space, the idea of a plaza, with the construction of particular rooms around the plaza appearing to represent discrete construction events or clusters of events. At Kiatuthlanna, construction apparently occurred in larger units. At this site most of the room walls in major architectural units seem to have been erected relatively simultaneously. At still other sites, a combination of cooperative-planned and individual construction seems to have occurred. Walls oriented in one direction were sometimes constructed at a single time using a single masonry style. The perpendicular walls that abut these show highly varied construction techniques, probably the result of individual family or clan efforts. I know of no excavated site within the overview area done in precisely this fashion. It was the technique used at Nuvaqueotaka, just outside of the overview area.

FUTURE RESEARCH: PATTERN AND GROWTH

At present, few generalizations can be drawn from these studies--they are simply too few in number and too restricted to a limited sector of the study area. However, a clear case has been made for the careful analysis of construction techniques and sequences at <u>all</u> future excavated sites in the overview unit. Such data could be used in the following sorts of analyses.

While most of the sites discussed above appear to have grown from relatively slow accretion, there are departures from this pattern. The site of Kiathuthlanna, for example, appears to have been built in highly planned segments. At Broken K, the existence of the plaza seems to have been implicit in the very earliest stage of construction at the site. The difference between planned and unplanned sites is a critical one since the former implies a degree of labor control or coordination that the latter does not. It is unclear whether planning was characteristic of some sites at all time periods: if this were true, these sites would probably be important central places or distinctive of particular time horizons. This would imply greater local organization but not necessarily centrality. Clearly this information is necessary to understanding the prehistory of the area.

The nature of residential clusters and of their growth sequences must be better understood. Of primary importance is an effort to combine studies of stylistic variation in ceramic materials and of functional or stylistic variation in other artifacts with residential data. The only study to date that approximates this approach is that of DeGarmo (1975). Not accidentally, his provides the most thorough evidence for the existence of both domestic groups and task specialization. Status differences among residential clusters remain largely unexplored. Statistical "pattern searches" are less likely to yield valuable results when they are applied to site space as if it were undifferentiated when, in fact, architecture can be used to provide a structure on which statistical analysis can be framed.

Detailed control of the epochs during which rapid construction of storage rooms occurred is vital. At present, it is equally likely that these reflect either a change in subsistence strategy associated with greater numbers of people living at sites, or a change in organizational strategy, with some sites serving as storage/redistributive centers, in a response to stress. Of course, these possibilities are not mutually exclusive. But the substantial variation in the construction of storage room through space and through time is an intriguing phenomenon that will certainly yield valuable insights.

Changes in construction techniques present a similar problem. Where detailed studies have been done, as at Broken K and the Joint Site, there is obvious variation. As noted earlier, the magnitude of this variation can even extend to include the presence of more than one of the traditional Southwestern styles at a single site. Whether such variation represents the degradation or improvement of construction techniques in response to environmental change, migration, or other social dynamics cannot be stated at present.

Analysis of wall bonding-abutting patterns at sites where dates can be assigned to different construction epochs is an obvious need.

SETTLEMENT PATTERNS: INTER-SITE

In the preceding section, on-site settlement patterns were considered. In this one, relationships between discrete sites are the focus. In general, inter-site studies rely heavily on observations of the surface conditions of sites rather than on excavated materials. In some cases to be considered the latter are used, but they are rare. Nevertheless, inter-site studies are clearly dependent to a very substantial degree on a thorough understanding of intra-site patterns and on the manner in which particular of these patterns are, or are not, manifested on site surfaces.

ELEMENTS OF INTER-SITE PATTERNS

There are probably few concepts in archeology that are more problematical than that of site type. The difficulty with the term reflects two considerations. First, as archeological sophistication in making inferences concerning behavior and activities in the past increased, acceptable levels of detail for defining site types increased. Second, success in defining site types is considerably dependent on the profession's ability to relate key pieces of surface information to subsurface deposits.

No effort is made here to generate a detailed definition of site types for the area, one that reflects behavior and activities. Instead, the discussion will focus on surface artifacts and surface features. While such an approach leaves much to be desired, site types defined even in this crude fashion present difficulties. The first topic of discussion will summarize efforts to define more specific site types in response to a variety of management and research problems. The second identifies studies that have been done in an effort to refine definitions of functional variation among sites.

Low Density Artifact Scatters

Low density artifact scatters, commonly termed "non-sites" in the literature, are different from prehistoric sites. Sites are discrete and interpretable loci of cultural materials. Low density artifact scatters lack the quality of discreteness and may also lack interpretability. They are relatively large areas characterized by a low density of artifactual materials, often less than one artifact in a 10 square meter area.

Lithic Scatters

Lithic scatters are defined by the exclusive presence of chipped and ground stone artifacts. Known sites of this type in the overview unit range from 1 square meter to over 1000 square meters. Their precise interpretation is at present unclear because two very different patterns of human behavior generate such sites. As mentioned earlier, prior to approximately AD 1, Southwestern peoples did not manufacture ceramic artifacts. Therefore, the sites reflecting their presence are almost exclusively lithic sites. Even after Southwestern peoples made and commonly used ceramic artifacts, some of their activities were carried out at loci where ceramic items were not a necessity--butchering and hunting camps are examples. Differentiating between these two behavioral patterns is extremely difficult, and analyses undertaken to date have proven unsuccessful (Slawson 1978).

Ceramic Scatters

Ceramic scatters are defined by the exclusive presence of ceramic artifacts. In general, the presence of ceramic materials allows assigning at least a rough date to such manifestations. These sites are generated by prehistoric human activity that, in respect to nonperishable artifacts, involved the exclusive use of ceramic containers. Such containers were used for cooking, for the storage of water, and for the storage and processing of foodstuffs. They may also have been used as boundary markers for the fields or lands associated with a particular settlement.

Artifact Scatters

Artifact scatters are defined by the presence of lithic and ceramic artifacts. These sites are generated by at least three distinctive patterns of human behavior.

First, they are produced by resource extracting behavior requiring the use of chipped and ground stone and ceramic artifacts in collecting resources. Second, they are generated when a locus is used for habitation but the habitation structures were so ephemeral in character that they leave no surface evidence. Third, they are generated when permanent habitation structures are obscured by later natural and cultural transformation processes. Preliminary analysis indicates that the majority of these artifact scatters in the study area are associated with activities other than habitation since their artifact inventories are distinct from those of habitation sites (McAllister and Plog 1979).

Petroglyphs/Pictographs

Petroglyphs are drawings made on rock surfaces by pounding those surfaces with a hard instrument to create a pattern. Pictographs are made on rock surfaces using pigments (see Figure 32). There are many known petroglyph and pictograph sites in the overview unit. These sites may reflect the efforts of prehistoric peoples to communicate with one another, or may be simply aesthetic expressions. Some scholars argue that these sites can be dated, while others question this claim. Some argue that the drawings are interpretable, others disagree. That the sites can yield valuable information is indicated by one glyph in the vicinity of Chavez Pass Ruin on the Coconino National Forest. This glyph is a presentation of Quetzalcoatl, a Meso-American god. In this instance the particular representation of Quetzalcoatl is one that is sacred to stone workers. This discovery illustrates the possibility of drawing symbolic connections between peoples of different areas using the rock art.

Water Control Devices

Southwestern peoples used reservoirs, irrigation ditches, terraces, gridlines, and check dams as mechanisms for water and soil control (see Figures 33 through 38). Examples of each are known from the study area. Terraces were constructed by placing rocks on top of one another to a height sufficient to level the land surface behind the terrace. Gridlines are also lines of rock, usually only a single course in height, aligned to closely follow the contour of the land surface. Contour plowing is the closest modern analog to gridding. Check dams are defined by rock alignments, usually one but sometimes more courses in height, placed across stream channels perpendicular to the flow of the stream. These served to slow the flow of water through the channel, reduced erosion by capturing soil suspended in the stream water, and increased the level of ground moisture in the channel.

Shrines

Shrines are a category of cultural resources the definition of which is somewhat of a problem. They normally are defined as low stone walls enclosing a circular or quadrilaterial area on the order of one or a few square meters. A shrine may consist of several such arrangements. Beads, ceramics, and chipped stone artifacts and a variety of esoteric materials may be associated with shrines. In the study area, shrines occur at high altitudes--on mountain peaks and overlooking the headwaters of major drainages.

Rock Shelters

The earliest "roofed space" that existed on the National Forests were rock shelters, erosional cavities in cliff faces that were used for perhaps occasional, perhaps permanent, human habitation. A large and a small rock shelter are shown in Figures 39 and 40. The most common occurrence of these features is in the larger and deeper canyons, Chevelon, Wildcat, and Brookbank, but they are also found elsewhere on the National Forests. These sites represent particularly important cultural resources because they often contain stratified deposits that yield information concerning changes in prehistoric behavior through time. Also, materials such as basketry and cloth not normally preserved in Southwestern sites are preserved in rock shelters (see Figure 41).

Pithouse Sites

Prior to about AD 1000, most habitation or living sites occupied by Southwestern peoples were pithouse villages (see Figures

Figure 32. Pictographs in Chevelon Canyon.

Figure 33. Agricultural terraces at Nuvaqueotaka.

Figure 34. Check dams in the Chevelon area.

Figure 35. Cleared field, Hay Hollow Valley.

Figure 36. Fossilized canal segment near St. Johns.

Figure 37. Vegetation marking buried irrigation ditch in Hay Hollow Valley.

42 through 44). Pithouse is a term that covers a multitude of sins. Some Southwestern pithouses are only a few dozen centimeters in depth while others are over 2 meters deep. Nevertheless, with rare exceptions, pithouses were built by erecting timber supports in a pit, laying branches and/or reeds against these to form walls and roofs, and covering these with dirt or adobe.

Pithouse sites are relatively difficult to identify, especially when the houses in question were relatively shallow. Their presence can be indicated by some combination of circular depressions, circular vegetation patterns, circular patterns marked by the absence of vegetation, circular configurations of wall stones or cobbles. Pithouses may be present on sites without any substantial surface indications.

Pueblo Sites

Pueblo sites are defined by evidence of above-ground masonry architecture (see Figures 45 through 49). These sites are characteristic of Southwestern peoples after about AD 1000, although there is substantial evidence that some peoples residing in the study area continued to live in pithouses well after this date. Pueblo architecture is markedly diverse. "Field houses" (Pilles 1979) are marked by a simple pile of boulders covering an area of several square meters. The associated artifact density is typically quite low. These structures were probably used seasonally in association with plant cultivation activities in fields.

Small U-shaped structures of one or two rooms are characteristic at higher elevations in the study area. The artifact density associated with these structures suggests that occupation at the sites, or at least the production of artifacts, was far greater than at field houses.

True "pueblo" architecture has four full standing walls. In the overview unit, these sites also typically average about two to four rooms. Their artifact inventories suggest, however, that they may have played a distinctive role in trade or exchange relationships within the area.

Figure 38. Irrigation ditch revealed in cross-section by archeologist's excavation.

Figure 39. Adobe-walled granary M. Chevelon Canyon.

Figure 40. Rock shelter in Brookbank Canyon.

Figure 41. Baskets from rock shelter in Chevelon Canyon. Each measures about 20 centimeters in diameter.

While both U-shaped and true pueblo sites are typically small, there are larger examples. Within the overview unit, the largest known site of U-shaped structures has roughly 40 rooms and the largest true pueblo has about 400 rooms.

Great Kivas

Great kiva sites are defined by the presence of large (ca. 15-25 meters diameter), usually circular depressions. These sites represent the centers of ceremonial activity among prehistoric peoples. Great kivas sometimes occur as features on pueblo sites, but they also occur in total isolation. While their principal importance was ceremonial, these sites also seem to have served as important centers of exchange and trade.

Compounds

Compounds are a completely enigmatic site type. They are defined by substantial masonry walls enclosing rectangular areas between approximately 300 to over 1000 square meters. The artifactual assemblage of such sites is generally quite different from that of contemporaneous sites, although the manner in which such sites differ is highly variable. While their precise role in regional settlement systems is currently unknown, they too apparently served as centers of trade and exchange within the study area.

Defensive Sites

Attribution of defensive characteristics to sites has waxed and waned in the literature. When this concept has been criticized, attention has been directed to the casual manner in which the term has been used, sometimes in reference to sites that are on a moderate hill (Figure 50). Nevertheless, there are sites in the overview unit that can be defined as defensive based on relatively firm criteria. These sites have one or more of the following attributes: 1) an inaccessible location--reaching the site involves a difficult climb taking at least several dozens of minutes; 2) low visibility--the site can be seen from only a relatively few points in the surrounding area, if at all, and; 3) defensive walls--(Figure 51) there are examples in the overview unit of walls bounding a site that are up to 3 meters in height and 2 meters in thickness.

ANALYSES OF SITE TYPES

Johnson (1970) provided a number of useful insights concerning local settlement

Figure 42. Pithouse site in Chevelon Canyon. The surface is marked by ground stone and upturned cobbles and boulders.

Figure 43. Woodruff Butte site. Houses are marked by substantial stone circles.

patterning. His data are from Hay Hollow Valley. He was particularly interested in the possibility that sites in different environmental niches were involved in different exploitative activities and that there were central places in which these productive activities were integrated. The results of his analysis do little in respect to the first claim. However, in regard to the second, he suggests that: 1) larger and more central places had twice as many storage rooms as surrounding sites; 2) sites generally occur in discrete clusters with one very large, or a few relatively large, settlements in each; 3) within each cluster, sites occur in loci that suggest different productive activities; and 4) larger sites, taken alone, are more evenly dispersed than the aggregate of sites.

Johnson's focus is on the period between AD 950 and 1100. His research suggests that the coordination of specialized productive activities through central places was present in the area by at least this time.

Hirvela (1971) tested a number of hypotheses concerning the relationship between settlement shape and potential independent variables including the size of the site, the distance to usable raw materials, and the physical setting. Testing of the hypotheses proved difficult and most tests were negative. However, the best correlations that she found were between the shape of the settlement and the presence of 25 or more rooms. In other words, large sites are not simply larger than small ones but generally differ in respect to the formality of the pattern of the settlement. This evidence suggests that the labor expended in the construction of the site is likely to have been both greater and more formally organized than at smaller sites.

Coe (1972) considered a number of possible relationships between environmental stress (as defined by Hevly 1974) and changes in material culture. Her analysis suffers from small sample size (four sites) and a rather tortuous argument that two of the sites, which were contemporaneously occupied during a stress period, are separable into stress and non-stress categories since one is near what is now a permanent stream. Nevertheless, the effort did show some interesting relationships between site size and the presence of ornamentation and indicated significant variation in the overall density of artifacts between the sites.

Gregory (1975) described the excavation of six one-room structures in Hay Hollow Valley. The effort was intended to provide some evidence of the function of these sites in the settlement system. However, recovered artifactual materials were too limited to provide any firm basis for inferences. Nevertheless, primarily on the basis of their small size, Gregory

Figure 44. A pithouse is exposed by erosion in the wall of an arroyo near Nuvaqueotaka.

Figure 45. Homolovi II seen from the air.

Figure 46. Ruins of a field house near Nuvaqueotaka.

Figure 47. Chevelon site 690, a Pueblo site.

Figure 48. Chevelon site 689, a pueblo site with exposed walls but no mounding.

suggested that they were a functionally specific class of sites. He noted the importance of understanding such sites for the interpretation of settlement patterns in the area. For example, a calculated nearest neighbor statistic of 1.14 for the period AD 1000 to 1100 increased to 1.81 if the apparent field house sites are omitted. Thus, the apparent randomness of settlement patterning in much of the overview unit may be the product of the relatively casual decisionmaking involved in locating such small sites. This can mask a very regular and highly dispersed pattern for the major dwelling sites.

McAllister and Plog (1978) attempted to identify criteria that would clearly distinguish between small "field house" sites and larger dwelling units in the Chevelon drainage. Variations were investigated in the presence of different types of chipped stone, ceramic, and ground stone artifacts on sites of different sizes and different time periods. Analysis of variance and discriminant function analysis were the major statistical techniques used. Their analyses failed to identify any clear cut evidence that the range of activities carried out on sites of different sizes varied. The claim that small sites were functionally different from large ones was not supported.

Wood (1978a) proposed a site typology on the basis of his study of the layout of sites in the Springerville area. His approach is essentially intuitive. Nevertheless, the study illustrates the substantial variation in the presence of rooms, kivas, plazas, and their layout on sites that can occur within a very small spatial area.

Dove (1979) has analyzed ceramics obtained from three-wall, as opposed to four-wall, sites in the Chevelon drainage. His analysis suggests that corrugated ceramics are more typical of the latter than the former. This pattern may be a temporal one, since corrugated wares are later than plainwares. However, immediately to the west of the study area, plainwares are characteristic at all time periods. Thus, the four-wall sites may reflect some interaction or cultural affiliation with corrugated ware producing groups to the south or to the east.

Preliminary analyses also suggest that there is some organizational significance in the presence of one or more rooms with four full standing walls in areas where the typical pattern is one of three foundation walls. In the Chevelon drainage, (Plog n.d.), black-on-red and polychrome ceramics have a statistically significant association with such sites (x = 19.49, p= .001). Similarly, over 80% of the examples of exotic materials (such as shell, steatite, and turquoise) occur on such sites. Great kivas and larger three-wall sites were apparently functional equivalents as distinctive ceramics and exotic materials are also characteristics of these sites. Nevertheless, why the presence of four standing walls should result in so distinctive a pattern is unclear. Some central role in local settlement systems, probably related to exchange, is strongly suggested.

A number of additional studies are pertinent to differentiating sites of different types, specifically those that deal with ceramic and lithic variation. These were discussed in an earlier chapter and will not be repeated here. Ceramic variation seems to provide a valuable tool for identifying the role of different sites in local and regional settlement patterns. The value of chipped stone studies is less clear. Surface collections do not seem to yield interpretable results with any regularity for issues other than the differential use of raw materials.

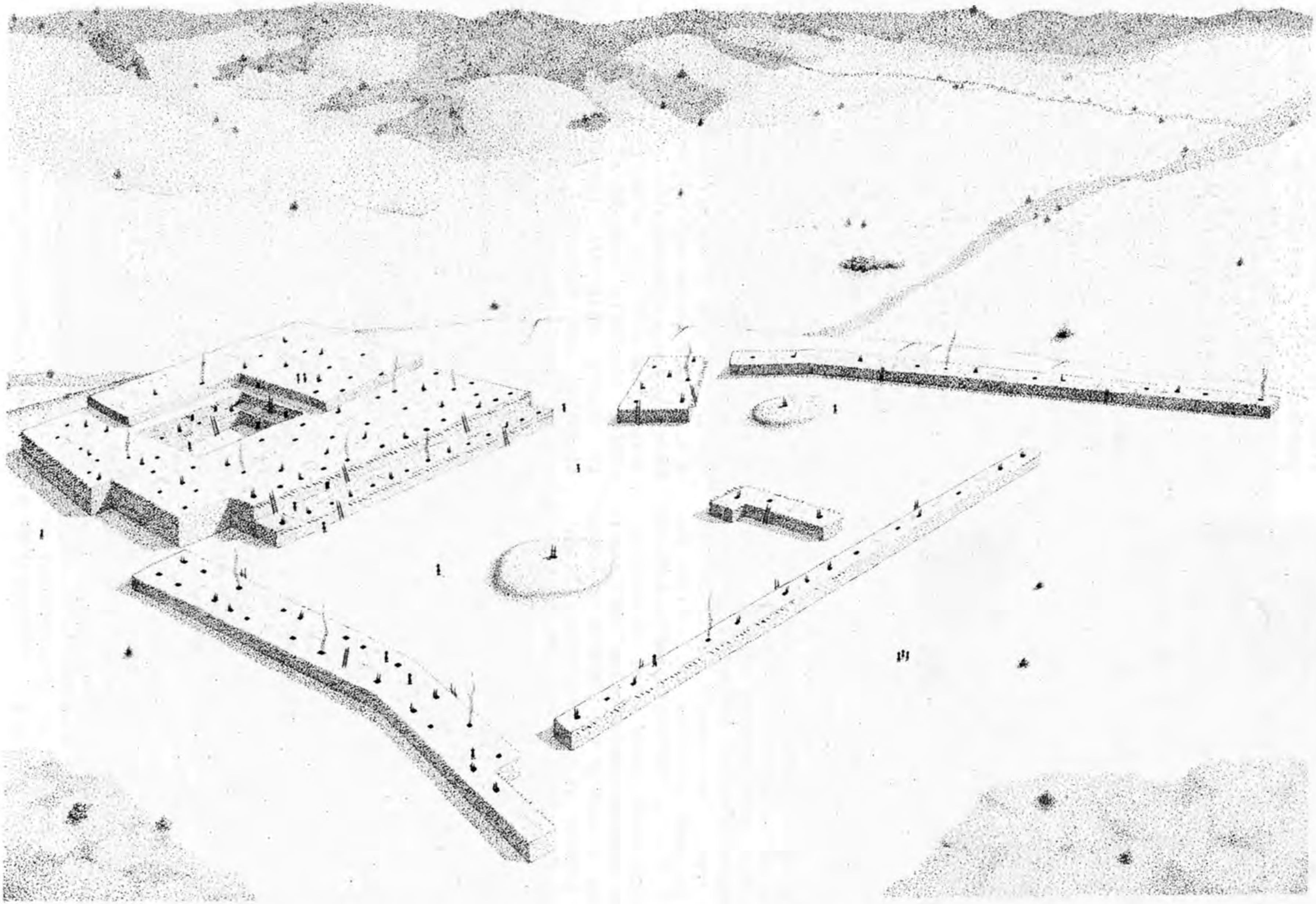

Figure 49. Artist's reconstruction of Four Mile Pueblo near Taylor, Arizona (Fewkes 1904; Lightfoot, personal communication).

Figure 50. An aerial view of site CS-189, a defensive site in Chevelon Canyon.

Figure 51. Site CS-189 in Chevelon Canyon.

SITES AND THEIR ENVIRONMENTS

A next set of studies has focused on locational patterning, the relationships of sites to one another and to various natural environmental features. Because many studies include analyses of both types, they are considered together.

Plog (1968) undertook a spatial analysis of the Longacre-Rinaldo survey data from of the Upper Little Colorado area. The analysis includes a reconstruction of population trends, changes in the distribution of sites, and changes in their location in respect to water sources and landform.

Bargen (1968) evaluated locational patterns in Hay Hollow Valley by comparing expected and actual distributions of sites. The valley was first divided into a series of roughly .5 miles by .5 miles squares. Each of these was evaluated on the basis of soil quality, availability of water, topography and erosion. A Monte Carlo simulation of potential population movement between the different squares was then undertaken.

He found that the actual distribution of sites corresponded fairly closely to the distribution postulated on the basis of the environmental ranking when evaluated using both chi square and rank correlation: the squares with the most desirable environmental conditions had the most sites. With the Monte Carlo simulation added, the correspondence between the actual and expected distributions were less close, although the rank correlation value remained significant.

Schiffer (1968) investigated the relationship between economic diversity and population growth in Hay Hollow Valley. The paper is useful principally for the manner in which it illustrates a number of different ways of generating population curves from the same data. It also illustrates means of testing to determine if some of the apparent variation in such a curve may be due to cultural or natural transformation processes that effect the surface record. Vanasse (1968) and Duncan (1968) generalize some of Schiffer's results to other parts of Hay Hollow Valley and the Upper Little Colorado area generally.

Derousseau (1969) analyzed the pattern of site distributions in Hay Hollow Valley from about AD 600 to 1100. She was able to demonstrate an increase in the extent of clustering of settlements.

McCutcheon (1969) investigated the relationship between changing climatic patterns and the locations of sites in relation to water sources in Hay Hollow Valley. Dendroclimatological data were used for the climatic reconstruction. Her definition of distance to water source attempts to exclude channels that are a product of recent arroyo cutting and to include the locations of potential, if not currently flowing, springs. The results suggest no predictable pattern of change in site locations relative to water sources as the climate of the area changes.

Powers (1970) analyzed settlement patterning in Hay Hollow Valley using Theissen polygons. Her analysis focused on the period between AD 1000 and 1100. Performed for all sites, the analysis yielded complex results. There was no clear relationship between the population estimate for the site and the amount of land surrounding it. As a result, she postulated the existence of large central sites with multiple site communities surrounding them. This analysis was successful: there was a good correlation between the size of the polygon and its estimated population. Her analysis, then, suggests the existence of multisite communities in the area by at least AD 1000.

Sandor (1974) generated a series of cross-tabulations of locational and cultural attributes of sites in the Chevelon Drainage. Perhaps the most important pattern that he detected concerns the contrast between earlier and later sites. While the former occur in far more diverse environmental settings (especially in respect to elevation and vegetation), the latter are more diverse in respect to their cultural characteristics. Sandor does note that many of the earlier sites are larger than typical later ones, although the period when sites in general, and habitation sites in particular, are smallest occurs in the middle of the sequence.

Findlow (1974) used multivariate statistical techniques to summarize the locational characteristics of sites in the Chevelon drainage generally and in the Purcell-Larson locality specifically. Measures of slope, vegetation, and distance to water were employed in the analysis. Since the sites were the "cases" used in the analysis, the results do not take into account the environmental characteristics of locations in which sites are not found.

Nevertheless, Findlow identified nine locational types and used SYMAP to illustrate their distribution within the Purcell-Larson area. He provides some comments, based on ethnographic data, concerning the probable interpretation of the locational types.

Loria (1975a, 1975b) attempted to evaluate the relationship between site densities and environmental variables in the Show Low and Pinedale areas. Her analysis focused on vegetation, altitude, precipitation, and soil. The first three variables are highly correlated and the extent of overlap between them is not evaluated in the study. The areas in which sites are most likely to be found are characterized by ponderosa pine or a mixed ponderosa, juniper, pinyon pattern; elevation of 6600 to 6800 feet; 15 to 18 inches of precipitation per year; and gravel loam soils.

F. Plog (1975a) compared the settlement patterns of the Hay Hollow and Purcell-Larson areas. The article attempted to identify a number of key indicators that could be used in such comparative studies. Some major differences in the nature of settlement patterns of the two areas are identified: at most time periods, sites in Hay Hollow Valley were larger and denser. Population in Hay Hollow Valley was subject to more rapid increases and decreases (see also F. Plog 1975b). There were also many more sites without structures in Hay Hollow Valley than in Purcell-Larson. Tentative explanations were offered for the differences between the settlement patterns in the two areas.

Plog (1978) described the relationship between site locations and environmental variables in the Chevelon drainage. While a number of different potential environmental predictors are significantly associated with the presence of sites, the overall percentage of the variance accounted for by the model is not great.

Vegetation is the best overall predictor of site locations, with the greatest number occurring in the juniper pinyon woodland. A number of different statistical techniques are used in the analysis with varying results.

Jewett (1978) undertook a detailed study of spatial patterning in the Pinedale area using transect data and compared these with the results of block survey data. Her analysis showed a strong association between site distributions and ecotonal conditions. Variation in site size and site hierarchies through time are also demonstrated. This study probably represents the most detailed application of a variety of locational techniques to a single locality and illustrates the problems that arise from, and the advantages of, such as approach.

Hantman and Jewett (1978) compared the settlement patterns of the Purcell-Larson, Pinedale, Little Colorado Planning Unit and Hay Hollow areas. Their results indicated that substantial variation is characteristic of the area. Site densities vary from a high of 50 per square mile in Hay Hollow Valley to a low of 12 near Springerville. Hay Hollow has the most average rooms per site (12.8) while Springerville and the Purcell-Larson areas are least (2.3 and 2.5 respectively). The percentage of sites that are limited activity sites, or at least lack structures, varies from highs of 74% in Pinedale and 73% in Hay Hollow to a low of 34% in Purcell-Larson. Morill's index of continuity was used as a measure of site spacing and varied from a value of .5 indicating clustering in the Purcell-Larson area to a high of 1.35 in Pinedale, indicating dispersion.

Wood (1978a, 1978b) generated a predictive model of site locations for the Little Colorado Planning Unit. His data show that vegetation community and soils provide good indicators of relative site densities, with landform also an important factor.

Grove (1978) has used a number of different locational techniques in attempting to understand settlement patterning in the Bagnal Hollow locality. She used SYMAP in an effort to determine whether sites of different types were associated with landforms of different elevations. While the results of the study were mixed, they do suggest some differences between pithouses and pueblos.

In addition, there appear to be two very different and, perhaps functionally specific, classes of artifact scatters which occur at different elevations. A rank size analysis of sites in the drainage is convex, suggesting a multiplicity of small, independent systems. A variety of different spatial statistics failed to indicate any significant departure from random in the distribution of sites.

Adams (1978) has summarized a number of locational characteristics of sites in the Purcell-Larson area. Both site density and site size increased regularly, but not drastically, through the sequence of occupation. The average number of rooms per site was never greater than 3.0. Rank-size distributions are generally convex to plano-convex, suggesting multiple small systems within the locality. The major exception is between AD 1050 and 1125 when the distribution suggests a more hierarchical system.

Legard (1978a) calculated nearest neighbor statistics for the Chevelon Juniper Push, Pinedale, and Purcell-Larson areas. While she noted variation through time and through space, none of the statistics proved to be significantly different from random. Extrapolating these results to previous studies that did not evaluate the significance of the statistic, one must suspect that most, if not all, of the apparent variation in the nearest neighbor statistic over space and over time is just that.

Slawson (1978a) has used a number of locational techniques in describing settlement patterns in the Pinedale area. Like several other investigators, her analysis suggests that structural sites had a very different and much more dispersed pattern than all sites and than non-structural sites alone. She argued that the larger sites discovered in timber sales in the area are secondary centers to Pinedale and that these secondary sites, but not Pinedale Ruin itself, are surrounded by a zone of smaller habitations and then by a zone of limited activity sites. All of the nearest neighbor statistics on which she based her inferences did prove significantly different from random.

Legard (1978b) attempted to differentiate pithouse, pueblo, and limited activity sites on the basis of five environmental variables: landform, elevation, facing, orientation, and distance to nearest water. Using discriminant function analysis, she was unable to detect any significant differences between these types. She also determined that the rank-size relationship for the area was convex, suggesting multiple independent centers (although it is far less so than that for some of the other areas discussed).

Millett (1981) undertook a number of studies of spatial patterning in Hay Hollow Valley between AD 850 and 1300. Nearest neighbor and various indexes of aggregation/agglomeration are discussed in relation to the postulated evidence of stress during the latter part of this period. Included are SYMAPs of both room and site distributions during the period in question that suggest the existence of settlement clusters, perhaps multi-site communities within the valley.

Blank (1979) has summarized a number of different aspects of our current understanding of site distributions in the Pinedale area. She noted that there is little evidence of a hierarchical settlement pattern defined using rank-size criteria. However, Blank also noted that the largest sites in the area were farther from one another than are smaller sites. She discussed the difficulties in achieving precise population estimates in an area where it is evident that even relatively large room blocks are sometimes buried.

Preliminary evidence from the area suggests that low density artifact scatters in the area are typically associated with only one or two periods of occupation. The greatest number of low density artifact scatters are associated with periods of rapid population growth at about AD 1000 and 1250. The first epoch is associated with the colonization of the area, and the second with the single most rapid period of population increase. Factors that affect the distribution of sites over the area were also considered.

Lightfoot (1978b) has argued persuasively for the existence of multi-site communities in the vicinity of Pinedale. His report summarized earlier thoughts concerning the existence of such an organizational and settlement pattern in the area. He also covered some of the pertinent ethnographic information and ceramic correlates to such a possibility. His basic approach was to use univariate and multivariate statistics to control for variation in time and in vessel function so that the remaining variation could be considered largely in regard to spatial boundaries. He was able to identify different clusters of sites in the study area in two of the four time periods under study. Of particular importance are two clusters, multi-site communities, that existed during the penultimate phase of occupation. The two are distributed parallel to one another in such a fashion that ecological and climatic differences between sites within each community are maximized.

Lightfoot was able to demonstrate that each community had one larger settlement with a kiva and that there are statistically significant differences in the ceramic design traditions associated with the two communities. While the paper utilized a more limited data base than would be desirable, it provides an excellent model for efforts to identify inter-community interaction while controlling for other variables.

In a subsequent paper, Lightfoot (1979) expanded on the theoretical and empirical reasons why multi-site communities might be present in an area with environmental diversity similar to that which is known for the study area. He argued for the presence of at least a one-tier system of managerial elite, and explored pertinent evidence. Lightfoot (1979) has also summarized the evidence of parallel problems and responses among Mormon communities in the area.

F. Plog's (1981) analysis of environmental patterning on the Apache-Sitgreaves National Forests is the most comprehensive effort to build a predictive model undertaken in the area to date. While the analysis may not be appropriate for lower elevations in the overview unit, since these elevations are not present in any quantity on the forests, it does indicate that elevation is the best site predictor at high altitudes with the vast majority of sites (88%) occurring below 7000 feet. Other variables improve predictability only slightly. There is a strong suggestion that, were more detailed soil maps available, soils would greatly improve the prediction.

FUTURE RESEARCH: SITE TYPES

The distribution of great kivas is only roughly known at present. We need some understanding of the locales within the overview unit where these commonly occur in association with sites and those in which they are more typically found in isolation.

To understand the importance of redistribution in the area, variation in the ratio of storage to habitation rooms must be understood. We should also study the possible association of kivas and great kivas with sites with larger than expected numbers of storage rooms, must be understood.

The nature of major distinctions among sites without architecture (time, organization, or function) is almost completely unknown at present. On Black Mesa, most such sites, when excavated, have proven to have structures (S. Plog 1978). If this same pattern exists in the study area, a major component of the settlement pattern is being missed at present.

Pithouses use apparently persists on sites almost until the abandonment of the overview unit, either alone or with pueblo structures. Whether these represent functionally different sites, or ones with ethnically distinct inhabitants, is unclear at present. Again, it is unlikely that the prehistory of the overview unit can be understood without clarification of this issue.

While criteria for distinguishing defensive sites from other sites have been identified, these do not satisfactorily resolve the question of the nature of such sites. Whether they date to particular time horizons must be known if we are to understand the occurrence of conflict in the overview unit. Similarly, whether they are homologs of non-defensive sites in all criteria save locations, or whether they represent distinctive functional or organizational components of the settlement pattern, is not known.

FUTURE RESEARCH: LOCATIONAL PATTERNING

Our current understanding of locational patterning in the area is best discussed by separating environmental and organizational issues. The principal efforts to understand environmental patterning in the area have resulted from a combination of planning studies for the Forest Service and Southwestern Anthropological Research Group (SARG) oriented efforts. As such, these reflect a relatively mechanical effort to predict site locations. The success achieved has been considerable and it is apparent that elevation, vegetation, landform, and soils should be a beginning point for any effort to predict site locations elsewhere in the overview unit.

At the same time, these studies have offered little insight into the reasons for the relationships that were discovered. First, sites of different time periods have rarely been separated. Second, little work has been done at the multivariate level that attempts to separate the interaction effects of the different variables. Finally, since the studies have rarely been coupled with excavation data, determining precisely what resources have been exploited in particular locations has been problematical. Badly needed at present are: (a) efforts to obtain better samples of floral and faunal remains from sites in the area, and (b) efforts to develop more complete models of the likely behavior of agricultural and hunting/gathering populations in the area generally and in respect to different microenvironments within it.

Evidence of organizational patterning is somewhat more complete but still tantalizingly incomplete. It is now obvious that there is immense variation in the size, density, and distribution of sites at different times and in different places in the study area. What is not now obvious is how this diversity was articulated, if in fact there is any sense in which the region was integrated. That some regional integration existed is strongly suggested by the growing evidence.

When limited activity sites, including field houses, are removed from site distributions there is an indicated pattern of dispersed site clusters. This is true in most, if not all, areas that have been studied to date. The existence of these clusters is also suggested by the convex rank-size curves that have been obtained in most studies, curves that suggest small autonomous systems. Missing from most such analyses are the largest and potentially most central sites that exist within the study area. No block or sample survey done to date has included one of these sites; their records result from the early and unsystematic surveys. It is entirely possible that if such sites were integrated into existing studies, a linear rank-size relationship would be indicated.

There is very little in the way of innovative analyses that is required for remedying the deficiency that exists at present. (Useful variables have been identified and shown to be operational.) The integration of more diverse data sets, especially those including larger and more central places, with current studies should provide a substantial increase in our understanding of organizational patterning. (Of course, far more can be done as our understanding of the "elements" of the settlement pattern **is refined.)**

Inter-site variability in the study area remains, on balance, poorly understood. One can clearly go too far in attempting to distinguish between the functions of different sites in a settlement pattern. At present, however, the needed effort is only beginning. Through excavation, and, when possible, more detailed surface maps of sites, architectural and artifactual indicators of varied roles in a regional settlement system must be found. Of course, there is also still considerable need for studies that help to pinpoint the dates of the sites in question; separating dating from function (Figure 52) remains a major problem, as discussed earlier.

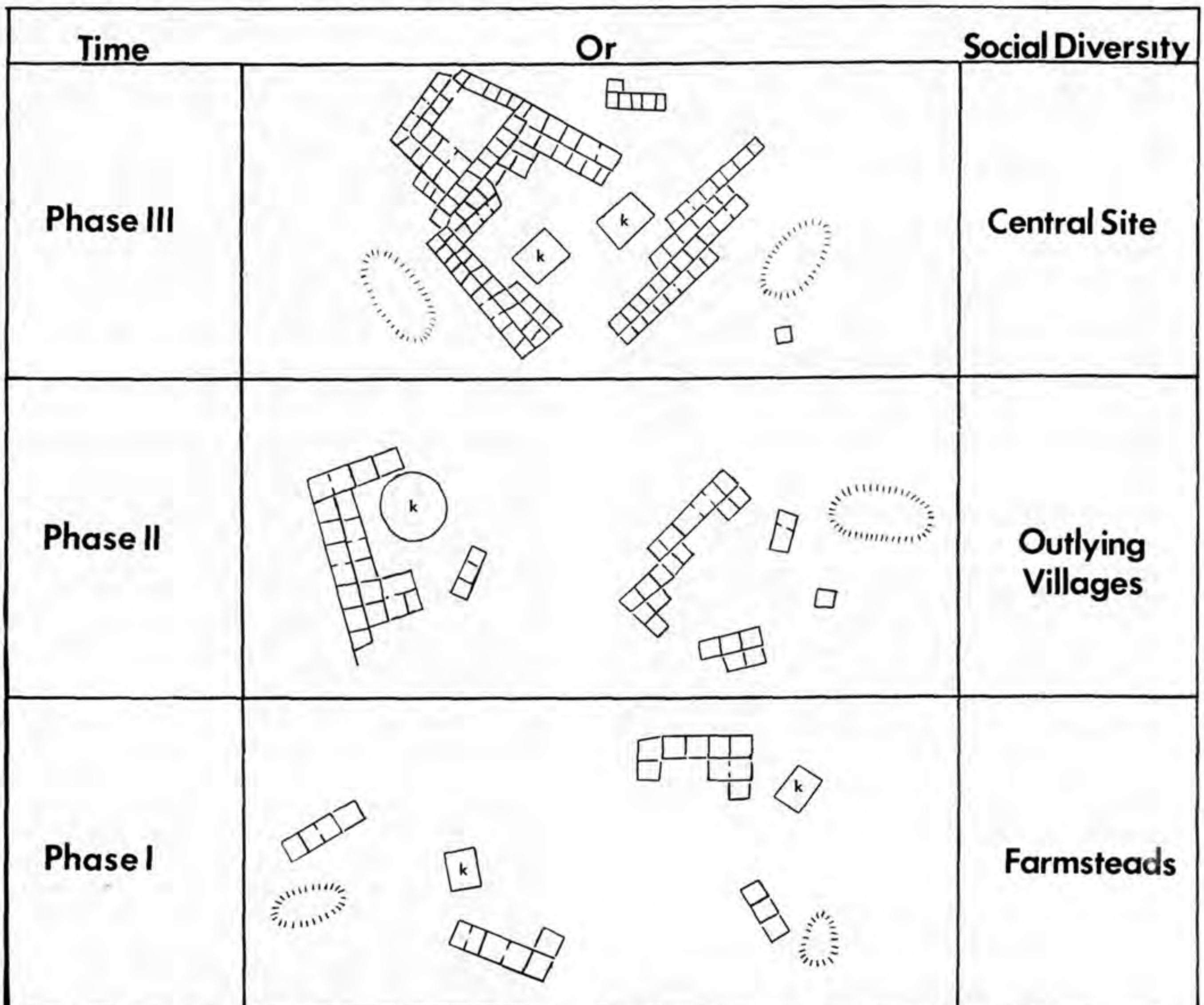

Figure 52. Differences in site size and layout could reflect temporal variation or a contemporaneous organizational pattern.

ARCHEOLOGY OF THE LITTLE COLORADO REGION

INTRODUCTION

The intellectual history of efforts to understand the systematics of regional prehistory for areas as large as that described in this overview is relatively brief. Summaries of regional prehistory have been common for decades, as have efforts to generate typologies of cultural patterning. But, it is only in the relatively recent past that archeologists have begun to explore methodologies for generalizing about large regions rather than assuming that such generalizations would readily grow from the results of excavation and more localized survey.

While I grant that there is room for disagreement, Willey's Viru Valley survey (1953) and Ruppé and Dittert's work in the Acoma Province (Ruppé 1966) represent the first characterizations of regional prehistory on the basis of field investigations specifically attuned to describing and explaining regional phenomena. Binford's discussions of research designs (1964, 1965) further elucidated the extent to which meaningful understanding of regional phenomena was unlikely to come from other than well focused regional research designs and field work.

The rapid growth in efforts to do regional archeology has been stimulated as much by the growth of contract archeology, especially the need for overviews such as this one, as by the growth of an intellectual tradition. The rapidity of that growth is perhaps best indicated by the existence of parallel regional literatures in archeology today, the citation patterns of each forming almost disjunct sets. For example, the regional analyses undertaken by members of SARG (Gumerman 1971; Euler and Gumerman 1978) draw little from the concepts and theories discussed by Johnson (1977) in his characterization of regional analyses and vice versa. Both of these differ markedly from Parson's (1975) summary of "settlement pattern" studies. While such provincialism is expectable in circumstances where a new research domain is being explored, there is much to be gained by exploring the manner in which efforts of different schools of regional archeology articulate.

Of equal importance is exploration of the manner in which strictly research oriented explorations of regions and management based studies can articulate. Surely, it is possible to distinguish between the concerns of managers and those of researchers. But, if management decisions are not informed by the best research strategies available, then it is unlikely that such decisions will be of the quality that we all desire. While it is easy to assume that managers and pure researchers want to understand different aspects of the prehistoric record, unless this assumption is documented, the true referent of the term "management information" is second rate data and inferences.

For these reasons, I attempt in the following pages to characterize regional archeology and its articulation with planning. First, I will consider the question of transformation processes and how these are to be understood at the regional level. Subsequently, the nature of inferences concerning spatial and temporal variation in regional phenomena are considered. Finally, I turn to the question of planning and how regional plans are informed by the approaches that have been defined.

SITE FORMATION PROCESSES

A first set of theories necessary to interpreting regional prehistory concerns transformation processes. In a series of articles Schiffer has described the transformation processes that form, transform, and reform the archeological record at particular sites (Schiffer 1975, 1976, 1978; Schiffer and Rathje 1973; Reid, Schiffer and Neff 1975). There is no point in repeating the details of these discussions. My major concern is the extent to which attention to the site specific processes described by Schiffer will suffice for a consideration of regional archeology and the extent to which an inter-site focus and attendant field strategies require an elaboration of the work that Schiffer has done. Are there transformation processes that effect the regional record in ways other than their manifestation at specific sites? Does the fact that the regional record is known largely through surface collection generate problems in the understanding of transformation processes that shape the record? The answer to both questions is "yes," and it necessitates a consideration of pertinent processes.

NATURAL TRANSFORMATION PROCESSES

Environmental Change

When site records are generated by survey, archeologists commonly make observations of the natural context in which the sites are found. Yet, because environmental change occurs, it is impossible to assume that the archeological context in which sites are found is identical to the systemic context in which they were utilized. At the same time, it is unreasonable to assume either that the archeological and systemic contexts differ or that the degree of difference is the same for all environmental variables.

While the archeological and systemic contexts of sites may be different, they are not necessarily so. In the early days of Southwestern pollen studies, many archeologists assumed that sharp contrasts between modern and at least some prehistoric conditions would be revealed. What is remarkable about the last decades' results is the limited evidence of change that has been documented. Certainly the environments of PaleoIndian and Archaic sites differed drastically from those of the present, as described in an earlier section. But, for later prehistoric sites, there is not evidence for drastic differences. That change occurred is clear; that the resulting environmental variation lies beyond the limits of modern variation is not.

Similarly, it would be a mistake to assume that the magnitude of change was the same for all relevant environmental variables. Precipitation and temperature conditions are the most likely to have changed. Vegetation patterns may or may not have changed. The character of drainages in an area is likely to have been similar at some points in the past and different at others. Major topographic features, in the absence of recent vulcanism, are relatively unlikely to have changed, although some topographic features, e.g., dunes, are more likely to have changed than others, e.g., basalt capped mesas. Certainly, a careful consideration is warranted of the probable magnitude of similarities and differences between modern environments and those that formed the systemic context of site systems at various points in the past. But, the analysis should never presume differences of great magnitude.

Our current understanding of prehistoric environmental variation in the study area was reviewed earlier. While appropriate data for understanding selected aspects of prehistoric environmental variation are available, they are woefully inadequate for meaningful regional generalizations in regard to the issues addressed above. These inadequacies stem from research with both prehistoric and modern focuses.

A primary problem in respect to modern records in the area is simply their limited extent. There are few weather recording stations relative to what would be desirable and even fewer detailed floral and faunal studies. The weaknesses of the modern baseline create immediate problems for generalizations concerning the prehistoric past. To the extent that planning activities carried out by Federal and State agencies generate more detailed environmental records, it is essential that these be made immediately available to archeologists working in the area.

Even with more complete modern records, there would be significant problems in prehistoric reconstructions. Pollen analyses are a case in point. The literature abounds with statements concerning the diversity of factors that affect the abundance of pollen of particular types recovered at a given locus (e.g., wind, humidity, and soil chemistry). But, as yet, no study has been undertaken that attempts to control for pertinent variables by analyzing samples collected from the modern surface (or appropriate pollen traps) on given days with known conditions. Until such studies are undertaken, the element of guesswork involved in the collection of prehistoric samples will remain so high as to render their results at least subject to substantial doubt.

Apart from this interpretive problem, there is the issue of the quantity and quality of specimens that it would be desirable to have from any given excavation locus. To the best of my knowledge, there is at present no study that investigates variation among samples taken from a single small living surface. While there is certainly a perspective from which one can regard a 200 grain count on a single slide as 200 observations, it would be useful to know how much variation among slides exists. With this information, economy and efficiency in the collection of pollen samples could be balanced.

A final problem is the simple absence of an adequate data base from which generalizations might be made. Even in recent years, no pollen analyses have been undertaken from the majority of excavated sites in the area. Archeologists will be in a poor position to realize any of the potential of pollen analyses until such samples are routinely taken and analyzed. In this regard, the cost of anlysis is typically prohibitive. A regional center for pollen analysis that functions much as the Laboratory of Tree-Ring Research would clearly be desirable. At the same time, research designs generated by palynologists that sample the region in both cultural and noncultural prehistoric contexts is essential if our understanding of prehistoric environmental variation is to proceed at more than a snail-pace. The days when palynologists could afford to serve as the archeologists' handmaiden are long gone. Truly regional generalizations require regional research design and data collection, an effort that surely belongs in the hands of palynologists.

Control of tree-ring variation by dendroclimatologists is far more substantial and sophisticated than that of palynologists. The primary problem with our current ability to use this data base for regional generalizations is a dearth of recording stations. While it is again possible to envision modification of this situation through the gradual accumulation of data from archeological sites, an immediate effort to generate additional stations through a second "beam expedition" (the effort made in the 1930s to gather materials needed to complete the tree-ring plot) would greatly improve our control of climatic variation data in the area. As earlier discussions make obvious, the three recording stations now available in the area do not provide an adequate basis for regional generalizations. Again, research design and data collection specifically attuned to regional description and interpretation seem warranted.

Other analytical traditions are so poorly developed at present that one can only suggest the need for their initiation. Packrat nests have, to the best of my knowledge, never been studied in the inventory area. While some localities within the overview unit lack appropriate topography, much of it does not. Cliffs and talus, for example, bound most of the Upper Little Colorado drainage as well as the majority of its southern tributaries. Detailed analyses of paleosols are also absent in the area. Even routine flotation of deposits from archeological sites to obtain simple lists of available resources generally have not been done. Overall, there is a need for increased sensitivity to our currently poor understanding of environmental variation.

Deposition

Deposition is likely to have a major impact on our understanding of the regional record in most areas. The existence of deposition is not the major source of the problem; even when it is heavy, sites can be located if appropriate survey techniques are employed. The problem is the differential effect of deposition, spatially, temporally, and functionally.

Temporally, the problem is straightforward: all other things equal, older sites are more likely to be buried than younger ones. Spatially, some topographic environments are characterized by higher rates of deposition than others. Deposition is more likely to have obscured elements of the archeological record in broad alluvial valleys than on flat mesa tops. Finally, there are functional problems: small and ephemeral loci, and those reflecting activities associated with depositionally active locations, are more likely to be obscured by deposition than larger and more permanent loci. Because prehistoric peoples carried out different activities in loci with different depositional conditions, it is necessary to consider the possibility that prehistoric activities associated with particular resource zones or time periods have been differentially obscured by deposition.

Erosion

The role of erosion directly parallels that of deposition. Because they have been exposed to erosional agents for longer periods of time, older sites are more likely to have been removed and redeposited than younger ones. Sites that were originally characterized by few materials are more likely to have been erased than larger ones. Sites in topographic environments that are erosionally active are more likely to have been removed than those in zones of less activity.

Differential Erosion/Deposition

The interaction of erosion and deposition create a still more complex set of possibilities since one can imagine environments where one but not the other, both, or neither were active during the relevant time interval. The greater the complexity of the interaction between the two processes within a study area, the greater the probability that some elements of the archeological record have been preserved differentially.

An initial problem in interpreting the effects of erosion/deposition on the prehistoric record in the study area is, again, the dearth of modern data. Hydrological records are <u>relatively</u> rich. While they do not permit a detailed mapping of stream flow within the area, they are sufficient to indicate substantial variation. It is unlikely that the desirability of further studies for archeological purposes is sufficient to justify the expense involved.

Soil and geomorphological studies are another matter entirely. The available data are weak and inconsistent. The mapping of extent of erosion, for example, is scarcely more than what one could do by drawing isomorphs on the assumption that erosion is heaviest near major drainages and least in the uplands, with a gradient between. Regional sampling to more specifically identify erosional difficulties is likely to be generated in the course of land use studies. The immediate availability of such information to archeologists is highly desirable.

At the same time, archeologists' geomorphological expertise is generally weak and we have rarely requested funds for appropriate research at the level necessary to create localized understanding of cut and fill sequences. In this instance, localized studies are clearly warranted. The hydrological data alone are sufficient to suggest the improbability of identical sequences from drainage to drainage. The whole history of Quaternary and Archaic research in the area is a record of the destruction of what were presumed to be valid pan-regional sequences: most depositional and erosional events are present in some areas but not others and occur at different times and magnitudes even where they do occur (Ackerly, personal communication). When working with Federal and State agencies, it is appropriate for archeologists to request the necessary assistance from the agency in question. The need for developing appropriate expertise on nonagency funded projects seems clear.

Catastrophes

While catastrophes should not be used to explain the evolution of prehistoric groups, the potential effects on the record of, for example, major floods or volcanic eruptions cannot be ignored. These are capable of obliterating evidence of prehistoric occupation over large areas.

Within the study area, the effects of three such phenomena require additional study. First, no meaningful research has been conducted in respect to vulcanism. It is generally understood that vulcanism may have been contemporaneous with the earliest occupation of the area, but the potential magnitude of this problem is not understood. Second, given the boom-bust pattern of stream flow in the area, the occasional prehistoric occurrence of floods that removed substantial portions of the archeological record in at least some drainages seems likely. Finally, and admittedly of somewhat minimal concern, is the possibility that high altitude glaciation obscured some aspects of the early prehistoric record in the region.

CULTURAL TRANSFORMATION PROCESSES

S-A Processes

Schiffer uses the concept "S-A process" to refer to those processes by which artifacts and sites move from a systemic to an archeological context: primary and secondary discard, abandonment, loss, and burial (Schiffer uses the term "disposal of the dead." I prefer the term "burial" since objects other than human bodies, including whole sites, can be purposefully buried and since the dead can be disposed of by what amounts to discard). These same processes operate to form the regional archeological record. However, a major problem exists in regard to differences in their relative effects at different loci.

First, when the prehistory of a region is approached through surface collection and the generation of site records, it is extremely difficult even to identify the specific processes that led to the artifacts presence on the site surface. While S-A processes can be difficult to identify or control for when sites are excavated, there are at least some contextual bases for attempting their identification. But, materials removed from the site surface often lack such context. It is sometimes, but not always, possible to differentiate a deep midden from a thin surface scatter. While artifacts collected from within the boundaries of a room were not necessarily used in that room, they may have been used there. While a thin scatter of artifacts on a depositionally and erosionally stable surface may represent sheet trash, they may also approximate the distribution of materials left by the inhabitants of a camp site closely enough to allow behavioral inferences (see Wait 1976). If collections made from some sites are largely from areas of primary refuse, and closely reflect a discrete set of activities carried out there, while collections made at another site are largely from areas where it is common to find secondary refuse which reflects no particular set of activities, the potential for making incorrect inferences concerning the activities carried out at the two sites is very great.

Such problems become even more extreme when both relatively discrete, high density artifact scatters and amorphous, low density scatters exist within a single study area, or when low density scatters are the only observable cultural loci. In the first instance, the relation of the latter to the former is extremely problematical since the latter could represent the movement of artifacts from high density sites by natural or cultural processes. It also could represent a discrete activity pattern. On the basis of surface evidence, resolution of this issue is close to impossible.

In the second case, the nature of the S-A processes that formed the site are even more difficult to infer than with data from high density sites since the context of the materials is even less clear. While a greater than expected occurrence of artifacts in some specific plant community, for example, could be produced by primary discard and indicate extensive use of that community, it could also reflect the centrality of the community alone--more people walked through it more times during a particular annual round and lost or discarded more artifacts. This problem has not been adequately addressed by proponents of "non-site" archeology.

A final problem involving S-A processes is burial. Simply put, aspects of the archeological record generated by purposeful burial are extremely unlikely to be known on the basis of surface survey or surface collection.

Several lines of investigation would greatly improve our understanding of the effects of these processes on the archeological record. First, there is a significant need for investigations of the relationship between surface and subsurface deposits. None exists at present. This problem is ultimately a relatively easy one to resolve. Presumably, most modern excavations in the area distinguish between surface and subsurface deposits. Thus, it is likely that existing records from a variety of sites in the area would be sufficient for highly detailed analyses of surface-subsurface relations if sufficient funds were available for such a study.

Second, there is a very great need for studies of the accuracy and precision of alternate collecting techniques. Studies of survey techniques are now available and can serve as an excellent model for studies of collection techniques. Unfortunately, appropriate data are not currently available. In order to conduct the appropriate research, one would need a fairly large number of sites that have had 100% surface collections. A variety of different sampling strategies ranging from grab to statistical samples could be simulated using these data. Existing studies of survey strategies (cf. Stafford et al., 1978) have demonstrated that, given a sufficiently large sample size, great latitude can be exercised in designing sample surveys. The applicability of their conclusions to collections from site surfaces could be quickly evaluated were the necessary site data obtained.

A third major problem concerns low density artifact scatters. For the reasons discussed above, the interpretation of these archeological manifestations is at best ambiguous. Moreover, they can present a major management problem. Collections and analyses of materials from a number of such scatters in different locations within the study area could quickly resolve the issue of their interpretability.

Finally, the entire set of issues concerning the processes that generate archeological sites is poorly understood within the study area. The only exception is the work of Schiffer and his associates in Hay Hollow Valley. His various writings provide excellent guidance for conducting studies of the effect of transformation processes in generating the patterns observable at particular sites. Those writings should be consulted for the design of appropriate research.

A-S Processes

A-S processes are those that move artifacts from the archeological context to that of the modern system: collecting; pothunting; and excavation. Collecting and excavation are generally documented, although unpublished surveys and excavations do create problems. Pothunting and collecting by amateurs can have a major, capricious

Figure 53. In the Pinedale area, walls exposed in potholes are often the only evidence of Pueblo architecture.

(Figure 53), elusive effect on the regional record.

Sites to which the public has easy access are more likely to have been effected by such activities than sites to which access is difficult. Large and obvious sites are more likely to have been impacted than smaller and more obscure ones. Finally, the kinds of artifacts that are removed from site surfaces may be quite specific. Metates and other large objects are more likely to have been removed from frequently visited sites than from low access sites. Decorated pottery and formally made tools, such as projectile points, are more likely to have been removed than undecorated pottery and debitage or casual tools. Thus, the kinds and frequencies of artifacts found at sites can be greatly affected by the differential removal of materials from them.

Very, very little is known of the impacts of such processes on the record of the area. I early noted that there is a distinct possibility that the impact of pothunting in the distant past was far more substantial than is evident from the inspection of site surfaces today. Two lines of research would greatly improve our understanding of this impact. First, it is desirable that an ethnography of pothunters be written as soon as possible. There are still individuals alive who observed, participated in, or at least heard of, the destruction that was occurring in the area at the turn of the century. These same individuals are a source of information on pothunting activities prior to the start of a major archeological presence in the area, which only began in 1960. These people should be able to provide at least rough information concerning the areas and sites where pothunting was the most intense. Admittedly, there will be difficulty in obtaining information from those who are still engaged in the activity. However, especially given the provisions of the Archaeological Resources Protection Act, Section 11, there should be relatively little difficulty in compiling a substantial body of information. This information could then be field checked to identify the presence or absence of modern indications of the activity. In addition, site records could be thoroughly reviewed and archeologists interviewed in an effort to identify the excavated evidence, reported or unreported, of the magnitude of disturbance.

A second line of investigation should focus on the modern effects of pothunting and collecting. A preliminary effort in this direction is reflected in the works of Lightfoot and Francis (Lightfoot and Francis 1978; Francis 1978; Lightfoot 1978). They attempted to determine the extent to which access via roads increases pothunting and the selective removal of particular artifact categories from sites. While their results are of a preliminary nature, the possibility of assigning degrees and zones of impact to particular modern activities that increase access or human activity in particular localities is clearly indicated. There is no reason why their analysis cannot be extended to include the full range of activities carried out by Federal and State agencies in the area. Plog (1981) has attempted to estimate the overall impacts of both land disturbance and pothunting on the Apache-Sitgreaves National Forests. Again, this information is critical for wise management planning and for understanding what components of regional prehistory are no longer available for investigation or have suffered qualitatively relative to others.

A-A Processes

A-A processes are those that move cultural materials within the archeological context: later occupation; land-levelling; and channelization. Two major problems in interpreting the prehistoric record arise from the consideration of these processes. The first is later occupation. When sites are known principally through surface collections, earlier components may be variably obscured by later deposits. Also, it may be impossible to differentiate sites with lengthy occupations spans from sites with a large number of episodic occupations. The first problem is illustrated in recent work by Arizona State University at Chavez Pass Ruin. A number of previous investigators (e.g., Wilson 1969) argued for the sequential occupation of the three major room blocks at this site. Our own surface collections supported these earlier conclusions. Once test excavations were undertaken, however, a quite different pattern was apparent. The occupations of the three room blocks were late and largely contemporaneous. The three areas differed in the extent of earlier occupation and/or the extent to which earlier deposits were buried by later ones.

The second problem is equally evident if characterized in the context of seriation analysis. The relative percentages of materials from different time periods are likely to be the same whether a site was occupied throughout each of a series of time periods or only for short episodes during each. There is the further problem of early and late episodic occupations being obscured by lengthy occupation during some intervening period.

Levelling the land and channelization have regional impacts since these activities are non-randomly distributed in relation to environmental variables that may have been important to prehistoric peoples. Juniper pushing, for example, can easily obliterate much of the archeological record in a woodland while leaving that in nearby grasslands and pine parklands intact. Similarly, channelization is most likely to have occurred and destroyed sites in the vicinity of major drainages. Thus, select elements of the regional record can be removed while others are left intact.

Archeological studies of these impacts are, again, few in number. Major impacts that may be envisioned in the overview unit and elements of these that require further discussion follow.

<u>General Impacts</u>

There is no question that the greatest single source of potential impact on cultural resources is a simple lack of awareness of those resources. While there is no way of documenting this argument, one must seriously question whether specific land modifying activities have had as much impact on cultural resources as that created by failure to be aware of the need to protect them. The casual destruction of sites and the casual removal of artifacts from site surfaces by agency personnel, contractors, and the general public may have had a greater effect on the quality of existing resources than the aggregate of land modification activities that have occurred there. Having raised this particular issue as a general one, it will not be addressed in the succeeding section. Means of increasing employee, contractor, and public awareness are discussed in a later section.

Timber Harvest Impacts

The greatest impact arising from timber harvesting is a result of the timber harvest itself. The movement of heavy equipment across the ground surface and the skidding of trees are the major direct impact. The construction of haul roads and landings is a second source of impact. These impacts occur to cultural resources, both those with and without surface manifestations.

The first impact is best resolved by prior survey of the area that is to be harvested and flagging cultural resource locations so that the movement of equipment through them can be avoided. As currently practiced, this approach has two negative side effects. First, it advertises the location of cultural resources to anyone passing through the area. Second, the flagging is frequently done so far in advance of the sale that many of the boundary markers have disappeared prior to the harvest. Technological means for resolving this problem potentially exist in the form of alternative site markers. Stores and libraries are beginning to use small chips placed in merchandise or books that amplify a transmitter signal. The use of such chips embedded in a site tag or a nail could be used to mark a site. The time required to return to a site and flag its boundaries immediately prior to harvesting in the area would be greatly reduced. Similarly, the flagging should be removed immediately after completion of the sale, with the tag remaining as a permanent indicator of the presence of a cultural resource.

The second impact is best resolved by actually surveying road and landing locations and realigning them, if necessary, to avoid sites. In this fashion, the costs and problems raised by conspicuous flagging can be avoided.

Given the energy crisis that we are currently experiencing, the cutting of fuelwood is likely to become far more of a problem than it has been in the past. Fuelwood cutting involves the movement of vehicles and trailers through an area. In addition, it increases the level of human activity in what have been relatively isolated areas. The potential for substantial additional impacts is great. At issue is the relative advisability of flagging sites to warn vehicle operators to avoid them (thereby drawing more attention to them) as opposed to simply ensuring that fuelwood cutters are aware of the potential existence of such sites and leave them alone when they are encountered.

Range Management Impacts

Three activities of range management potentially impact cultural resources: juniper clearance; fence construction; and the construction of stock tanks. The first of these impacts is potentially the most damaging. The movement of heavy equipment through an area and the disruption of subsurface deposits when large trees are removed are the sources of destruction. These impacts have been largely avoided in recent clearance activities by prior survey and flagging of cultural resources. While there is potential for the same problems with flagging that arise in timber harvesting, the time lag between the cultural resouces survey and the clearance can be greatly reduced. Again, the flagging should be removed after the activity has been completed.

An indirect impact of juniper clearance is that it increases the visibility of cultural resources. A few early efforts to protect cultural resources resulted in tree zones around them that virtually identified the existence of the resource. Vegetative screens are left to minimize the impact of clearance on the aesthetic qualities of an area as well as its quality as a wildlife habitat. Incorporating the cultural resources into these will also serve to protect the cultural resources there. Such devices would be useful for providing an inconspicuous indicator of site locations in any circumstance.

Fencing has both direct and indirect potential impacts. Survey in advance of actual construction is probably not warranted since the actual zone of disturbance is not great. However, at least one individual able to identify cultural resources should be a member of the construction crew. An indirect impact of fencing is the use of fences as a trail through the forest. To the extent that hunters and hikers use the fencelines they will be attracted to nearby archeological sites and casual collecting may result. Thus, when a fenceline is moved around a cultural resource it should be moved a sufficient distance so that the resource in question is not visible from the fenceline.

Because stock tanks are isolated points, minimizing their impact is relatively simple. As long as the site, and the means by which heavy equipment will be moved to the site, are inspected, direct impacts are easily avoided. The indirect impact resulting from the construction of a stock tank is the concentration of cattle in its vicinity. Site surfaces can be disturbed to a point where materials can no longer be analyzed when those surfaces are repeatedly trampled by livestock. Therefore, stock tanks should generally: (a) not be located in zones of exceptionally high site density; and (b) not be located in the immediate vicinity of an archeological site.

From these activities, a major secondary impact is derived--grazing by cattle. Evaluating the specific effects of grazing is only possible through specific studies of sites that have been impacted. On the one hand, it is clear that there can be impacts. The author participated in the excavation of one site that had been a stock pen. The sherds there were often so small as to defy analysis. But, this site represents an extreme situation.

Trampling along fence lines and in the vicinity of stock tanks have, to the best of my knowledge, never been evaluated. And a heavy degree of impact in these areas should not be assumed. Similarly, while it is clear that overgrazing can lead to erosion that in turn impacts cultural resouces, the magnitude of this problem has never been determined and remains a subject of great controversy.

Engineering Projects Impacts

Apart from their role in the activities just discussed, the major impact of engineering projects is the construction of roads. The direct impact of road construction is the disturbance of the ground surface. Careful survey of proposed roads prior to construction is, therefore, warranted. To the maximum extent feasible, actual flagging of sites should be avoided for the reasons discussed earlier. The major impact of roads is opening public access to areas where cultural resources are dense. The major impact that enhanced access has had on cultural resources is discussed elsewhere in the report. Roads are necessary and some of these impacts are unavoidable, but they can be ameliorated by: (1) avoiding road construction in areas of exceptionally high site density; and (2) either leaving vegetation that screens cultural resources or revegetating in a manner that screens the resource from traffic moving on the road.

Fire Suppression Impacts

The potential impact of fire suppression on cultural resources is substantial. Stories of fire crew members removing artifacts from sites and direct evidence of the destruction of cultural resources abound in the case of the Day Burn, one recent fire that occurred in an area of high cultural resouce density. Whenever possible, it is advisable to have one or more archeologists present during fire suppression to reduce the impact of the activity on cultural resources as much as feasible given the more immediate and pressing concerns. It is especially important that the sensitivity of temporary summer personnel to cultural resources be increased to prevent both casual and major destruction of cultural resouces.

Recreation and Land Exchange Impacts

The primary direct impact of recreational activities is the construction of camp sites. In general, these sites increase access to cultural resouces. The magnitude of the problem created by that access is difficult to estimate, but it may be substantial. Most of the rock shelters, for example, in the vicinity of the Chevelon Creek campground are virtually devoid of cultural materials as a result of illicit excavation. The limits of the impact area are essentially defined by the average distance that citizen-users range from the camp during their stay there and this datum is at present unknown. It should be assumed, however, that survey undertaken in conjunction with the development of a new camping area should not be restricted to the direct impacts of construction.

Land exchanges are another potential source of impact. Unfortunately, there is a high density of cultural resources in the vicinity of rapidly growing communities. Clearly, the relationship between the forests and those communities will deteriorate unless some allowance for their growth is made. Given that growth may occur in virtually any direction, planning for this eventuality should begin soon. Specific proposals are made in the discussion of the inventory of the forest's resources.

Mining Impacts

Mining is by far the most destructive single activity that threatens cultural resources. Given that most mineral raw materials are now becoming scarce, this activity is likely to increase; the rising cost of the raw material allows the exploitation of previously uneconomical sources and efforts to discover new sources using more expensive techniques. Both testing and actual mining can be highly destructive. The movement of heavy machinery to drill locations can destroy sites. Similarly, there are some testing procedures that result in heavy impacts to the land surface within several hundred meters of the drill site.

None of the processes discussed above are well understood. It is easy in envisioning a particular project, to imagine major impacts that later prove to be minimal and to fail to suspect major impacts. Similarly, corrective measures that initially seem desirable may in the long run draw attention to, and increase impacts on, sites. Only through careful documentation of efforts to avoid sites, and the subsequent occurrence or lack of impacts, will the necessary knowledge be obtained.

S-S Processes

These processes are ones that move cultural materials within a systemic context: recycling, secondary use, lateral recycling, and conservation. The negative effect of such processes on the prehistoric record is potentially great and difficult to evaluate. In essence, the question raised is the extent to which the first pothunters were in fact later prehistoric occupants of particular regions.

The problem is perhaps most evident in the case of projectile points. In some areas, early and late manufacturing technologies have been identified (cf. Plog 1974). Yet, most late sites, even the very latest ones, typically have points made using the early technology. Whether this pattern reflects the survival of the earlier technology or the systematic removal and reuse of earlier points from earlier sites is impossible to say. The removal of building stones from earlier sites for use in the construction of later ones has also been discussed.

While such behavior almost certainly occurred, it is easy to confuse a settlement that had only foundation stones rather than full standing walls with one from which stone was removed. The ultimate extent of recycling and reuse at the regional level will be difficult to define. But, it certainly must be considered rather than simply assuming that the materials found at a particular loci were made by the people who lived there or by their contemporaries.

Clearly, one can become so concerned with the potential role of such processes in obscuring behavioral patterns that behavioral analyses no longer seems fruitful. It is not correct to argue, as some students of transformation processes seem to, that one cannot do archeology without controlling for these processes. At the same time, one should never fail to control for intervening variables whenever possible. There is little doubt that some of what we currently perceive to be patterns of material culture left by prehistoric peoples in the overview area will prove to be the products of transformation processes. The sooner archeologists are able to undertake studies such as those mentioned throughout the preceding discussion, the better our interpretations will become.

DESCRIBING SPATIAL VARIATION

Assuming that transformation processes are controlled and described to a meaningful extent, the major focus of archeological analysis is the description, interpretation, and explanation of spatial and temporal patterns. Spatial patterns and their treatment are considered here and temporal patterns are considered in the following section. In both discussions, the need for truly regional approaches is presumed. That is, I do not assume that a genuine picture of regional patterns will emerge from the accumulation of site excavations and local surveys. Instead, my assumption is that generalizations about large spatial entities require research designs specifically attuned to that task, research designs that involve a dynamic interplay of local and specifically regional analyses. Spatial variation in artifactual distribution, settlement systems, and behavioral systems are independent foci of the following discussion.

Artifacts

There are two current problems that confound efforts to describe and explain artifactual distributions at the regional level: for some artifact classes our understanding is too limited and for others, too complex. Perhaps the best example of the first problem is stone technology. There is no typology for either chipped or ground stone that is shared by archeologists working either within the Southwest or within a particular region of it. Even efforts to establish terminological consistency between different classification systems are lacking.

At first glance, the problem of ever attaining the agreement necessary for such a lithic classification system seems insurmountable. It can, however, be easily overestimated. I would suggest that most of the lithic typologies that have been used presume that lithic technologies should ultimately attain the same degree of formality that is characteristic of ceramic systems. Recent studies challenge this assumption. In the cases of both chipped (Decker 1976) and ground (Mundie 1976) stone, statistical analyses of attributes have resulted in the definition of relatively few types. That is, of all the categories that archeologists have used in defining different types of scrapers and knives, few prove to have integrity when subjected to attribute analyses of a relatively large collection of artifacts. Certainly, the archeologists who created the earlier typologies recognized distinctive forms, but quantitative treatments show both a complex and a relatively continuous pattern of variation among the ideal types defined on the basis of especially distinctive forms.

The underlying problem seems to be that the behavior of prehistoric peoples, as they utilized chipped and ground stone tools, was far less structured than that involved in making and using ceramic vessels. What was originally a projectile point could be, and often was, refashioned as a knife or scraper. What began as a casually retouched piece of stone became a formal scraper through episodic use and retouch.

The possibilities are endless and existing evidence suggests that prehistoric peoples did not simply make and subsequently use stone tools. Instead, they used flakes eclectically in a process that involved many discrete episodes of manufacture, use, modification, and reuse in a sometimes exceedingly complex chain. Until more studies demonstrate what categories and/or attributes are useful in characterizing variation in lithic technology, it is unlikely that we will be able to understand the functional variation in activities that were undertaken at particular sites as they are reflected by variation in this class of artifacts. On the other hand, these same studies may identify a simple typology.

The movement of raw materials used in manufacturing stone tools is also poorly understood. When analyses have been undertaken (cf. Green 1975, 1978), considerable support has been generated for the proposition that some raw material classes were exchanged over a wide area. Unfortunately, there are so few such studies that the magnitude of exchange, not to mention the patterning of exchange relationships, is impossible to characterize.

Ceramics are an example of the problem of too complex classification. Simply put, archeologists have allowed themselves such latitude in defining types that it is now close to impossible to establish equivalencies within, not to mention across, regions. This situation is particularly unfortunate since a consistent application of the standards used by Colton in the early period of type definition would have resulted in a very different situation. His approach was directly modeled on the process of manufacturing a vessel. Technological distinctions were used to create wares, stylistic distinctions to create types within wares. Unfortunately, he and others, to a more substantial degree, began to use space-time rather than formal attributes in generating definitions.

We now face a situation in which, for example, the technological variation within the category "Cibola White Ware" can only be described as five different wares if the criteria that are used to separate Tusayan from Little Colorado White Wares are consistently used. Similarly, we have failed to resolve the question of whether a horizon style system, cross-cutting wares, can be defined for the northern Southwest. Wasley (1959) proposed such a system some years ago, but Breternitz's analysis of tree-ring associations appeared to contradict it (1966). (The reasons for using the term "appeared" will be discussed later.)

Dee Green (personal communication) observed some years ago that the temporal sequences of change in line width and line density on painted sherds was similar on the various National Forests within the Southwestern Region. As noted earlier, recent analyses of stylistic change within major domains (e.g., Kana'a-Black Mesa-Sosi; Puerco-Reserve-Tularosa), using materials from the Upper Little Colorado and Kayenta areas, document regular changes in line width that correspond roughly to the horizon styles defined by Wasley (Hantman and Lightfoot 1978; S. Plog and Hantman 1978; Hantman, et al., in press).

I suspect that there are indeed horizon styles with pan-regional integrity. I also suspect that we will gain much in our understanding of regional prehistory as we begin to see that there are, at virtually every time period, some areas of the northern Southwest that have a localized style rather than the one that then characterizes most of the area. In any case, regional prehistory will be far easier after the efficacy of a horizon style system has been resolved.

There is also a major unresolved issue in our understanding of ceramic exchange. Virtually every petrographic study of Southwestern ceramics has suggested relatively localized manufacture and relatively widespread exchange (see S. Plog 1977, 1980 for summaries). Even Colton's ware system monitors this variation relatively well. Coupled with the nearly absolute failure to find evidence of ceramic manufacture despite decades of excavation, the case for specialized production and widespread exchange, rather than village-level ceramic manufacture, is strong. Yet, existing literature continues to assume village-level manufacture. The implications for understanding regional prehistory are enormous. If the petrography is correct, an enormous volume of material items circulated in the study area in every calendar year.

For effective regional analysis, typological and distributional problems such as those described above must be overcome. Until they are, the interpretation of even our most basic artifactual evidence is in doubt. And, problems with artifactual interpretation create difficulties in virtually every other area of analysis. As noted earlier, it is impossible to discuss with any security the distribution of Pueblo I period sites when a clear argument associating this time period with a particular artifactual assemblage cannot be made.

I do not intend that any of the above be interpreted as a recommendation that monolithic typologies be created and that archeologists working in the overview unit all be forced to employ them. Nevertheless, some commonality in the manner in which artifacts are described is essential if the results of independent research efforts are ever to be comparable. The commonality that I envision would be sufficient to place artifactual materials with gross spatial and temporal units, at least allowing dating within a 100 year period and allowing at least initial confidence that particular artifacts were or were not made in a particular area. At the same time, an effort to generate such an approach would recognize that independent attributes of particular artifact classes vary for different reasons. The variation in some attributes is temporally sensitive, others spatially sensitive, and still others functional. Other detailed local analyses will tap the rich potential of such attribute analyses and it is dubious that sufficient detail can ever be contained in a workable regional scheme.

It is indicative of the current state of our understanding of these issues as they pertain to the overview unit that many of the citations used to illustrate specific points are studies undertaken within the area. A number of the distributional studies described in this document simply have no parallels at the present time elsewhere in the Southwest (the computer mappings of ceramic and chipped stone raw material distributions, for example). Nevertheless, it is precisely these studies that have led to perception of the clear need for a greater investment in typological studies. These are important not simply for interpretation of the prehistory of the area, but because effective management cannot occur without standardized and comparable treatments of artifacts in spatially disparate contexts. Specific studies that need to be undertaken were identified in earlier chapters. Here the discussion has focused on how the further interpretation of the materials in question might proceed.

Spatial Systems

One might argue that the logical next step in attempting to construct a regional approach is a consideration of sites and variation in site types. However, it seems, on the basis of recent literature, that the concept "site," is a highly problematical one, at both extremes. On the one hand, where one draws the boundary between sites and low density artifact scatters is an issue. In some parts of the Southwest, such low density scatters or "non-sites" are more typical than sites. On the other hand, not all sites are communities or even settlements. Multi-size communities have been described in a number of areas. Thus, I suggest that instead of building regions from sites, it is preferable to begin with an effort to understand the distribution of material remains irrespective of how those remains are agglomerated into entities that may be called sites. From this perspective, the major analytical effort is toward analyzing the pattern of the distribution of cultural points, (architectural or artifactual), however those points are defined.

Points can first be analyzed in respect to the environmental settings in which they occur. There are two pertinent methods for exploring this relationship. The first is site catchment analysis (Vita-Finzi and Higgs 1970). While this approach is useful in considering environmental relationships for a relatively few points, its application is somewhere between difficult and impossible when the number of sites under study is large. There would be large numbers of sites in the case of most, but certainly not all, regional studies. Nevertheless, it can be profitably applied to a sample.

The second approach is that developed by SARG (Gumerman 1971; Euler and Gumerman 1978). This more analytical approach seeks correlations with particular environmental variables. As recent descriptions of SARG efforts demonstrate, these variables can be treated in terms of local and regional patterns as well as individually. The ultimate goal of both site catchment analysis and the SARG approach is the same: to understand the manner in which prehistoric peoples used their environments and the manner in which the environment shaped their use of particular areas.

Locational or point pattern analyses of the relationship between a set of points have now been described in some detail and from a number of different perspectives (F. Plog 1974; Hodder and Orton 1976; Clarke 1977; Johnson 1977). Common to all of these approaches is the assumption that attention must be given to characteristics of a system of points rather than the unique characteristics of the points themselves, although the latter may very well be differentiated in a set of types or categories. Characteristics of the distribution are then measured: density, evenness, agglomeration, differentiation, integration, hierarchy, and symmetry. There are appropriate quantitative techniques for each. Each measure is implicitly systemic because the value or condition that a particular variable takes is a product of the relationships among the entire set of points. Properly used, such analyses define localities or subsystems within regions and identify their distinctive properties.

It is undeniably difficult to escape familiar paleoethnographic handles such as butchering camp, field house, town, etc., and the equally familiar notion of an ideographic version of the cultural landscape, a settlement pattern. Nevertheless, the time has come to recognize the dubious empirical content of such terms for many regional situations. Simply put, too many different usages of a given locus are possible to allow the conclusion that such analogic characterizations are providing real information. Similarly, eyeball analyses of distributional maps leave too much room for observer bias and error to permit confidence in their results. A more structural approach, where structure is defined over largely quantitative elements of the points under analysis, is the more likely source of understanding of settlement and spatial systems.

The studies that will lead to further understanding in this area were discussed in earlier chapters on intra- and inter-site settlement patterns. Comments made here are primarily intended to clarify the broad implications of such efforts. While archeologists will never completely agree on acceptable site typologies nor on appropriate locational statistics, some agreement is essential if the cultural resources of the area are to be managed with any overall design in mind.

Behavioral and Organizational Systems[1]

In 1968, and in reaction to the efforts of new archeologists to do paleoethnography, Marvin Harris warned against the use of familiar ethnographic terms in describing prehistoric behavior and organization. Whether at the inter-site level (tribes, bands, etc) or intra-site level (residence groups, etc.) such terms are highly problematical in the ethnological and ethnographic literature and likely to be more so in the past. More recently, Leaf (1973), Quinn (1975) and others have cogently argued that the social behavior and categories that have been described as rules, norms, and even groups are best treated as elements in complex decision structures, not real behavior.

Avoiding mishandling of archeological data requires changes both in the way in which we employ the theoretical literature of sociocultural anthropology in formulating interpretations and the way in which we employ ethnographic data. The major problem that typically arises in using the theoretical literature occurs when typologies are employed. Most archeological discussions of social organization, for example, are based on either Service's (1962) or Fried's (1967) typology.

The use of these typologies to interpret the prehistoric past necessarily involves four major problems. First, when some key attribute is used as a basis for classifying a particular site or region in terms of a typology, the nature of explanatory arguments that can be explored is either sharply truncated or becomes hopelessly circular. If, for example, the size of the largest settlement is used to define state organization, it is impossible to construct arguments relating state organization and population aggregation; such an argument would be tautological since population aggregation was used to define state organization.

The second and third problems stem from the typologies themselves, from the very fact that they are based on ideal types. As Fried has observed, the advantage of ideal types is that they isolate key aspects of variation and key patterns of covariation by treating complex continua as simpler categories. I do not doubt the importance of such simplification in the search for structural regularities. However, there are two difficulties created in the study of prehistory. On the one hand, when one studies evolution, it is precisely the complex patterns of continual variation that are crucial to understanding why the patterns identified by categories of ideal types are the most typical outcomes of evolutionary processes. One must be able to explore the range of variation to understand why there are relatively few typical outcomes. On the other hand, if we are to take seriously the claim that one strength of archeology is the ability to identify behavioral and organizational patterns not found in the ethnographic and ethnohistoric records, then we must employ conceptual strategies that allow the possibility of patterns of variation and covariation not found in the present. Thus, in both respects, a more continuous approach to the archeological record is desirable.

A fourth problem arises when summary categories are injudiciously used by archeologists, ethnographers, or both. I do not question the utility for some discussions, arguments, and syntheses of applying terms such as band and tribe, egalitarian and stratified. But, there are limits to the kinds of analyses in which the use of such terms is appropriate. Because these terms mask substantial variation in particular institutional, status, and power patterns,

1. The following discussion was prepared in collaboration with Steadman Upham.

their utility for detailed comparative and evolutionary studies is questionable. Perhaps more important is the danger of too quick a summary judgment and subsequent failure to reevaluate evidence of variation over space and time.

Virtually every typology of social forms presents similar problems. I have previously noted (Plog 1977) the difficulty in separating reciprocity, redistribution, and marketing on behavioral criteria alone, not to mention artifactual patterns. And, it is unlikely that most of the groups occupying the overview unit after about AD 300 were easily classifiable as hunter-gatherers or agriculturalists. The probability is quite high that the same cultural and biological group drastically shifted its subsistence procurement behavior over time. There is also the probability that nearby groups practiced different strategies.

A second problem arises when summary descriptions of behavior and organization are borrowed from the ethnographic literature. This has typically occurred in the case of the overview unit in the abuse of Puebloan ethnography.

For example, by virtually every summary classification, the modern Pueblo are tribal and egalitarian. As a result, analysts of Puebloan prehistory typically assume these patterns. Yet, there is more than suggestive evidence of very non-egalitarian organizations and behaviors among the Pueblo. Brandt (1976) has contrasted the "New" and "Old" People at Taos:

> These groups represent emergent social classes. The New People are poor ceremonially and tend to be poor in other respects as well. They are disenfranchised and lack kin support which would enable them to obtain favorable grazing permits and access to land. They are unable to hold political office and rarely have friends in high places . . . . The lulina (Old People) are leaders and produce leaders. They allocate land, water and permits (1976:11).

The Old People, who number 50 out of a population of 1200, hold all of the political and ceremonial offices in the Pueblo. Membership in the group is inherited patrilineally. A similar situation has been described at Acoma Pueblo where the Antelope Clan holds all political and ceremonial offices and is far wealthier than any other clan (Ruppé, personal communication). In point of fact, substantial variation in wealth and power between individuals and groups has been described for most Pueblos, not to mention craft specialists and "caciques." And, this information exists despite the consummate skill with which the Pueblo are able to shelter intra-village reality from the outside world (Brandt 1976).

Of course such evidence is generally dismissed as a product of Pueblo participation in modern economic and political systems. And yet, there are both ethnohistoric and prehistoric data suggesting that stratification and political complexity have existed for some time in the Pueblo area. For example, high status burials, elite residential complexes, craft specialization, and status restricted material goods have now been documented prehistorically. Similarly, the records of the entrada indicate that the chiefs of particular settlements were capable of assembling large quantities of goods. Espejo, for example, is said to have been given 4000 cotton mantas by the cacique of Awatobi (Hammond and Rey 1966). Even granting overestimation and misrepresentation by the Spanish recorder, the production and distribution of so sizeable a quantity of one craft good is inconsistent with the argument that the Pueblo were egalitarian.

The point of this discussion is not to argue that the Pueblo are, in fact, a stratified society. Rather, the claim is that there are elements of both egalitarian and quite highly stratified organizations and behaviors in Pueblo culture that have considerable time depth. At various times and places, particular Pueblo settlements were almost certainly characterized by quite complex political and social organization. Either our commitment to an Apollonian view of the Pueblo, or the necessity of identifying unbroken Puebloan traditions that grew out of the land claims cases, has interfered with anthropological judgment.

Perhaps it is also too heavy a reliance on summary categories that necessarily obscure some variation that has prevented us from seeing that evidence of complexity and stratification among the Pueblo cannot simply be dismissed as a product of white contact. Again, these same problems arise when a "direct historical" argument is used as a basis for preferring Puebloan, or a particular subset of Puebloan, ethnography over alternatives. It is likely that only through employing the full range of southwestern ethnographic literature will we formulate interpretive models that are sufficient to describe the range of variation that actually occurred in prehistoric times.

Recognizing such problems, Cordell and I (1979) have recently argued that our most meaningful investment in attempting to describe the past will come from the study of strategies, coping behavior. Specifically, we have argued that demographic, productive, and organizational strategies are likely to be the best bases for understanding prehistoric organizational variability. In a regional context, such an approach begins with the assumption that the inhabitants of different sites and localities were, in all probability, involved in quite different strategies. To the extent that these strategies are harnessed in sub-regional or pan-regional systems, it is through interaction, alliance and exchange. Thus, if efforts to describe and explain artifactual and settlement distributions are to achieve fruition, it will not happen through the assumption of regional homogeneity. Rather, we require careful investigation of local similarities and differences and how they are integrated through casual interaction and/or hierarchically and non-hierarchically arranged alliance and exchange systems.

Such an effort must begin with more detailed discussions of variation in particular strategies across the overview unit. At present such a description is impossible, but suggestive data do exist. For example, the earlier discussion of demographic variation focused on two areas, Hay Hollow Valley and the Purcell-Larson locality, for which acceptable demographic studies have been done. It is clear that the demographic trajectories for these two areas were not the same. Whether different trajectories will be identified for other areas is unknown. Similarly, evidence of craft specialization in some sites was reviewed. How widespread and varied this phenomenon may be is also unknown. The stellar architectural diversity of the overview unit was described, although even a beginning interpretation of this diversity is missing at present. Given even these preliminary data, it is clear that there is potentially enormous variation over the overview unit in the particular demographic, productive and organizational strategies that were employed. Much careful analysis and description is necessary though, before we will be able to describe spatial variation in such strategies.

But, detailed local descriptions are ultimately unsatisfactory. The question of large organizational entities must ultimately be addressed: were there times when the overview unit, or most of it, functioned as a single interactive, or even political, entity? Answering such a question again involves the dynamic interplay of local and regional analyses.

At the local level, the analyses of interaction and multi-site communities discussed earlier is needed. These provide a means of working from the bottom up toward an understanding of larger interactive and organizational units. Point pattern analyses are also useful in that, applied to larger bodies of survey data, they may identify clusters and verify the reality of the apparently variable density that one may divine from existing records. Distributions of distinctive artifact types and styles provide a means of working from the top down.

I have argued elsewhere that the concept that most closely approximates the notion of region is what Ruppé and Dittert have called a "province." Provinces are defined by a distinctive system of material culture that is assumed to result from interaction. However, both behavioral and organizational variation are assumed to have occurred within the province. I find this concept appealing for both theoretical and empircal reasons. Theoretically, one ought to be able to define such spatial entities. Recent discussions of mate and material exchange (e.g., Sanders 1975; Wobst 1977; S. Plog 1977) suggest that, controlling for population densities, there are upper and lower limits to the expected spatial extent of interaction and exchange. Empirically, the size and shape of provinces, such as Zuni and Acoma, that have been described correspond to such expectations. They are roughly hexagonal and about 15,000 square kilometers in area. Similarly, even given the vague characterization of actual boundaries, maps showing the location of cultural branches appear to correspond with expectations for province locations and boundaries, at least for the Anasazi area during the period AD 900 to 1400 (F. Plog 1980).

The strategy I envision for characterizing such provinces does not involve generating definitions of average or typical characteristics of one province for comparison with others. Rather, I would examine the way in which the province is built through interaction and exchange between local groups that were, in all probability, culturally and organizationally distinct. As Cordell and I (1979) have recently argued, a major source of our misunderstanding of the prehistory of the northern Southwest is a result of preoccupation with facile descriptions of what is typical at most times and places. Understanding organizational and evolutionary dynamics necessitates a concern with the manner in which local groups that were distinctive in their demographic, exploitative, organizational, and symbolic patterns came to be articulated into systems of diverse sorts. These systems are sometimes intraregional, sometimes regional, and sometimes pan-regional.

Admittedly, we are not always in a position to begin a particular study with a well defined province. Naturally defined entities (a drainage, or a foothill zone) or one defined for managerial reasons are more typical starting points. Nevertheless, a crucial initial question is whether the entity contained single or multiple cultural entities at different points in time. Such descriptions are, however, ultimately of composition or behavior rather than of structure. Structural descriptions necessitate a more careful treatment of political organization, which itself proves a domain for archeological analyses.

Fried defines political organization as,

> . . . those portions of social organization that specifically relate to the individuals and groups that manage the affairs of public policy or seek to control the appointment or action of those individuals or groups (1967:20).

At a general level, Fried's definition is acceptable. For archeological purposes, the emphasis must be placed on evidence of the manner in which affairs were managed since access to information on specific managerial individuals and groups is difficult without written records. Thus, archeological data are not best analyzed in terms of the status and role related concepts of egalitarian, rank, and stratified societies that Fried has developed for ethnographic groups.

Although status and role are pertinent, the archeological record, to the extent that it reflects political organization, is the product of managerial decisions broadly conceived. In any society, the most basic such decisions concern: (1) access to space; (2) access to human and natural resources; (3) access to social statuses and organization; and (4) access to social symbols. Across societies, access in each of these areas varies in: (1) the extent to which it is restricted to particular individuals and/or groups; and (2) whether the restrictions are consensual or cooperative.

The extent to which one can demonstrate that particular patterns of decisionmaking are likely to leave clear patterns in material remains is critical to the success that archeologists are likely to achieve in studying political organization in the past. The difficulty of this task should not be underestimated. While it is beyond the scope of this essay to discuss all of the issues mentioned above, a consideration of variation in access to space will illustrate the problems that arise and approaches that might be taken.

In recent years, a large body of literature has appeared concerning human territoriality (see Plog and Upham, in press, for a summary). There is little agreement about territoriality in these studies, the vast majority of which, both archeological and ethnographic, deal with band level societies. As Dyson-Hudson and Smith (1978:21) point out, "The territoriality controversy in anthropology has primarily focused on hunter-gatherers." King (1975, 1976) and Peterson (1975) are recent examples of a long line of anthropologists (e.g., Radcliffe-Brown 1930; Service 1962; Williams 1974) who argue that some form of territorial band is the optimum pattern of spatial organization for hunter-gatherers under all or most ecological conditions. Various authors (e.g., Lee and DeVore 1968; Damas 1969) have argued that a more flexible pattern of spatial organization and resource utilization is typical of hunter-gatherers.

Dyson-Hudson and Smith ultimately argue that both patterns are possible but will occur under different circumstances. Specifically, defense of an identified territory is likely to occur only when,

> critical resources are sufficiently abundant and predictable in space and time, so that costs of exclusive use and defense of an area are outweighed by the benefits gained from resource control (Ibid).

One can extend their analysis by noting its articulation with the common anthropological distinction between proprietary and usufruct rights. In essence, at any level of social organization (the family, village, clan, or cultural group) individuals and groups may seek to claim proprietary rights over space. Yet such a claim is neither necessary nor necessarily common, but only likely to arise in specific circumstances as described above.

This formulation is particularly useful for archeological purposes because it forces upon us a clear distinction between the observation that a particular group of people occupy space and the claim that they either define or defend an explicit territory. In all probability, the continuum between these two extremes, especially prior to the existence of state organized societies, was quite complex. It is difficult to believe that there was not a great deal of fluidity in the spatial domain that a given hunting-gathering band occupied. In all likelihood, that space changed regularly in response to the growth and decline of a group, similar processes operating in those around it, and short-term and long-term fluctuations in climatic patterns. This same variation would have produced changes in the extent to which territories were explicitly defended. At the other extreme are groups that defend territories and do so with complex political coordination and use specialists, soldiers. Many combinations are possible. Many specific organizational entities can assert proprietary rights over space: households within villages; villages; clans that are either within or cross-cut villages can all claim and defend rights to space. Similarly, there is no reason to assume a temporally invariant pattern for any given location.

Given this potential complexity, what evidence of the waxing and waning of territorial behavior can archeologists expect to discover?

Distributions of Material Traits

It is tempting to view the spatial zones that can be defined on the basis of material trait distributions as indicators of territories. It is true that in at least some areas it is possible to define periods of time when the distribution of such traits is relatively homogeneous over a broad area and others when a number of highly distinctive divisions of that same space are warranted. Unfortunately, our growing understanding of the manner in which such boundaries are generated by interaction and exchange provides little support for the notion that such distributions reflect actual territories. Whether such zones were occupied by particular ethnic groups or whether they simply describe patterns of particularly intense interaction or exchange, there is no a priori basis for inferring that the spatial entity in question was consciously perceived as an exclusive territory, much less defined as such. I am not arguing that such distributions are irrelevant to the topic of this paper and will return to the issue later.

Boundary Markers

Shrines, cairns, petroglyphs, and even potbusts, have been identified ethnographically and ethnohistorically as territorial markers. While it is not possible to say precisely what limitations of access are intended by such markers, some restriction of access is clearly being symbolized. Thus, to the extent that the use of such devices varies in time and/or space, increasing concern with access to space is indicated.

Warfare

It is tempting to take evidence of warfare, in the form of mutilated bodies, etc., as evidence of territoriality. It is evident, however, that (especially prior to the advent of state organized societies) warfare and raiding have no necessary connection with actual defense of territory.

Architectural Features

Architectural features such as forts, defensive walls, garrisons, or signal towers are suggestive but not conclusive evidence of warfare and territorial defense. Nevertheless, it seems reasonable to assume that when a society reaches a point of making a major labor and material investment in defense, territory is likely to be an issue. Unfortunately, analyses of the spatial distribution of such features that could, for example, indicate that they bound a spatial unit or occur at key passes between different valleys, have rarely been undertaken.

Art

Representational art can provide clues to the existence of more or less formally defined groups of warriors which must, as described above, at least strongly suggest a substantial concern with defense of territory.

Unfortunately, most of the evidence discussed above pertains largely to the more complex end of the political spectrum. In the case of simpler groups, one can only caution against overly quick territorial assumptions. While analysts should certainly seek to identify changing patterns of spatial use, these must never be confused with actual restrictions of access to space. It does seem likely that appropriate ethnographic, especially cross-cultural studies, might provide good indirect evidence of highly territorial behavior. For example, once societies are making substantial investments in the construction of features that improve agricultural land or create permanent dwellings, a greater concern with territory seems likely. At the same time, such correlations lead directly back to the problem mentioned earlier: if settlements with permanent architecture or the presence of intensive agricultural systems are taken as evidence of territorial behavior, one cannot study the relationship between the former and the latter.

The preceding discussion envisions harnessing all of the available distributional information concerning the study area for the purpose of identifying meaningful sociopolitical and sociocultural entities. The proposal is essentially to set aside the site-focused efforts of investigators such as Longacre (1970) and Hill (1970) and to begin with definitions of broader patterns. Fortunately, such an effort has already been undertaken for the late time period in the study area. Upham (1980) has done an analysis very similar to that outlined in the preceding pages. He has found evidence of site clusters that may represent local polities and of variation in the exchange ties between particular polities. Upham's work provides a more succinct and comprehensive model than can be described here. Extending his analysis into the earlier periods of time in the study area would accomplish the goals identified here.

TEMPORAL VARIATION

Unlike the recent literature on spatial variation, current discussions of temporal variation are far from convergence. Whether the topic is as specific as the use of radiocarbon dating (Read n.d.), the contextual analysis of dated samples (Dean 1978) or theories of change (Plog, in press), the current literature places principle emphasis on the many problems that must be resolved if we are to deal with temporal variation effectively. Initially, I will summarize what seem to me the more salient points of these discussions and then discuss approaches to change studies.

The association between dated samples and artifact assemblages must be more carefully defined. When associational controls are not cautious, one runs the risk of rejecting viable temporal models. This problem seems to characterize the rejection of Wasley's horizon style system for Anasazi ceramics (1959), which Breternitz's (1966) tree-ring analysis of Southwestern ceramics appears to destroy. In point of fact, precisely what ceramic materials are associated with particular dated specimens, and in what quantity, is so highly variable that Breternitz's work cannot be considered a viable test of Wasley's model.

Chronologies must be recognized as hypotheses to be tested using a combination of chronometric and statistical techniques. In most instances, the most complete chronologies (those applicable to the greatest number of sites) will be based on ceramic seriation. These should always be done first, with appropriate quantities of chronometric techniques employed to a sufficiently large number of specimens to permit statistical tests of the viability of the chronology. The odds that inappropriate specimens or techniques with too high an error factor will have to be used are so high that a meaningful test of an entire chronology is improbable.

Spatial correlates of temporal processes should not be assumed. In those few instances where historical records have been used to evaluate the presumed spatial correlates of a temporal process, e.g., diffusion from a center of innovation, great variation has been found. Moreover, when one infers temporal processes from spatial patterns, the use of evolutionary arguments to explain particular spatial phenomena is likely to become circular.

Observations should be made for a sufficient number of temporally discrete points. Too many archeological studies have been before-after studies. Growing evidence in both the social and natural sciences (Hamblin, Jacobsen and Miller 1974) suggests that unless 7 to 11 discrete points can be measured, the description of variation over time for any given variable is likely to be quite problematical.

Continuous, rather than categorical, variables should be preferred. By their nature, categorical variables reduce the variation with which an investigator can work. The essence of studying evolution or change is discovering the manner in which variation is shaped. While categories (tribes, chiefdoms, etc.) may roughly characterize the most common outcomes of particular change episodes, it is improbable that we will ever understand why particular structural configurations are more probable unless we employ techniques that allow the identification of the diversity from which they emerge.

Linearity of change processes should not be assumed. Linear change processes are extremely rare in both natural and social phenomena. One runs an immense risk in assuming linearity underlying dating techniques (as the radiocarbon experience indicates) and social processes.

Derivatives of variable trajectories should be investigated. For some social and natural science problems, regularities and patterning have proven difficult to identify when simple plots of variation over time were analyzed. When attention turned to the investigation of rates or even derivatives of change, substantial regularities were discovered.

Immediate causality should not be presumed. Again, in both natural and social scientific phenomena, dramatic changes occur as substantial lag effects, post-dating the triggering event(s) by substantial intervals. Systemic effects also confound efforts to deal with linear and immediate causality.

Explanations of change processes should focus on trends and not event-outcome connections. The same triggering event can have different outcomes and the same outcome can have different triggering events. It is for this reason that most natural science laws describe trends or tendencies, not events and outcomes.

Specific changes occur in the context of many other changes. Too many of our efforts to explain change focus on the environmental and organizational context in which the change occurs. A particular change may just as well result from some aspect of the change processes itself, as when an increase or decrease exceeds some limit and deviation amplifying processes are triggered.

There are, of course, many problems with specific models and theories of change, both in their structure and in their application. However, I would argue that the majority of these specific problems reflect difficulties in the overall approach taken to change studies, the way change is conceptualized, of the sort discussed above.

The focus of the preceding section has been on concepts for interpreting temporal variation. Most of the specific needs for investigations within the study area were identified in earlier chapters. Nevertheless, some conceptual coherence is essential if management strategies in the study area are to be interpretable with respect to one another.

BUILDING PROCESSUAL MODELS

Explaining temporal variation necessitates building testable models for investigating the topic phenomenon. In this section, I discuss two aspects of that process, the definition of processual variables and the construction of processual models. Three different sets of variables will be discussed; long-term, short-term, and programmatic.

Long-Term Processual Variables

Based upon his extensive analyses using the human relations area files, Naroll (1973) has argued that there are really only a few well defined lines along which human behavior and culture have evolved. In his analysis, these are in fact major differences among ethnographically described societies. But, there is good justification for arguing that there is archeological evidence for change of the sort that he envisions. While I have added some ideas of Flannery (1972), and redefined some of the terms for clarity's sake, I would suggest that the most important long-term changes in human behavior are measured by the following variables. (Sources of more operational definitions in parentheses.)

1. Intensification: change in the product derived per unit of land or human labor (Boserup 1965; Sanders 1973; Logan and Sanders 1975).

2. Specialization: change in the number of specialists; change in the percentage of the entire set of activities carried out in a society in which an average individual is likely to be engaged (Wright and Johnson 1975; Plog 1974).

3. Stratification: change in the relative access of individuals and groups to resources and power (Adams 1966; Sanders 1973; Tainter and Cordy 1978).

4. Nucleation: change in the size of settlements in which humans dwell; change in density (Swedlund 1975; Cook 1972; Baker and Sanders 1972).

5. Centralization: change in the extent to which sociopolitical and socioeconomic decisions are concentrated in the hands of a few individuals or institutions (Flannery 1972; Sanders 1973).

6. Differentiation (or diversification): the number of separate organizations or institutions (Flannery 1972; Plog 1974).

I believe that these variables describe the major long-term changes that have occurred and are occurring in human societies. When I say long-term, I mean that the periods over which changes occur is measured in centuries or millenia. This is not to say that they can't or don't sometimes change over shorter periods (that are called "revolutions") but that frequent radical changes are unlikely in an ongoing society.

I am not arguing that one can make laws of such trends, "centralization tends to increase," for example. Such a statement is nonsensical because centralization can clearly increase as well as decrease and has done so in the past. They are more properly regarded as consequences of many short-term processes operating within a society. But, they do provide a means for defining and using continuous variables to describe (a) differences between two societies at the same point in time, and (b) differences over time. Moreover, specificity and generalization are not opposed--the measure at each instant of time and the measure of change over time utilize the same variables.

Short-Term Processual Variables

I recently suggested a list of variables for modeling exchange betweeen the inhabitants of different settlements (Plog 1977b). Before the article appeared in print, I discovered that Michael Schiffer (1980) had formulated a nearly identical list of variables in discussing how one might model an activity. In retrospect both of us were asking how one models an ongoing behavioral system and arriving at reasonably similar answers. We both begin with the notion that what is worth modeling is not the things (people, goods, institutions) of which a system is composed but the nature of the interaction between the parts of the system. In human systems this interaction most basically involves exchange of information and goods. Major variables that must be considered to accurately describe such a system follow:

1. Content: the things that are being exchanged.

2. Size: the number of people involved in the exchange.

3. Magnitude: the quantity of things that are being exchanged.

4. Diversity: the number of different kinds of things that are being exchanged.

5. Frequency: the incidence of exchange in time.

6. Duration: the length of the exchange.

7. Territory: the spatial extent of the exchangers.

8. Directionality: the direction (one or many) in which the goods or information flow.

9. Symmetry: the relative quantity of the flows in each direction (even or uneven).

10. Centralization: the extent to which the exchange is centrally regulated.

11. Complexity: the extent of variation in the first 10 items from area to area and time to time in the operation of the system.

Again, each of these is a continuous variable; changes in each can be plotted over time. Moreover, the same variables are relevant, whatever the scale of analysis--a few individuals, a settlement or a series of interacting settlements. Changes can be, and are, relatively short-term occurring over days, weeks, and years, although longer periods are also possible. There are two other aspects of the variables that are appealing: (1) they are characteristics of the entire system; not simply a part, they synthesize a critical bit of information that relates to the entire system; and (2) they are dynamic. In passing, it is noteworthy that moving from a focus on things to a focus on interaction has been critical to the growth of most disciplines.

Programmatic Variables

Throughout the discussion thus far, I have talked of temporal variables as if they were almost exclusively described by lines on a graph. Obviously there must be words, variables, that can be used to describe these lines. I suspect that four variables are both necessary and sufficient for describing any temporal process:

1. Magnitude: the scale over which variation occurs 101, 102, 103, 104, etc.

2. Amplitude: the height of the curve.

3. Frequency: the duration of cycles, if any.

4. Slope: the overall direction of the curve (up, down, constant).

Having defined such variables, they too can become parts of particular causal models.

Underlying Assumptions

However neat the models and concepts, some notion of people and what they are about must be the basis of any approach. Some of the most important current ideas have, in fact, grown out of elements used to build a more dynamic evolutionary theory in biology. I rely here, in particular, on the works of Slobodkin (1968, 1972) and Holling (1973).

In modeling change it is necessary to remember that people have a number of alternative responses that they may employ to a change in their situation; alternative behavioral, cultural, and physiological responses are all possible. Behavioral and physiological changes can obviously occur far more quickly than cultural ones. But all are possibilities; all must be explored in relation to the specific change in question.

In nature, the only constant is change. It makes no sense to try to explain behavioral or cultural changes by citing a change in the natural environment; people are adapted to change in their environment. One may attempt to demonstrate, however, that the magnitude, duration, frequency, or novelty of a particular change is sufficient to drastically alter the circumstance in which people find themselves.

By the same token, evolutionary success is a matter of resiliancy, not stability. Stability is a measure of the ease with which a system returns to equilibrium after absorbing disturbances. Resiliency is a measure of the degree of change it can undergo while still maintaining its basic elements or relationships. It is the resilient, not the stable, who inherit the earth. But, how does one measure resiliency? Human numbers are an initial factor; the more expendable bodies in a system, the greater its resiliency. But, clever (not wise) strategies for adapting (not proper, but smart, in Frielich's [1973] terms) are what ultimately make the difference.

If resiliency matters most, then it makes sense to think of evolution as an existential poker game the object of which is simply to stay in the game. Optimization and maximization are probably less accurate descriptions of what people strive to do than satisficing (Isard 1975) or coping. Most people, most of the time, are not involved in inventing clever strategies to acquire the most poker chips possible for the least effort. They are simply trying to get by.

In evolution there is a demographic baseline. Given instability, given that the prehistoric peoples continually faced changes in resource availability and in their own numbers, two strategies were possible: (1) limit population; and (2) increase the production of resources, (intensify). Both strategies effectively solve the problem. But, strategy one results in a retardation or cessation of growth while strategy two does not. The members who practice the latter become more numerous relative to those of the former. It is for this reason that the earth is not populated by hundreds of small stable societies effectively balancing resources and their own numbers but rather by rapidly growing and rapidly declining societies.

Preliminary studies along the lines suggested above have been done in many areas. And, there are other studies that could be done almost immediately. However, much of the temporal detail of prehistoric activity in the overview unit cannot be described until more and better chronological techniques are employed and the resulting data are handled using variables that are amenable to study through time.

REGIONAL PLANNING: THE SIGNIFICANCE ISSUE

Regional planning is an effort to insure that cultural resources are never casually destroyed and to avoid the costs of protecting expendable resources. If we fail in the former, we will be the parties most responsible for the destruction of our resource base. If we fail in the latter, others will justifiably insist on the right to make for us decisions of which we have proven incapable. A pivotal concept is significance. In this section, I will discuss the issue at a general level and turn to more specific implications later.

It is safe to say that only a decade ago few archeologists had given meaningful thought to the significance of archeological sites. Certainly there were sites of sufficient importance that they were declared National Historic Landmarks. Others attracted excavation projects. Similarly, there were sites worth salvaging and those that weren't. But, the boundary between sites that deserved preservation and those that did not was largely unexplored.

For reasons familiar to archeologists, that situation has dramatically changed. There are several extensive and insightful explorations of the concept (King, Hickman, and Berg 1977; Schiffer and Gumerman 1978; Moratto and Kelley 1978), and also of the conservation and preservation ethic from which such a concept is derived (as above; Lipe 1974). These treatments are themselves summaries of the use of the significance concept in hundreds of specific resource evaluation studies.

Despite the immense growth in the precision with which most archeologists understand and define significance, despite the growing concord within the professional community, we are told that there is a problem with significance (see especially ACHP 1977). The problem is described in a variety of ways. According to some, the definition of the term is simply unclear. According to others, the concept has been extended in application to include sites that are not significant under the original intent of the pertinent legislation, executive orders, and Code of Federal Regulations. According to still others, the concept generates preservation processes that are too slow and too expensive for proper government planning. Again, if one believes the Advisory Council on Historic Preservation's (ACHP) "Issues in Archaeology" (1977), there is little that goes wrong in conservation planning that is not blamed on significance. As a result, the concept of significance is the major focus of the ACHP task force currently studying the archeological preservation process.

It is appropriate to step back from the emotion of the current concerns and ask what a concept such as significance should accomplish and whether or not the current definition meets those standards.

EVALUATING THE UTILITY OF SIGNIFICANCE

It is easy to say that the ultimate measure of the utility of any concept is its clarity. In point of fact, some of the most critical concepts in any profession are of dubious clarity--one need only mention the thousands of pages that have been spent in exploring the meaning of the term "culture." Nor is such ambiguity the exclusive property of academics. Were legal concepts and their meaning exquisitely clear, courts of law would consider only neat questions of application, not the tortuous issues of interpretation-in-application that form the theoretical basis of the legal profession and consume years of court time.

Certainly, there has been a problem in the clarity of the term. Two literatures exist, one within the federal archeological community and one within the academic archeological community, that explore the meaning of significance. There is, I think, remarkable agreement on pertinent issues given the limited interaction between the two communities. Moreover, relative to the time spent in defining and redefining most legal and quasi-legal concepts, the evolution has been quite rapid. Finally, I think, there is little doubt that, were the members of the academic and federal communities who have invested the greatest effort in exploring the concept to meet, still greater clarity and agreement would result.

Despite the existence of the literatures, and irrespective of my claims of increasing agreement on the abstract meaning of significance, there is the separate but related issue of its clarity in application. Some argue that the lack of clarity in the case of specific sites is the real problem, or that inconsequential sites are being called significant. The implication is that sites' significance ought to be immediately obvious and that the significant sites are the larger more interpretable ones. There is sometimes the further implication that the problem of significance is unique to archeological sites: because they are beneath the ground their significance is not manifest.

I observed earlier that few legal or professional concepts could meet the standard of clarity that underlies arguments such as those discussed in the preceding paragraphs, especially in so short a time. The problem is, I think obvious--significance is a pivotal concept in a legal and political process. As is the case with any such concept, parties with different interests in a particular case will seek to interpret the concept to their own ends. Two examples may illustrate the problem.

In a recent study of a highway right-of-way, a small historic structure was located and determined to be the homestead of the Correjo family who settled in the area in question about 85 years ago. In due course, the issue of the site's significance was taken to the ACHP and the National Register where it was determined that the site possessed integrity, was reflective of a distinctive architectural style, and reflected important events in the area's history. The historic archeologist who undertook the on-the-ground evaluation of the site noted that the main structure was marked by only three courses of wooden logs and that if this site was significant there were tens of thousands of other such sites that would eventually clutter the register. It is difficult to conclude from this case, that the problem of inconsequential sites is uniquely prehistoric; historic sites are equally a problem. The real problem is either political or administrative, but, in any case, reflects inflexible adherence to a misinterpretation of the ACHP's guidelines rather than a legitimate question of significance.

A second case involves a site that covers an area of more than a square mile, a site at which limited and unsystematic excavation was done in the 1920s. A 22-foot deep, 8 lane freeway will be excavated through 2.3 linear miles of the site. Existing evaluations of the site's significance are based on surface remains because the agency in question refuses to undertake testing until a final right-of-way decision is made. Professional opinion ranges from a conclusion that the site is significant to one that it lacks both integrity and significance. While the disagreement is phrased in terms of significance, the real problem is the failure to undertake preliminary studies of the sites at an early point in the planning process. In any case, the problem of significance does not arise only in the case of small sites but can clearly occur in the case of very large ones.

Interestingly, in both of these cases the concept of significance serves to structure the controversy. The concept is certainly not the origin of the controversy. Structuring and thereby helping to clarify the controversy is, I submit, the function of a good concept. To blame the concept for the existence of the controversy is nonsense.

CONSISTENCY

A final consideration, the basic one I think, is whether the concept can be applied consistently. Surely, a major problem exists if a concept is defined in such a way that there are inherent inconsistencies when it is applied in different situations. One might envision two areas in which such inconsistencies might arise in the case of significance: the application of the concept in different cultural regions and its application by different agencies with different missions.

The cultural resources of different regions are, of course, highly varied. Sites can be marked by adobe or stone walls, by mounds, or by no more than a thin scatter of artifacts. But, it is not the kinds of artifacts or architecture found on most sites that make them significant or insignificant, it is primarily their potential for yielding information important to understanding a variety of different aspects of prehistory. Even should the specific central research problems vary from one region to another, the relevance of a particular site to those problems remains the key issue.

There is no question that the application of the term in specific contexts will vary with the nature of the research problems and the abundance of sites at which those problems might be pursued. However, the problems resulting from this variation will be resolved by more complete regional overviews and inventories, far superior to any that currently exist, and more precise models predicting the abundance and distribution of particular site types.

Does the definition produce inconsistencies when applied by different agencies with different missions? Clearly "land management" and "project" agencies encounter the archeological record in very different forms. Land management agencies are dealing with millions of sites on millions of acres of land, while project agencies attempt to cope with a few, a few dozen, or a few hundred sites on typically small and spatially disparate (e.g., highway rights-of-way) parcels. Interestingly, it is not the land managing agencies caring for millions of sites, many of them "inconsequential" in some sense of the term, who find a problem with significance.

Project agencies do face a special problem because a single project involves only a few sites. It is difficult to assess significance against a background of knowledge of the abundance of particular site types in the area when project funds cannot be used to develop that background. But, this is a problem that is best resolved by the creation of high quality overviews and long-term planning documents. It is difficult to see that a definitional change will resolve the issue. Moreover, I suspect that for some agencies the real complaint is reflected in statements such as the following:

> . . . once you go to work for the Federal government in the area of historic preservation, you all but become automatically suspect in the broader academic world. (Weakly 1977:20-22)

> The States are faced with a slow down of Federal funds which causes them to resist the compliance, not as a matter of fact, but to see what they can get away with to prevent delays. (Crecco 1977:32)

> We anticipate pressure being exerted on our agency, and I wouldn't be surprised if it fell on other agencies as well, to expedite projects at the expense of the existing cultural preservation system. (Olson 1977:46)

Again, while there are agency complaints concerning significance, the overwhelming majority of the specifics seem to concern procedure. Will not these complaints exist whatever the definition that is used to evaluate the need for conserving/preserving specific resources?

THE ALTERNATIVES

Authoring an impassioned defense of the status quo is not an activity in which I have great experience. Yet, to this point I have done just that. As I have argued, the problems I perceive are not with the definition of significance but with the context in which that definition is now used. I wish to go further and suggest that some of the proposed alternatives either do not solve the current problems or make them worse. None of the proposals I will discuss are the unique creation of a single individual, and I do not intend to attack any specific proposal or its author.

A first proposal has been to create a checklist or scorecard of characteristics that a site might possess. A site with a sufficiently high score would be considered significant. There are a number of problems with such a proposal. First, an inflexible quantitative boundary between significant and nonsignificant sites is necessarily arbitrary and will result in arbitrary decisions concerning cultural resources. Second, it is doubtful that a single list of criteria can be applied nationwide. Sites in some regions and some types of sites will almost necessarily receive lower scores. That is to say, the procedure would almost certainly result in inconsistencies. Third, the use of such a system would require overviews and planning studies that do not currently exist. Fourth, such a procedure violates the spirit, if not the letter, of pertinent legislation. Finally, such lists would increase the quantity of grounds for arguments about archeological sites. Rather than phrasing debates in terms of a central concept and arguing about its interpretation, as many as a dozen criteria could become bases for dispute. The discovery of the type-variety system did not stop the proliferation of taxonomic units; it simply shifted the growth in numbers from types to varieties. Similarly, writing a more detailed definition of significance will not stop the proliferation of arguments concerning significance, but will increase the grounds on which arguments can occur and, ultimately, their number.

A second proposal is to create a master list of significant sites that is either a real "honor roll" or a random sample of sites or some combination of the two. Such an approach presumes the completeness of our understanding of the archeological record in two ways. It presumes that no new sites of significance will be found and that we will continue to evaluate the significance of particular sites and site types exactly as we evaluate them today. Were protection extended only to such a list of sites, much of the progress of recent years would be destroyed.

While the complaints concerning significance are obvious to us all, the successes are rarely so evident. For example, on land managed by the Forest Service and the Bureau of Land Management, thousands if not tens of thousands of sites that would have been casually destroyed in the past are nearly effortlessly saved by small changes in project locations. These are made by staff archeologists, paraprofessional archeologists, and managers of other resources who have been sensitized to the ease with which cultural resources can be preserved when there is an appropriate planning process. It would be folly to undercut a structure that results in the preservation of so many sites with so little friction by denying cultural resource managers the option of simply presuming significance. Yet, any effort to tighten the definition of significance would do just that.

PLANNING: IMPLEMENTING SIGNIFICANCE

If the definition of significance is not a problem, its implementation certainly is. And, it is precisely the preparation of sound regional plans that will render implementation less problematical. Sound regional plans are ones based on quantitatively and qualitatively acceptable data bases.

It is increasingly evident that something between a 10% and 20% sample is adequate for most studies in most natural and cultural circumstances as long as the sample unit is small and the number of observations correspondingly large. A recent study by Stafford et al. (1978), strongly suggests that much of the argument in the current literature concerning the size and shape of survey units and the efficacy of different strategies is misleading.

When variation in the number of observations is controlled for, such considerations are far less important than once believed. After the 5% (and especially after the 10%) sampling fractions have been surpassed, information gain for each new unit surveyed drops drastically. Past 20%, the new information gain does not justify the greater expense. Of course, there are still archeologists who object in principle to sampling.

Evidence grows that even very small samples (ca. 2%) of large regions generate information on thousands of sites, more than we are methodologically capable of handling. As the way in which the highly selective survey procedures of an earlier archeological generation essentially constituted a sample the biases of which cannot be described or even estimated, such arguments lose credibility. Even when a complete inventory of sites in a region will eventually be undertaken, sampling is basic to designing a program that will ensure the wise expenditure of survey dollars.

Qualitatively, a data base must be evaluated against the best ongoing research methodologies appropriate to the region in question. It is precisely those standards that I have tried to summarize at a general level in the early sections of this overview. I wish at this time to identify somewhat more specifically the planning information that will be foregone and the risks that will exist if information meeting these standards is not developed.

Transformation Processes

My concern here is principally with the effects of natural transformation processes. While there are potentially important effects of cultural processes, a full understanding of these will require new analyses of excavated materials the conclusions of which can be extrapolated to survey situations.

A careful review of major land modifying activities that have been undertaken in a region is both necessary and relatively simple. While one might argue that for a small locality the effect of natural transformation processes is likely to be negligible, for an entire region the opposite is true.

Of particular importance are the effects of erosion and deposition since, at least in principle, these agents are capable of enhancing the visibility of cultural remains in some contexts and obscuring it in others. Checking for such effects is not difficult: when archeological sites cluster on geomorphologically stable ridges and are rarely found on alluvial plains, natural transformation processes should be immediately suspect. We serve our interests poorly if we fail to identify environmental contexts in which sites with little surface evidence occur since any deep land modification activities will destroy these resources.

Spatial Variation

Failure to generate a relatively complete description of cultural resources as outlined earlier creates two problems. First, unless the nature of spatial variation is understood in some depth, one runs the risk of permitting the expenditure of cultural resources on the assumption that they are not significant when in fact they are. Were we to believe that sites are sites are sites, we would randomize our protection of the cultural resource base and our understanding of it. The more refined our control of spatial variation, the greater our ability to differentiate critical from common resources, the greater our ability to wisely invest in the use of particular resources.

Second, understanding spatial variation as described earlier is in itself a planning tool. The generation of predictive models on the basis of environmental correlations is an example. On the one hand, controlling for transformation processes, this information is an aspect of understanding prehistoric behavior. On the other, it permits us to identify environmental contexts in which sites are likely or unlikely to be found and to assess particular projects with such a background.

Similarly, knowing the spatial distribution of cultural resources can greatly aid planning efforts. Even assuming that one is discussing areas in which site densities are high, it is useful to know something of the evenness of the distribution, for example. Depending upon the particular proposed impacts, it will be more or less easy to design a project in such a way that impacts will be minimized depending on whether points in the area are randomly, evenly, or unevenly distributed in relation to the proposed project zone and available alternatives.

Temporal Variation

The relationship of temporal variation to the planning process is less direct. Many of the specific pieces of information generated pertain to understanding the archeological record more than to managing it. Nevertheless, unless temporal variation and processes that need to be understood are clearly identified, one runs the risk of expending resources that represent potentially crucial pieces of information. Similarly, as we begin to understand such processes, it becomes possible to evaluate the impact of a particular project not in respect to a temporally homogenous data base but in respect to a more refined notion of the importance of particular sites to particular research efforts.

Given an understanding of the above, the class of significant sites consists of the quantity of distinctive sites of pertinent site types necessary to ensure an understanding of regional prehistory through the application of identified research strategies.

MANAGING CULTURAL RESOURCES

The discussion to this point has focused on assessing current understanding of cultural resources in the overview unit and identifying research efforts that are necessary for a more complete understanding of those resources. At issue now are management strategies that will ensure a framework in which that research can be completed as opportunities arise. Five such strategies can be defined: (1) completion of management planning and the inventory effort in the study area; (2) protection of resources from other land use activities; (3) regulation of consumption of the resource by the scientific community; (4) regulation of the consumption of the resource by the public; and (5) administrative studies. Each of these is addressed separately.

PLANNING AND THE INVENTORY EFFORT

A management plan for the portions of the overview unit under Forest Service administration has been completed. A management plan based on a Phase II effort (sample survey) for Bureau of Land Management and other agency lands has not yet been written. It will be difficult to pursue a management plan on Bureau of Land Management and other agency lands using the same strategies employed on the National Forests. The National Forest is a contiguous spatial unit. Remaining agency lands are discontinuous units. Moreover, and as a result of the spatial situation, planning needs necessary to inform wise management decisions are somewhat different since decisions must often be made that compare a 1 square mile parcel in one location with another several miles away. Such considerations must be based upon statistically valid information. Without multiple observations of each parcel statistical validity is impossible.

I, therefore, propose that the Phase II effort involve survey of 15 small units in each section under the jurisdiction of the agency conducting the survey. Fifteen observations are sufficient for a statistical comparison of the cultural resources of two populations. These sample units should be 50 yards wide and 120 yards long. This coverage is equivalent to a single transect used for planning purposes on the National Forests. Ten of the sample units would be chosen using a random systematic design to ensure even coverage of the area but with reduction of bias. Five might be placed in "likely locations" of cultural resources thereby allowing a judgmental effort to enter the survey design.

Thus, management information might be generated using two procedures. First, as management needs require the survey of particular parcels, those parcels would be sampled following the strategy identified above. Secondly, in a single contract or in several smaller contracts as funds become available, survey of remaining parcels would be undertaken. In combination, these two mechanisms should result in the generation of information necessary for a management plan within a period of a few years. In the meantime, management decisions could use available information for parcels with prior survey or generate new survey information.

Only one viable alternative methodology exists. If the Bureau of Land Management and other agencies holding dispersed parcels within the study area are willing to assume the risks of a nonprobabilistic sample, the risk of unstatable biases, then Phase II and Phase III planning (inventory) might be combined in the following manner. In any parcel, a block representing about 25% of the land surface of the parcel will be randomly selected for survey. Upon arrival in the field, the survey archeologists will have discretion to break this survey unit into two parcels. The situation under which that decision might be made would include suggestive evidence of high variation in the density of cultural resources, clear indications of extreme environmental variation, or both. Such a procedure would provide "some information" about each parcel held, but would lack the generalizability of the first approach discussed. On the other hand, it is a good compromise between "planning" and "inventory" goals, if one allows such a compromise.

INVENTORY GOALS

As originally envisioned, the consideration of future plans for obtaining an inventory of the cultural resources of the study area was to identify a series of programmatic goals consistent with agency deadlines.

Considering the various topics discussed herein, such an approach to cultural resources in the area seems unproductive. First, such an approach presumes that the primary goal of future research concerning cultural resources and their distribution is to obtain as complete as possible a catalogue of what is there. This goal makes no more sense than the presumption that successful management planning requires an inventory of every tree or every acre of grazing land within the study area. Clearly, one can plan for a timber harvest program or one for grazing without such detailed information. In the same manner, one can plan for the wise management of cultural resources without knowing where each and every one of them is located.

The original inventory goal assumes a set of conditions that may exist on some, perhaps most, public lands but not those in question here. The assumption is that cultural resources are relatively rare. It is difficult to speak of tens of thousands of cultural resources as a rare resource. That the resource is nonrenewable is clear. That without wise management it will disappear more quickly than many other resources, is clear. That each and every cultural resource must be described in the same detail is unclear.

The investigative strategy required to know every resource in even approximately the same detail would be wasteful of the tax dollar. There are places in the overview area where the probability of encountering cultural resources is close to zero. Yet, the dollars required to inventory those acres differ insignificantly from the dollars required to inventory acres on which cultural resources occur in abundance. The major expense incurred in inventory work is getting to a cultural resource. In this sense, whether the result of an effort to get to a cultural resource is successful or unsuccessful, the expenditure is relatively the same. Thus, the question of means of accomplishing the inventory goal without pedestrian survey of all public lands in the area is a critical one.

Such an effort is inconsistent with a multiple-use philosophy. Some resources are critical because they are used, trees for example. Others are important because their protection is in the public good. In the case of the latter, inventory is crucial to the extent that use activities sometimes have ill effects on protected resources. If land use is not occurring in a particular location, impacts on cultural resources are unlikely.

Finally, an effort to achieve an overall inventory is unnecessary because the "consuming public" would have no use for its results. There are two possible constructions of the consuming public in regard to cultural resources, those who use them for recreation--the general public--and those who use them for knowledge--the scientific community. It is obvious that the general public has no concern for such a resource in the quantity of tens of thousands of cultural resource loci. Scientific strategies for utilizing the evidence from tens of thousands of cultural resource loci do not exist at present, are unlikely to exist in the foreseeable future, and will be superceded by more economical strategies in the distant future.

For all of these reasons, it seems preferable to discuss the inventory problem not in terms of goals, such as acres surveyed or sites recorded, but in terms of how to achieve an increased understanding of how these resources can be routinely preserved and conserved as more "consumptive" activities are carried out on public lands. This can be done in much the same way as projects are defined so as to avoid major watershed impacts. This is not to say that there are no plans for the consumers that need to be formulated. These will be discussed later. The point made here is simply that there is a difference between the inventory task on the one hand and provisions for the wise use of the resource on the other. This last question will be discussed in a later section.

An inventory plan should articulate with other land use activities. When a particular area is to be impacted because of a timber harvest, land exchange, or road construction, the designation of that area for a cultural resources survey is essentially random in regard to the cultural resources. The growth of the inventory in conjunction with, rather than separate from, other forests goals is ultimately consistent with archeological as well as management goals.

There is no justification for additional transect surveys for planning purposes on the National Forests. The distribution of resources described for the Apache-Sitgreaves National Forests is probably as good as can ever be achieved using such a device. In fact, and given that hindsight is always 100%, current conclusions (Plog 1981) could have been reached with approximately 25% less effort, which--given that a substantial part of the effort was at no expense to the Forest Service--equates with about 10% less expenditure by the Forest Service in relation to the current project, given the substantial volunteered time.

Transects are an inefficient inventory unit for further research, even though they are an efficient planning tool. Inventory is best accomplished in more sizeable areas. There is ultimately some indecision as to which sites have been recorded and which have not, what areas have been surveyed and which have not when transects are used as a primary tool. Moreover, the critical planning information that is not contained, but only suggested, in this report is local variation in site distributions. Only through the survey of relatively large blocks of land evenly spaced over the study area will such information be obtained.

Project areas are not always of a size useful for inventory purposes. Small and sinuous project areas provide a limited basis for spatial generalizations. For this reason, it will often prove useful to attach inventory dollars to project dollars to increase the size and regularity of the boundaries of a study area. In this way, inventory goals and other planning goals can be accomplished together.

There are some portions of the study area for which pedestrian survey is an inefficient means of obtaining an inventory. In those areas where site densities are high the cost of obtaining an inventory record, assuming a standard survey cost of $10 per acre, is about $160. Where site densities are very low, the cost rises to $6400 per site (if only a single site is found). While some gains in the efficiency of survey in low density areas are realized, the strategy is still not cost effective.

In areas above 8200 feet, for example, cultural resources likely to be found include shrines and historic sites. The former may often be documented using a check of likely loci--springs, peaks and promontories. The latter are perhaps best documented, although clearly not exhaustively, by records searches. None of this is to argue that every specific project will not require some effort to identify, conserve, and preserve cultural resources, only that the identification of all such resources is not best accomplished through an inventory effort.

These same conditions will exist in the more arid, desertic, portions of the overview unit. While the nature of locations where sites are likely to occur is not clear at present, studies could clarify its meaning and lead to far less expensive survey efforts.

With these goals in mind, an inventory is probably best accomplished through a number of activities.

1. Drawing upon the conclusions of this study, an effort should be made to identify the boundaries between areas with, and areas largely devoid of, cultural resources.

2. In areas where the density of resources is likely to be quite low, the inventory effort should focus on checks of likely locations in the case of prehistoric resources and on records searches in the case of historic resources.

3. In areas where site densities are likely to be high, an initial 10% survey should be used to define specific areas where resources occur. A 100% sample should be designed on the basis of the information obtained in this preliminary stage.

4. Where project areas are small and/or irregular, inventory dollars should be used to create larger and more regularly bounded study units.

5. Given identified needs for resources on public lands, inventory dollars should be used to fund studies lying in areas where immediate project needs are not substantial.

6. The immediate goal of such efforts should be greater understanding of local variation in cultural resources so that projects can be defined so as to avoid them. The long term goal is inventory.

A survey of the entire unsurveyed acreage within the study area would cost over $20 million assuming an average cost of $10 per acre during the time that the survey is done. Such an expense is unjustified from the perspective of wise planning for the preservation and conservation of cultural resources. The $10 per acre figure converts to a cost of roughly $700 per resource. There is no consumer of the resource that requires the information generated by such an expenditure.

If inventory survey is focused on areas where sites are likely to be found, the cost can be greatly reduced. For the specific case of public lands administered by the Forest Service, the reduction would be to about $305 per resource. Even at this level of activity, the cost is high and the availability of necessary manpower to conduct the survey within a reasonable period of time dubious. By further refinements of the need for inventory survey in specific areas, cost can be further reduced. These refinements include the identificaiton of areas where checks of likely site locations are a more justifiable tool than generalized survey. In the case of the forests, the proposed plan would result in a cost of about $160 per resource. For the entire overview unit, an expenditure on the order of $7 million would be required. While high, the cost is far below the initial $20 million figure.

PROTECTION FROM LAND USE ACTIVITIES

An earlier section of this document discussed the nature of impacts and described strategies either currently in use or that could be developed for avoiding further impacts to cultural resources. The continuation and improvement of these strategies is the primary basis for protection proposed in this study. One question remains, however: how to proceed when a situation arises where impacts to at least some cultural resources are unavoidable.

The answer to this question presupposes some effort to complete the inventory of the study area along the general lines just discussed. I will use the Apache-Sitgreaves National Forests as an example and assume that there are about 18,000 such resources on the National Forests distributed among site types in the manner shown in Table 33. Further inventory will certainly result in the refinement of these figures. I also assume that it is unlikely

Table 33. Numbers of Prehistoric Site Types

Site Types	Numbers
Surface habitation structures Pueblo	
1 room	1731
2-5 rooms	3159
2-5 rooms, with kiva	87
6-9 rooms	476
6-9 rooms, with kiva	43
10+ rooms	619
unknown number of rooms	173
Pithouse structures	
1 room	649
2-5 rooms	433
6-9 rooms	43
Combination of surface and pithouse structures	303
Artifact scatters	
1-9 square meters	173
10-99 square meters	1211
100-999 square meters	3894
1000+ square meters	865
unknown square meters	260
Lithic scatters	
1-9 square meters	43
10-99 square meters	260
100-999 square meters	606
1000+ square meters	130
unknown square meters	43
Ceramic scatters	
1-9 square meters	87
10-99 square meters	303
100-999 square meters	216
Water control features	822
Kivas, alone	216
Rock ring or amorphous rock structures	563
Petroglyphs	43
Historic	130
Rock shelters	216
not enough details	606

in the case of projects that have major impacts to acquire the level of funding sufficient to mitigate by a data recovery program all of the resources that are to be impacted. Given all of these assumptions, the quantity of such resources available for study and their distribution on different districts should serve as a major guide to decisions as to where mitigation dollars should be directed.

For example, the most abundant site type on the National Forests is an artifact scatter between 100 and 999 square meters in area. There are potentially 3894 such sites in the study area. There have been five excavation projects done at such sites in the last 20 years. If these sites are used by the scientific community at the rate of one every four years, the resource would not be spent until the year AD 17,555. By that date, archeologists, if they still exist as an identifiable profession, will be studying the archeology of us. To the extent that prehistoric materials are still necessary to archeological research, improvements in site discovery techniques, and in the economy of analytical techniques (not to mention the vast amount of pertinent materials that will be stored in museums and at universities), will probably assure the adequacy of a data base in ways that we cannot currently envision. In the context of a multiple use philosophy, the expenditure of funds to protect or improve some other resource seems far more justifiable than the protection of cultural ones.

At the same time, the distribution of such resources must be taken into account. For example, in the case of artifact scatters of the size we have been discussing, there are 33 times the number of such resources on the Pinedale Ranger District as on the Alpine Ranger District. On the Alpine District they represent a far rarer resource and are therefore more crucial to interpreting the prehistory of that district.

Clearly, the boundary between these two districts is not a cultural boundary and, therefore, variance estimates might be further refined. However, the boundary between the two districts is a natural one and, therefore, may have been a cultural one so that the estimates are unlikely to be very far out of line.

It must also be recognized that there are within the study area very scarce resources. Compounds, defensive sites, and multi-hundred room pueblos are examples of known rare sites. PaleoIndian sites are examples of types that may be present although they are not currently identified. These very rare site types warrant the highest degree of protection, especially in regard to vandalism. The utility of formulating a list of eligible or "super-eligible" sites is dubious. In the absence of detailed field inspection, the current integrity of many of the sites on which records have now been assembled is also dubious. Similarly, the probability of highly significant sites that have not yet been inventoried is dubious. I cannot honestly respond to the issue of which sites within the overview unit are most worthy of nomination to the register on the basis of available information.

THE SCIENTIFIC COMMUNITY

Archeologists are one major category of potential user of the cultural resources on public lands. Unfortunately, despite the use of the "conservation-preservation ethic" to ensure enlightened treatment of resources by public agencies, there is still some insensitivity to the resource impacts created by archeologists themselves. In one recent case, an archeologist working in Arizona chided the Forest Service for its insistence that a road be moved to avoid impacting a prehistoric site when the Arizona Department of Transportation (ADOT) was willing to pay for the excavation and the archeologist was interested in undertaking it. Such a position is clearly inconsistent with the ethic in question. But, it is no less inconsistent than the behavior of a doctoral dissertation committee that fails to insist that students demonstrate the need to pursue a particular research project using newly recovered materials rather than existing collections. The profession as a whole has invested little effort in exploring the strengths and weaknesses of such collections, presuming that they were collected using techniques that are below current standards and thus they are totally useless.

For these reasons, it is entirely appropriate that public agenices develop their own strategies for ensuring that the resources they seek to protect are never taken unnecessarily. While this discussion is of one particular area the amelioration of the problem will most likely result from a coordinated effort at the regional level. If different agencies, not to mention administrative units within them, have drastically different policies, more resources will be taken on some land than others and our understanding of the regions history and prehistory correspondingly biased. A first step in that direction would involve systematic review of all research proposals by the appropriate representatives of different agencies sitting as a panel.

The subject of their review should be a document that is both a research design and a demonstration that the resources that will be taken are essential to the success of the project in question. I use the term "essential" purposefully. The greatest percentage of the region's cultural resources exist on private land, where they are totally unprotected, or on State lands, where the level of protection currently given them is far less than desirable and far less than that characteristic of Federal lands. It is preferable that the scientific community take resources from State and private lands, leaving the better protected ones on federal lands as a storehouse for the future.

Drawing upon the general literatures of anthropology and archeology and others that will undoubtedly be generated in the near future, a number of questions should be addressed.

1. What are the theoretical, methodological, and empirical goals of the project?

2. What specific theoretical, methodological, and empirical advances would result from the project?

3. What categories of data are necessary to the completion of the project and in what quantities must these data be available given the inferential techniques that will be used in the study?

4. Why are data from federal, rather than from State or private, land essential to the success of the project? What are the available options on the latter and why are these unsatisfactory?

5. What existing collections have been evaluated in regard to their adequacy for the study? Why is the recovery of new cultural materials essential to the success of the project?

Clearly, this list could be elaborated and the level of detail increased. But the above questions identify the general grounds on which particular projects can be evaluated, to determine whether the taking of new resources from Federal lands is essential.

INTERPRETIVE PROGRAMS

The goals of preservation-conservation and of interpretation are closely related. On the other hand, there is little justification for spending public monies on the conservation of cultural resources unless there is a social value to the product, that is, unless the resources are actually resources. Interpretation of those resources is the only means to that product both through the enjoyment that citizens obtain through seeing the material remains of past cultures and the education that results from actual interpretation of the lifestyles of prehistoric peoples. Education can be both direct and indirect. Direct education occurs when a cultural resource becomes a part of a display, exhibit, lecture, or publication that is readily available to the public. Indirect education occurs when the resource is used to contribute to understanding the past but in a more mundane scientific fashion; the results are in relatively inaccessible publications.

On the other hand, interpretation is essential to the conservation and preservation of cultural resources. The expenditure of funds that would be required to stop the destruction that is now occurring to sites on public lands because of illegal collecting and excavation would be close to unimaginable and might very well not succeed. Neither will new legislation, however high the attached penalties, cause this criminal behavior to stop. Tighter laws and stricter enforcement will ultimately increase the value of antiquities and the willingness of pothunters to continue their efforts. This is not to say that legislation and stricter enforcement are not partial answers. Indeed, they are necessities. But, there must be positive reenforcement along with the negative. Preservation and conservation will ultimately be based in a concerned local community that sees efforts to protect cultural resources as an integral part of maintaining the community.

Changing a community's attitudes toward cultural resources will not be an easy task. But, given the level of destruction described earlier, a negative attitude toward collecting and pothunting is a cost effective check on those activities. If concerned citizens begin to report such events, the burden falling on the agencies is greatly decreased. If citizen awareness grows that their illicit activities may be reported, they will be less likely to engage in them. Similarly, a citizenry that is aware of the potential benefits of preserving the resources--benefits such as increased visits to the area and prolonged lengths of visits--is more likely to accept the necessity of protection and participate actively in it.

Awareness Program

Goal: To increase local awareness of cultural resources and their value to the local community.

The ability of different agencies to participate in interpreting cultural resources to the local communities varies. The Forest Service has a permanent presence in the area, the Bureau of Land Management does not, for example. Similarly Forest Service lands are contiguous while those administered by other agencies are not. For this reason, the roles that different agencies can play in increasing awareness is different. This discussion is of the overall nature of such a program with the recognition of desirability of interagency coordination as the position and resources of the agencies warrant.

A first step in interpretation is forging a link between agencies and local communities for the benefit of both. This effort should focus on education and can be pursued in a number of different directions. The following specific efforts are suggested:

1. The Forest Service, because its resources are currently better known, should publish a booklet describing cultural resources on the National Forests, interpreting the same, describing their existing and potential value to the community, and mentioning the laws that protect these resources.

The preparation of descriptive material and illustrations for such a booklet should be in such a form that it can be distributed at district offices, at some campgrounds, and to interested local educational groups.

2. A slide and tape program should be developed. The necessary slides are already on file. Thirty and sixty minute talks to accompany the slides could be taped. The program could be a cooperative effort between federal and state agencies.

3. Contacts should be initiated with local schools, church groups and service clubs and talks to these various groups scheduled.

The potential in this area is almost limitless. I have talked about the archeology of the area in forums ranging from service clubs to priesthood meetings. There are enough different educational, religious, and civic groups in the vicinity of this overview area that a schedule of one or two talks a week is not an impossibility. Given that the program is updated each year, this program could be continued indefinitely. Its implementation would require either full time assistance of someone with public education skills or training one or more individuals in handling the program. Alternatively, the taped talk could be used for the verbal portion of the program.

Additional attention should be given to schools in the area. Segments of the social studies curriculum deal with local history and prehistory and with American Indians. At these points in the curriculum, agencies can provide major assistance in enriching the education of local students through the presentation of talks, the loan of artifactual materials, arranging visits to sites, and providing booklets on local prehistory. Efforts in the schools should be given high priority--the education of the next generation is a more productive path to protecting resources than changing the behavior of this one.

In the case of service clubs a somewhat pecuniary addendum is in order: to the extent that the resources are preserved and developed along some of the lines to be discussed later, the community will benefit economically.

4. A program of weekly or monthly press releases to local radio stations and newspapers should be initiated.

By the end of 1981, a wealth of digested material on the cultural resources of the overview unit will be available. At regular intervals, short (100 to 200 word) stories discussing a specific aspect of local prehistory could be released. (I once wrote weekly columns for the Winslow newspaper.) The media are generally willing to publish the information. To the extent that this effort can be regularized into a weekly/monthly archeology column or talk, its impact will be further increased. Since a "local presence" is not so essential to this activity, it might be handled by the Bureau of Land Management.

5. Agencies should foster the development of local archeological societies.

Whetting local interest in archeology without providing a means of satisfying that interest would be a mistake. An immediate means of providing a way of actually involving local citizens in archeology is founding a chapter of the Arizona Archaeological Society. This organization currently has chapters in a number of cities and towns throughout the state. Its members are active in visiting sites, and have been involved in field work both on a paid and volunteer basis.

One or more local societies would, on the one hand, provide a group of concerned local citizens with which a variety of cultural resource efforts could articulate, and on the other hand, a pool of manpower of a variety of different tasks that might be undertaken. Direct involvement could involve the forest archeologists playing a guiding role in the founding of the society(ies), the provision of meeting facilities, the use of sites on the National Forests for some of the early field trips, and, possibly, for field work training and experience as has been done on the Coconino National Forest.

Display Program

Goal: To provide brief visual interpretations of prehistory to visitors.

1. The agencies should produce a poster concerning archeology and cultural resources for display at district offices, campgrounds, and other appropriate public locations.

This program is intended as a quick-and-dirty means of generating a display program. It would consist of a silk-screen, multi-color poster with illustrations of a few interesting artifacts from the overview unit and three messages: (1) a brief summary of local prehistory (200 words), (2) a discussion of the value of cultural resources (100 words), and (3) A warning concerning the illegality of collecting (50 words).

2. A series of roughly 1 meter by 1 meter display boards for use at district offices should be produced.

These displays are intended as more sophisticated versions of the posters. Rather than illustrations, reproductions of artifacts would be attached to a solid wood background. The prose could be somewhat more extensive than that on the posters. Still, it should be possible to produce them for not more than $25 to $50 each.

3. The agencies should produce a set of "archeological columns."

I use the jargon for want of a better term to describe the display I have in mind. Basically, it consists of a wooden box 1 meter on each side and 2 meters high. Two sides of the box are flat panels. On these sides there are prose descriptions of local prehistory on one side and of some specific aspect of the location where the column occurs on the other. The specific discussion might focus on a nearby site, the nature of the prehistoric occupation of a particular canyon or district, or simply on some interesting aspect of regional prehistory--the earliest corn cobs, the abandonment of the National Forest, etc.

The other two sides would be sealed cases. In one there would be a diaroma showing a reconstruction of one of the more interesting sites in the vicinity. In the other, there would be reproductions of chipped, ground stone and/or ceramic artifacts along with some interpretation of them.

This proposal is the heart of the display program. It is intended to be completely flexible. The columns could be located at district offices, in campgrounds, outside Post Offices, along highways--anywhere that made sense. The intention is to fit the specifics of each column to the location where it is found.

A column at Correjo Crossing, for example, would describe the importance of the homestead there in local history. One at the I-40 rest area near Hibbard could focus on Chevelon Ruin, right across the Little Colorado River and in clear view from the rest area. One at the Forest Supervisor's office would more likely deal with National Forest prehistory in general. Columns could occur singly or in groups. An excavated site might have several columns interpreting it and illustrating the work that was done there. Initial construction costs should be no more than $300 to $500 per column and maintenance costs, save for some inevitable vandalism (a factor for which local community interest will again be important), minimal. Once a dozen or more columns existed, it would also make sense to distribute a mimeographed sheet of their locations so that visitors could spend a Saturday or Sunday "touring" the area's archeology as captured in the columns.

Interpretive Archeology

Goal: To develop a series of archeological parks that the public can visit for their education and enjoyment.

Discussing a large scale excavation-for-interpretation program taxes current understanding of the cultural resources and is difficult without at least some crude notion of likely funding levels. For this reason, my suggestions pertain more to broad principles rather than to specific work at specific locations.

1. The agencies should undertake an inventory of the land lying within 300 meters of major highways and forest roads. The survey should occur along 40 miles of roadway in situations where archeological site density is likely to be high and land-use factors indicate suitability for hiking, camping, etc. The specific 40 miles should be selected so as to provide maximum possible dispersion over the overview unit. Survey should not be done in segments of less than .25 miles in length. The purpose of the survey is to provide an inventory of archeological sites in easy-access situations. This information is unavailable at present. Some high quality sites near roadways are known (e.g., site 203 and the "fort", both along the 504 road). There are other sites with great interpretive potential that are substantial distances from roads (e.g., Stotts Ranch, Bear Ruin, East Lincoln Ridge, Homolovi II).

Reasonable decisions must be based on a balance of archeological potential and fiscal reality. Frequently, this will involve comparing similar sites at varied degrees of access. The survey would provide the data basis from which assessments could be made. "Typical" sites would be selected in easy-access locations. A few sites with particularly difficult access problems but with high interpretive value would be included in the plan. (While this survey is discussed here in respect to interpretation, it has a high priority in both administrative study and protective proposals discussed elsewhere.)

2. Interpretation should focus equally on many different aspects of archeology.

Archeological exhibits saturate the interest of the curious when they become monotonous. This is most likely to occur when displays are all of the same type--all excavated rooms for example. To avoid this problem, displays should have a number of different foci including, but not limited to:

(a) excavation--when possible visitors can be directed to sites where they can watch ongoing excavation.

(b) survey--a transect-size area is fenced and the visitor is challenged to find the sites, fill out a sample site form, etc.

(c) vandalism--a particularly badly potted site could be used to show what pothunters destroy that archeologists can learn from.

(d) site formation processes--the descriptive material at and the tour of, the site focus on how the site came to be as it is. The depositional and post-depositional processes that created the site are illustrated.

(e) settlement patterns--a walking tour along trails through an area of dense but unexcavated sites to provide an understanding of inter-community patterns.

(f) excavated sites--to show architecture, activities, etc.

3. Interpretation of sites should encompass multiple-use goals.

The goals of educating the public concerning cultural resources and multiple use management should be amenable to mutually reinforcing display strategies. Each prehistoric family was involved in a multiple use approach to the resources of the area in a way that the typical modern family is not. Most modern families utilize the area in very limited ways--for recreation, for Christmas trees, for grazing the family herd. They are not dependent on nearly so wide a range of resources as were prehistoric peoples.

It is only the agency that is in a position to view the entire set of resources and to act for effective resolution of competing use needs in the same way that prehistoric families did. A prehistoric family needed wood for fuel and for construction, but cutting the juniper and pinyon trees reduced the availability of food stuffs and, in some cases, may have changed the climatic regime. In summary, discussions of how prehistoric families met their resource needs may be an effective means of explaining multiple use strategies to the public.

At the same time, such an approach can help to directly and indirectly educate the public concerning cultural resources. First, the very use of the analogy is a means of educating the public to the potential importance of studying the past: at least some prehistoric peoples did mismanage their resources and had to abandon the areas where they lived. Second, specific cultural materials could be used in illustrations.

This approach could be implemented in a variety of ways. Pamphlets could be written that describe the history of multiple-use of public lands from earliest prehistoric to modern times. Archeological sites could be moved to, or reconstructed in, multiple-use demonstration areas. Finally, descriptive material in all archeological exhibits should make reference to the multiple-use concept.

4. Excavation and restoration should be directed to low maintenance products. Self-guided walking tours, sufficient to allow the handicapped access and at least occasionally specifically directed to particular handicapped groups, should be the norm at all exhibits. When camping facilities are associated with the exhibits, they should be pack-it-in, pack-it-out facilities. Displays should be archeological columns as described earlier.

5. There should be some provision for seasonal supervision of the sites. Interpretation would be greatly aided by two or more archeologically trained seasonal employees who spend portions of each week at different sites giving talks, answering questions, etc. These same employees could provide campfire talks at the larger campgrounds.

6. There should be local involvement in the planning, development, and operation of the interpretive program. The interpretive program is a community resource. If it succeeds, the increase in tourist dollars in the area will be substantial. Beyond economics, local citizens and their guests will be frequent visitors at the facilities. Finally, the public is an expert advisor as to what the public would like to see in such sites. One recent evaluation of the characteristic attitude of visitors to archeological and historic exhibits is that they are bored (Leone 1978). This comment taken in conjunction with the rapidly increasing rate of visitation suggests that the public wants more from such exhibits than it is currently getting.

After planning, volunteers of time and resources can greatly assist in excavation and development. Later, volunteers could carry on demonstration excavation programs and even serve as docents for particular exhibits. Finally, volunteers and an interested local community can provide the ultimate protection for those exhibits.

7. Interpretive development should be done at an interagency level. The development of major interpretive exhibits should be primarily a National Forest responsibility. A great potential would be lost, however, were there not some interagency cooperation, specifically between the Apache-Sitgreaves, Tonto and Coconino National Forests and the BLM. Four of the

largest and most interesting sites in the area are near Winslow (Nuvaqueotaka and Homolovi), Payson (Shoo-fly), and Heber (Stotts Ranch). Developing these sites with a degree of coordination so as to facilitate a driving tour of the archeology of the area would enhance the interpretive value of each. The nascent "Hopi Origins Project" has a similar potential for cooperation between the forests and the Bureau of Land Management.

ADMINISTRATIVE STUDIES

There are undoubtedly effective means of managing cultural resources that are not described in this document because they have not been tried. Administrative studies are necessary in a number of areas to improve management strategies.

1. Low density artifact scatters. For reasons discussed earlier, a fuller understanding of low density artifact scatters could have immediate benefits to management activities.

2. Site signature study. The agencies now are in possession of high quality air photos. It is essential to determine as quickly as possible the potential utility of these in locating cultural resources. An appropriate procedure involves using a stereo viewer to find esoteric vegetation or soil patterns that may represent archeological site "signatures." These locations are then checked for "ground truth," to determine which signatures are false and which are in fact indicative of sites. Control of typical site signatures in an area--and they cannot always be found--is a means of quickly estimating the likelihood of finding resources in a particular project area.

3. Site formation processes baseline. Quite apart from specific human impacts that result in the deterioration of the quality of the archeological record, there are a variety of continuing unpreventable natural impacts, such as trampling by herd animals, excavation by rabbits, badgers, coyotes, etc. In discussing the impact that a particular project has on a resource, it would be very useful to have some standard other than "the pristine archeological site" with which to compare a probable impact.

To establish such a baseline, it would be necessary to generate information on roughly 100 randomly chosen archeological sites in the study area. The sites should be in a variety of different locational contexts (both cultural and natural) and should have suffered a variety of obvious previous impacts. Low level air photos of the sites requiring probably 10 hours of helicopter time could be used to generate site maps and for an overall assessment of current major impacts. Roughly 1 day of collecting at each site using a formal grid system would provide a basis for a baseline characterization of the artifactual materials. In addition, some artifacts would be field analyzed and left in situ.

Periodic studies at a sample of these sites each year in 10 year intervals would provide a relatively continuous monitoring of the impacts the sites suffer. Given the continuation of the study for several decades, it is likely that land modification and other projects will be carried out in their vicinity, allowing a comparison of a great range of different impacts.

4. Site surface renewal. A few sites in the study area have been collected several different times within the last 100 years. Partial collections have been made from over 2000 sites. The rapidity with which the surface of sites is renewed is an important consideration in evaluating impacts. If the artifacts that occur on the surface of a site at some point in time are a subset of all those that have ever been there and the set that contains those which will be there in a decade, then protection against surface impacts is a significant consideration. Alternatively, if the surface of a site is "renewed" at a sufficient rate that the same quantity and types of artifacts endure over long periods of time, then protection against only the most extreme impacts is warranted.

Recollecting a sample of already collected sites and testing to see whether a variety of inferences that might be made using surface materials have changed or remained the same is a beginning. Close articulation between surface renewal studies and the studies described in item 3 are, in the long run, a source of more sophisticated information that may reduce the preventive actions that need to be taken in the face of particular impacts.

5. Sites and fires. The probability is quite high that most archeological sites found on the National Forests have been burned over by a forest fire at least once. The effect of burning on sites in unknown. Yet, that burning may have seriously affected the quantity and quality of materials available on sites.

Bone, for example, is present in subnormal quantities on sites on the National Forests. Yet, there is no obvious characteristic of soil chemistry or hydrology that explains the poor preservation that has been observed. Periodic forest fires may be the cause.

This issue can be addressed by three administrative studies. (1) Excavating sites in an area immediately after a major fire. Especially when some parts of a site have been impacted more than others, the extent of degradation of the archeological record by the fire can be estimated. (2) It is justifiable to use some sites, partially excavated in advance, in areas where slash is to be burned, to begin to understand this impact. (3) Sites could be "built" and then burned.

6. Juniper pushes and animal habitat. Juniper pushes are justified on the grounds that they increase the quantity and quality of grass for animals. The direct impact of Pushes on archeological sites is alleviated when boundaries are shifted to avoid sites. If, as a result of a push, carrying capacity is increased and animals move to the remaining vegetated areas for shade, the indirect impact on archeological sites in the vicinity of the push may be substantial. Archeologists recognize the great destruction that occurs on sites where the density of cattle is high--sherds are very small, chipped stone is characterized by "cow retouch." A systematic before-after study of sites in the vicinity of pushes would help to resolve this issue. There is no reason to believe that pushing would become so overwhelming an impact through greater animal densities so as to make it inadvisable. However, wider boundaries around cultural resources might be warranted.

7. Sampling dispersed parcels such as those managed by Bureau of Land Management is a problem for the reasons discussed earlier. Before a full Phase II survey of the overview area is undertaken, useful information could be gained by comparing the relative results of the two approaches suggested earlier. A sample of about 15 parcels for each strategy should be used. Once the sample is done the predictive power of the two approaches could be compared, by surveying the entire parcel and examining the relationship between the sample and the population.

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BAE - Bureau of American Ethnology.

CARP - Chevelon Archaeological Research Project.

DOA - Department of Anthropology (used with any university).

FMNH - Field Museum of Natural History.

MNA - Museum of Northern Arizona.

Ms. - Manuscript on file at (used with any location).

NAU - Northern Arizona University, Flagstaff.

SAA - Society for American Archaeology.

SWAE - Southwest Archaeological Expedition.

UCLA - University of California, Los Angeles.

UNM - University of New Mexico.

UOA - University of Arizona.

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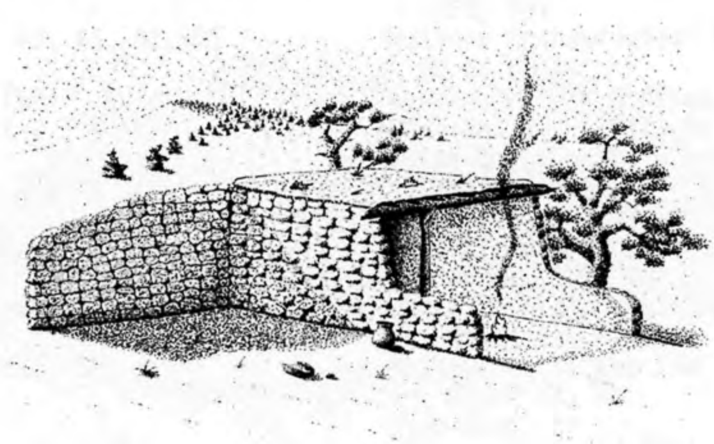

Artist's reconstruction of Puebloan "carport" site.

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