PREHISTORY AND HISTORY
IN THE SOUTHWEST

Collected Papers in Honor of Alden C. Hayes

Contributors
Helen G. Blumenshein
Thomas J. Caperton
Bertha P. Dutton
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Natalie B. Pattison
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John D. Schelberg
Albert H. Schroeder
John P. Wilson
Thomas C. Windes
Arnold M. Withers

THE ARCHAEOLOGICAL SOCIETY OF NEW MEXICO: 11
Edited by Nancy Fox
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**AWANYU**

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PREFACE

This, the eleventh in the Archaeological Society of New Mexico's series of Papers, honors one of its most valued friends and a distinguished member of the archaeological profession. His contributions to anthropology have been numerous, his work in the field enhanced by his many scholarly publications. His career has spanned both ethnography and archaeology, has led him from northern Mexico to Tennessee to Nevada, and has been highlighted by his long association with the National Park Service at Mesa Verde, at Chaco Canyon, and at Gran Quivira.

He has been a trustee and a long time friend of the Society. We are proud to dedicate these papers to him, reflecting some of his many and varied interests, contributed by colleagues who hold him in deep affection and esteem.

The Society expresses its thanks to Claudia Hubbard, who offered many helpful suggestions during preparation of this volume and assisted greatly with the proof reading.

July 1985

Nancy L. Fox
The year was 1918 when a two year old infant saw New Mexico for the first time. He came with relatives to visit an uncle who was a health seeker on a ranch near Carlsbad. The little boy was Alden C. Hayes, born January 11th, 1916, in Englewood, New Jersey. Today he is one of the Southwest's most productive and distinguished anthropologists.

Hayes, a soft spoken, quiet man, is overly modest concerning his many accomplishments. But he has earned the respect, admiration, and affection of the many who have had the good fortune to be associates and/or friends.

It might also be said that Alden seems to fit the description of what people seem to think an archaeologist "should look like." The first time I became aware of this was many years ago at a Pecos conference of Southwestern archaeologists being held at the Research Center of the Museum of Northern Arizona. I was standing near a group of students from one of the ongoing field projects when I heard one of them say, as she pointed toward Al, "I wonder who that man is? He looks like a real archaeologist."

One of her two male companions replied, "Well, I'll bet you're right. He has to be, because he looks like one."

My association with Al Hayes spans nearly a half century. The first time I became distinctly aware of him was on an August evening in 1935, in the Jemez Mountains of New Mexico. The University of New Mexico was conducting a field training program, excavating at Giusewa and San Jose de los Jemez Mission complex, now a state monument. Alden and some fellow students were engaged in an Indian war dance of some kind. He had a bandana tied around his forehead, with a feather sticking up in back. His choreography must have had something of an extraordinary quality, for he is the only one of the participants I remember, although I knew all the students in that summer camp.

Al recalls that he first became interested in aboriginal Americans when the Hayes family lived on the outskirts of Missoula, Montana. He was fascinated by the Salish Indians who came down from Flathead Lake in the summer. They would pitch a ring of tipis about a mile from the Hayes home, spending their time digging bitterroot. Their long braided hair, beaded moccasins, and big black hats topped with eagle feathers left a vivid, lifelong impression.

At age 17, Alden went to one of Ernest Thompson Seton's summer camps. He had planned to go on to forestry school, but at Seton village he met Dr. Edgar L. Hewett, Head of the Department of Anthropology at the University of New Mexico and Director of the Museum of New Mexico in Santa Fe. Through this chance meeting with Hewett, Alden went to the University of New Mexico to become an anthropologist. His choice was lucky for all of us, for Hayes has had a tremendous impact on Southwestern Indian studies, as well as on the lives of his peers.

Concerning the direction he chose to follow, he credits teachers with whom he studied in the Department of Anthropology: Donald Brand, Leslie Spier, Florence Ellis, and W. W. Hill.

At first he hoped to become an ethnologist, but instead he became more and more involved with archaeology. His keen appreciation of contemporary Indian cultures has obviously aided him greatly in the meticulous interpretation of the excavated materials he has researched throughout his career.

Following Hayes' introduction to academic field work in 1935—augmented by classes in Southwestern studies—he next went to Chihuahua with Brand in 1936. He was also a student in the University of New Mexico's Chaco Canyon field school.
during 1936-1937. In years to come, the Chaco was to become for him a major area of research.

Hayes next served as an assistant to Anne Cooke Smith, who was conducting ethnographic field work among the Utes, Goshiutes, and Shoshones of Utah and Nevada. These studies were conducted in 1936 and again in 1939.

With Wesley Bliss and Douglas Osborne, he served as an archaeologist on the Mackenzie Expedition of 1937-1938.

Alden Hayes received a Bachelor of Arts degree in Anthropology from the University of New Mexico in 1939. To quote him verbatim, "And never any more education—I'm one of the last of the uneducated archaeologists." This statement is indeed an oversimplification, for Alden had already acquired more field experience, as well as knowledge of aboriginal American life, than many gain in decades.

Following graduation in 1939, Hayes became an assistant at the Hatchell Site, a Caddoan temple in Bowie County, Texas. This project was conducted for the Texas State Archaeological Survey. Not all of Hayes' early professional career was spent in the Southwest, however. In 1940-1941 he was excavating burial mounds in Watts Bar Beach of the Tennessee River for the University of Tennessee.

During the period 1941-1956 Hayes was a rancher in Cochise County, Arizona, with time out to serve in the United States Army during World War II and in Korea. He entered the army as a private but left as a major.

One of Alden's most productive periods began when he decided to enter the National Park Service in 1957. In that year he became Ranger-Archaeologist at Casa Grande Ruins, Arizona. The following year, his long association with Mesa Verde National Park began when he became Supervisory Archaeologist on the Wetherill Mesa excavations and research project. This lasted until 1965. The time spent in Mesa Verde resulted in Hayes becoming one of the leading authorities on the prehistory of that area (as reflected by his bibliography).

When his work in the Mesa Verde National Park ended, he began the excavation of Mound 7, a large house-block on the site of Las Humanas (Gran Quivira) for the Southwest Archaeological Center, Globe, Arizona. His research proved to be of considerable significance because of the light it shed on New Mexico's Indian-Spanish Contact Period. Some of the finds made during 1965-1968 were kiva mural paintings, the first discovered from this part of New Mexico. The careful excavations of architectural features in the communal building produced concrete evidence of a retreat, if not the actual living quarters of Fray F. Letrado, the priest who for a short time exerted some influence on the Las Humanas people. Excavated materials, especially Tabira Polychrome pottery, shed significant light on the livelihood and art of the inhabitants.

On the death of Mrs. Jean Pinkley, Hayes completed her excavation and repair of the large church and monastery complex at Pecos National Monument. This project took all of his time during 1968-1971.

In 1971 Alden Hayes became one of the most valued and experienced members of the Chaco Research Center, headquartered at the University of New Mexico in Albuquerque. Research in the Chaco consisted of both survey and excavation, although time was also devoted to laboratory work and to the written word. He was on the staff of the Center until his retirement from the National Park Service in 1976.

Alden Hayes was married to Gretchen Chapin in 1941. She was also an anthropologist, whom he met at the University of New Mexico. This marriage ended with her death in 1982. There are two sons, Eric and Mark Hayes, and two grandsons.

While on safari in Africa in 1983, he met Karen Chalker, whom he married in March, 1984. They make their home in Portal, Arizona. However,
much of their time is spent in travel, since they share a common interest in animals, nature, and outdoor living.

Al's field and laboratory work are only part of the contributions he has made for nearly half a century. His monographs and other publications are found in reference libraries in universities, anthropology departments, and museums all over the Southwest, as well as other parts of the United States. Although he would be the last to admit it, he is one of the leading authorities today on many facets of Southwestern anthropology. He has shared his knowledge and enthusiasm with all of us for many years.

Thinking of Alden Hayes with the affection all of his friends and associates share, some words of the late Dr. Alfred V. Kidder, Senior, come to mind. Kidder was summarizing the personality of his longtime friend, Earl Morris: "And above all, he had integrity and was the most dependable man I ever knew."

I can think of no better words than those to describe Alden Hayes' most admired qualities.

The Archaeological Society of New Mexico is proud to honor him with this volume in recognition of the major role he has played in the creation of a better understanding of the cultural history of New Mexico and the Southwest.

Santa Fe, N.M.

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INTRODUCTION

Archaeologists are asking more sophisticated questions than they once did, and they are in need of better analytic techniques for bridging the gap between archaeological material remains and their theoretical concepts (Schaafsma, 1984; Raab and Goodyear, 1984). Respecting Raab and Goodyear's point (1984), this is not a discussion of "middle-range theory." Sullivan has contended that "Material remains are not data. They are phenomena which archaeologists manipulate to generate data" (1978, p. 189). The following is a discussion of techniques for developing more powerful analytic tools to generate the data our research questions demand. The specific focus is on the incorporation of geological materials into our analyses, especially as this applies to southwestern research.

Experience has shown that these more powerful techniques can generate "data" that are far more than we can sensibly make use of in descriptive reports. One does not mindlessly generate mountains of statistics documenting how many of which kinds of analytic units lie latent within a given set of archaeological materials. Rather, one has to report the general kinds of material that were recovered, and then utilize these more fine-grained units in testing hypotheses to which the archaeological materials are relevant. More powerful techniques demand more powerful hypotheses, carefully crafted to specify the data needed to resolve and test the hypotheses that are advanced. The days of simple description are over, and the development of analytic techniques now available underscores the fact that an archaeologist has to do far more than merely weigh, measure, tabulate and report how many of which kind of thing came from an archaeological site. This is not a discussion of how to describe archaeological materials; it is a discussion of how to generate culturally meaningful units relevant to a sophisticated, problem-oriented analysis of archaeological materials.

In part the interaction now possible between geology and archaeology is the result of advances in the field of mineralogy that allow the highly specific identification of rock types, especially as these apply to locating geological proveniences of rock and mineral types (Earle and Ericson, 1977). In part, it is due to archaeologists asking questions that can be illuminated by analyzing the movement of geological materials in the context of prehistoric trade networks and other kinds of social networks. It also has to do with archaeologists asking more sophisticated questions of individual sites (Hill, 1970).

The recent literature has contained much material on geological sourcing in Mesoamerica (Moholy-Nagy et al., 1984) and the tracing of trade routes in Mesoamerica. The Southwest has generally lagged behind other areas. However, important efforts along these lines have been made for the past 50 years or more, and wherever possible illustrations will be selected from this older literature—which still pertains. Many of the illustrations are drawn from the writer's work on Archaic and Navajo sites at Abiquiu Reservoir (Schaafsma, 1975; 1977; 1979a). There seems little reason to doubt that the careful analysis of geological materials occurring in archaeological contexts will be as useful in archaeology as ceramic analysis has been in the past. As Montgomery stated: "The mineralogists can help in solving certain archaeological problems involving mineralogical materials. Closer cooperation between mineralogists and archaeologists can benefit both geology and archaeology" (1963), p. 48).

5
IDENTIFICATION AND DESCRIPTION

The first step in the analysis of geological materials from a site would be an accurate identification of the rock and mineral types represented. These would all be materials that either clearly were or are thought to have been meaningful components in the functioning socio-cultural system that once existed at a study locality (site or region). The selection of materials is determined by the archaeologist and is either holistically selected to describe the range of materials overall, or is selected with some particular problem focus. At any rate, the geologist is presented with a collection of materials for identification, much as a botanist would be presented with a series of flotation samples. The identification and description of geological materials should be as detailed as possible. The exact methods and procedures would have to be worked out separately for each case, but ideally the procedures would be fine enough that each category could be confidently correlated with a specific source locality. In some cases a visual inspection is probably adequate, as in the case of fibrolite from the Truchas Peaks (Montgomery, 1963) and fine-grained basalt from Arroyo Hondo (Schaafsma, 1980, p. 94). In other cases an elaborate chemical analysis would be needed (Earle and Ericson, 1977; Moholy-Nagy et al., 1984). This phase of the work is straightforward mineralogy adjusted to the needs of the archaeological problem. At this point it is enough to note that mineralogical methods exist to handle most of the problems with which archaeologists are faced.

LOCATION OF GEOLOGIC PROVENIENCES

Once the relevant set of geological materials in a study area has been identified, one is in a position to begin seeking the sources of these materials. Again, this is a variable problem that would have to be adjusted to the needs and capabilities of any analysis. The ideal would be to locate the exact (or as nearly as possible) deposits from which the materials were obtained. This is an involved problem and one best done by someone well acquainted with the geology of the region. Helene Warren has done a great deal of work along these lines. Montgomery's work (1963) has been mentioned above. Long ago, Kirk Bryan did a good deal of work to locate sources of prehistoric geological materials and quarries (Bryan, 1938). Obsidian is particularly well suited to sourcing studies (Findlow et al., 1975; Griffin, 1969; Moholy-Nagy et al., 1984). Evidently the fine-grained basalt in Arroyo Hondo (Taos County) comes from a single quarry several miles east of the site (Schaafsma, 1980, p. 94).

It is at this point that an archaeologist would have to re-adjust and fine-tune his research strategy and modify the guiding research questions that control his analysis of the material. The geological materials would to some extent be "inductively" provided information. They could not be known beforehand, although one might have made some informed guesses. Information provided by the geologist might well indicate which directions subsequent research should take. Data relevant to some original research questions may simply not be present in the materials under study; conversely, the geological materials may have information that may radically revise the trend of the original research design.

While it is clear that a number of southwestern archaeologists over the years have attempted to search for the sources of geological materials found in sites (Kidder, 1932; Dittert et al., 1961, p. 157; Schaafsma, 1977), few have utilized the techniques now available to mineralogists to obtain the sort of positive identifications that are now possible. Simply put, it is now possible to locate with a high
degree of probability, the sources of many geological materials found in an archaeological site (Earle and Ericson, 1977). One immediate result of this work might be a regional map showing all the various sources of the geological materials from a site, with the relative proportions of different rock types indicated. Such a map would be of considerable value in guiding the subsequent phases of research.

INTER-SITE DISTRIBUTIONS AND TRADE PATTERNS

Once the sources of the geological materials being considered in the site have been identified, it is appropriate to consider how they were moved from their geological proveniences. What socio-cultural mechanisms were at work to extract and transport the materials to the sites where they were found? Are there indications that some villages were economically specialized, and trading local materials? There are a wealth of inter-site questions to be asked once the data base is provided by the observed distribution of geological material sources. In fact the available information rapidly becomes unwieldy and discrete problems must be framed through hypothesis testing to focus one's research (Hill, 1970; Schaal, 1984).

There is no shortage of problems to deal with. Indeed the hypotheses that demand empirical testing with geological materials are growing exponentially these days, and with a disconcerting disregard for empirical verification. Cordell for example, has referred to the plethora of hypothetical reconstructions of Chacoan society in the San Juan Basin, as "a tolerable amount of creative chaos" (Cordell, 1984, p. 325). Without much question, regional trade networks and regional social interaction are receiving more attention these days (Upham, 1982; De Atley, 1980), and the distributions of geological materials are being utilized to obtain data to substantiate, corroborate or refute the various hypothesized trade systems that are being discussed (Scheiberg, 1983). I submit that all of the recent reconstructions of puebloan trade networks and social systems in the Southwest (Cordell, 1984) are in need of major testing with empirical materials of which geological materials are one kind. None of these hypothesized trade networks mean much until they have been submitted to the scrutiny of testing with empirical data (Schaafsma, 1984). Geologists can help us get are feet on the ground in an environment that is high on concepts and low on data.

So far, the most instructive studies have been done in comparing the occurrence of geological materials in a set of sites within a limited local survey region. For a pioneer, but little emulated, illustration of this approach, see the Navajo Reservoir survey report and a chart showing the frequency of lithic debitage by cultural period (Dittert et al., 1961, p. 158). A similar chart comparing lithic material types between sites is in the author's study in the Abiquiu Reservoir District (Schaafsma, 1977). In the Abiquiu study it was found that lithic selection patterns varied between cultures over time to a remarkable degree. It is hypothesized that this empirically demonstrated inter-cultural variability reflects differential access to quarries, participation in different trade networks, and culturally regulated patterns of material type preference.

The integration and careful analysis of information provided by the study of geographic proveniences of geological materials and their temporal occurrence within a region of study should lead to a wealth of information concerning inter-site or regional social integration and trade networks. For one thing, the changes found in the temporal occurrences of various materials at different periods may well illuminate regional contacts and social changes over time. This parallels the results that can be obtained through
a similar study of ceramics (Schaafsma, n.d.; 1979b).

INTRA-SITE DISTRIBUTIONS

While the location of sources and the implications that this has for inter-site and regional relationships is of major interest, it would appear that the examination of geological materials within a site is also of considerable interest. This phase of work depends upon the existence of a firm identification of all materials, and the ability to recognize all major classes by visual or other simple means so that large samples can be processed (Schaafsma, 1977; 1979a). Most of the work past this point would utilize computer statistics and other means of processing large amounts of data. Many of the specific techniques were proposed in 1966 in regard to the analysis of data from Picuris Pueblo (Schaafsma et al., 1966).

Spatial Distributions

Where are the various geological materials found in a site? Are they randomly distributed, or are the distributions patterned in ways that reflect the structure of the extinct socio-cultural system which left them arranged in the archaeological record? In a large, complex pueblo site it is predicted that there will be a great deal of patterning which in turn reflects the functional and social structure of the original village. Techniques for analyzing spatial distribution have matured a great deal in the past decade or so (Whitlam, 1976). These can be used for working with the spatial distribution of discrete geological materials within a site. At Abiquiu Reservoir, computer drawn maps showing the intra-site distribution of obsidian were made in conjunction with the National Park Service inundation study (Schaafsma, 1978). An example of how one can compare the sets of geological materials occurring in features within a site is found in the Cerrito Site (AR-4) report (Schaafsma, 1979a, p. 174). In this instance the distribution data was used to test the hypothesis that all the site inhabitants were selecting rocks from the environment according to the same patterns of preference (they evidently were).

As the AR-4 example indicates, one can readily utilize the geological materials themselves in testing interesting hypotheses. However, for this discussion, the distribution of geological materials within a site is incorporated within a subsequent discussion relating to the combination of artifact types and materials and will not be elaborated further here.

Temporal Distributions

Through the use of stratigraphy and ceramic analysis, one can obtain quite a high degree of relative and absolute temporal control over the proveniences of a pueblo site (Schaafsma, n.d.; 1979b). Most of the rooms, burials, pits, trash levels, etc., can be placed in approximate temporal relationship one to the other. Once this is established, the occurrence of geological materials over time within the site can be examined. The question is: what materials were present during which time periods? Are all materials equally present in about the same ratios during the life of the site, or are there changes over time in the presence or relative proportions of various geological materials? Techniques for handling large amounts of data in this manner were worked out by the writer in 1966 for the analysis of ceramic attributes at Las Madres Pueblo (Schaafsma, n.d.) and subsequently generalized for proposed work at Picuris (Schaafsma et al., 1966). In any long-lived site one would expect there to be many changes in the occurrence of materials over time.

Temporal and Spatial Distribution

This stage of analysis combines the two previous stages and makes things more complex and more accurately a reflection of the real con-
ditions within a site. The basic question is: what geological materials were present where during the various periods of the site's history? At this point in the whole site analysis enough empirical information would be accumulating that one would have to be guided by significant hypotheses. These hypotheses could then be tested with the data base that was accumulating. Mere description could not be the goal of research at this level. At the descriptive level one would be dealing with computer drawn site maps of each discrete material type for each discrete temporal period. In a well-controlled site these temporal intervals could be about 10-20 years in duration. There would also be many material types present. There is therefore the potential of generating a great deal of "data."

As a hypothetical example, it might become apparent that there was a significant clustering of items (ceramic designs, temper types, architectural forms, etc.) which suggests that some given unit of the site served as the locus of some discrete social unit over approximately a 20 year period. The testing of geological materials in their spatial and temporal context might well show that there were significant associations of certain lithic materials with this residence unit during the period in question. The correlation of distinctive lithics with this residence unit would be one more indication that this was indeed a structurally discrete part of the former social system. The procedure used is similar to that employed by Hill in searching through his data for clues to the social organization of Broken K Pueblo (1970, pp. 57-74). The main difference is the utilization of geological materials in addition to ceramic designs, animal bones, fire pit types, and other features he utilized. When one considers that there would be many different separate geological materials to be tested, and these are easily affected by individual or family preferences, different trade partners, differential ownership of lithic sources and so forth, it becomes clear that the distribution of such materials can become a very sensitive means for locating social and functional differences within a site.

Correlation of Geological Materials and Artifact Types

It is through analyzing the information lying latent in the association of geological materials and artifact forms in their spatial and temporal contexts within a site that we should be able to learn the most concerning the structure of the former socio-cultural system. Combining information from artifacts and geological types can give us even more tightly focused analytic units than can the study of material types alone. This is a primary emphasis of the article by Moholy-Nagy et al. who state: "If source determinations can also be linked to culturally-derived constructs such as behavioral typology, their potential for cultural reconstruction can be more fully realized" (1984, p. 116).

By going through the preceding stages it should have become apparent which geological categories are culturally significant and have a taxonomic validity. For example, if two geological materials are similar (red felsite versus brown felsite, for example) but have consistently behaved differently in their statistical distributions through the previous analytic steps, one would feel confident that they were distinct taxonomic categories (that is, they have analytic utility). Of course, if similar materials (e.g. varieties of obsidian) could be confidently associated with different geological proveniences, then their differential taxonomic validity would be assured. In other words, by this time it should be well established which geological categories have culturally relevant taxonomic status and which do not.

The analysis of artifacts follows different analytic routes and is con-
cerned with human modifications of raw materials. It is presumed for this discussion that the typological validity of the various artifact classes has been established beforehand and that the artifact typology (Schaafsma, 1975; 1977) has already been worked out. In the case of a large pueblo site there would be a large number of artifact classes and a large number of artifacts—perhaps several dozens of the former and many thousands of the latter. It would thus be prudent to have the artifact typology firmly under control before this information is correlated with the many geological materials of which the artifacts are made.

In its simplest form, a correlation would be made between artifact types and geological materials to determine which artifact types are constructed from which materials (Schaafsma, 1977). Initially this could take the form of a simple row/column matrix such as the one Dittert et al. (1961, p. 159) constructed, which has the artifacts in the rows and the lithic materials in the columns. However, since this would obviously be done in a computer, the summary output could be presented in any way that was needed. An example of one kind of output is found in the analysis of lithics from Abiquiu Reservoir (Schaafsma, 1977). Basically what is obtained is a correlation of artifact types with material types, which yields a finer typology—of the sort potentially useful for intra-site analysis. At one level of thought, one could view the inclusion of materials in the typology as merely another attribute to be considered which would yield a more detailed and intricate typology. That in the Southwest, any given formal artifact type will have been manufactured from a number of geological materials is clear from a brief perusal of the studies that have considered the materials out of which artifacts are made (Kidder, 1932; Woodbury, 1954; Dittert et al., 1961; Dick, 1965). What has generally not been done is to utilize geological materials as a discriminating attribute to define yet finer artifact classes which can then be tested for differential distributions within a site (or set of sites) to reveal patterns that reflect the nature of the original socio-cultural system. Techniques for generating such classes were developed in the research in the Abiquiu Reservoir district in an effort to test hypotheses relating to Archaic social boundaries (Schaafsma, 1975; 1977; 1983).

As an illustration of the potential usefulness of the above finer types derived from the combination of formal types and geological materials in the analysis of a pueblo site, let us invent a possible situation that might occur, and be discernible through this kind of analysis. It is possible that an artifact class called "end scraper" might be found. A distribution analysis of this class might determine that there were no significant patterns in the occurrence of this type either spatially or temporally over the area and life of the site. That is, the type was randomly distributed throughout the site spatially and temporally (a most unlikely event). At any rate, it is then determined that the end scrapers were fashioned from five different lithic types. The spatial and temporal distribution of each of these minor types is then examined. For example, the distribution of the obsidian end scrapers is checked, then the gray chert end scrapers, then the fine-grained basalt and so forth. This time it is found that the minor varieties have a definitely skewed or patterned distribution.

It might turn out in this hypothetical example that Unit A of the pueblo had 90 percent end scrapers fashioned from obsidian from near Cochiti. In contrast, Unit B had 90 percent end scrapers fashioned from yellow chert obtained from a source south of the Sandia Mountains. Initially, this would serve to discriminate Units A and B within the site and help define the structure of the site.
in the above hypothetical example of the distribution of material types alone, it might turn out that these two areas had been segregated on the basis of other distinctive items found in them (ceramics, etc.). This would provide empirical data for discriminating the several parts of the pueblo. But more interesting would be the implications that such a distribution would have for regional relationships that various units of the village might have maintained. For example, somebody or the whole social group occupying Unit A, must have maintained some kind of contact with people (or at least the area) near Cochiti in order to get the obsidian. A similar contact would be indicated between the people of Unit B and the area or people south of the Sandias. If it were then shown that the obsidian end scrapers in Unit A were only found for about a 30 year period and they were tightly clustered around some habitation area in Unit A, it might suggest that some man had married into the village who grew up near Cochiti, learned how to make end scrapers (and probably many other tools as well) out of obsidian, and regarded this material as sufficiently superior to merit occasional trips to his natal home in order to obtain supplies of the material, which he then used in his new home. An empirical situation such as this would offer fertile ground for theorizing about the social implications. If the overall analysis demonstrates a number of other ties between this part of Unit A and the general area of Cochiti, a good case could be built up for a social linkage of the sort suggested above. This example ought to demonstrate the potential usefulness of correlating geological materials and artifact types, and then searching the temporal and spatial distribution of these items throughout the site.

**SELECTION OF GEOLOGICAL MATERIALS FOR ARTIFACT TYPES**

A second major result that would emanate from a correlation study of geological materials and artifact types would concern the choice of materials for particular artifact types. This aspect of an analysis would not be aimed at intra-site systemic reconstruction, but would concern itself with artifacts as such, much in the manner that artifacts have been dealt with in the past (Kidder, 1932; Woodbury, 1954). It may also be possible to reveal trade in finished artifacts as has been done in Guatemala (Moholy-Nagy et al., 1984). The artifacts are considered out of context and as groups such as axes, manos, and so forth. The goal would be to take a large sample of artifacts and, using summary statistics, find out what materials were used for which artifact classes. Previous work along these lines has shown that specific materials were selected for particular tool types (Schaafsma, 1977; Dittert et al., 1961; Dick, 1965). It would appear that there were preferred materials for every major class of artifact, and that many specialized tools were primarily manufactured out of specific materials from equally specific geological proveniences. Moholy-Nagy et al. state that:

> Available evidence demonstrates that obsidian from different sources was preferred for different types of artifacts... In assessing the properties of materials, stone-knappers made fine distinctions that are now lost to us, or that we are just beginning to discover through experimentation (1984, p. 116).

In a descriptive sense, most archaeologists do this kind of correlation routinely. However, it needs to be done in a more inclusive and dynamic manner seeking to explore the larger implications of these empirical associations. An example of this sort of asso-
ciation is the spiral-grooved axes which occur in late Pueblo sites in the upper Rio Grande region (Kidder, 1932, p. 50; Montgomery, 1963). Kirk Bryan thought the fibrolite of which most of these axes were made originated in the Picuris Range (Kidder, 1932, p. 50). A subsequent geological survey has demonstrated that the fibrolite used in these axes comes mainly from the Rio Medio near Cundiyó and a primary source around the Truchas Peaks (Montgomery, 1963, pp. 34-48). Thus in this instance a specialized artifact form was made from materials obtained from a single locality which can be confidently identified geologically. In the case of the Pecos axes, it is clear that the Pecos people thought it worthwhile to go all the way to the Truchas Peaks, or trade with the people in that area, for this distinctive material. How many more examples of this sort of thing there are, we have only the haziest idea; but it would seem likely that there are many cases where people went to the trouble to obtain specific rocks for particular tool requirements.

To elaborate the case of the spiral-groove fibrolite axes, it should be pointed out that Woodbury identified them in the materials from Awatobi (1954). These axes at Awatobi have a wide temporal occurrence, but there is a predominance of them around A.D. 1650. At Picuris about 1650 there occurs a complementary presence of trade pottery from Hopi (Dick, 1965). The clay used in the Hopi sherds demonstrates that these vessels were actually made in the Hopi villages. The Picuris may have been among those people who were making the spiral-grooved axes of fibrolite (Dick, 1965, p. 162), although they are oddly uncommon at Picuris (Ibid, p. 184). At first glance, it would appear that Picuris axes were being traded for Hopi pottery about 1650. Regardless of what was actually being traded for what, and who was making the axes, it is clear that trade, hence social contact between Hopi and Picuris existed about 1650 (Ibid, p. 162). When one remembers that the Pueblos in general, and Hopi and Picuris in particular, were leading up to a rebellion in 1680, this stone axe and pottery demonstration of contact between these people becomes even more interesting.

It is hoped that the above discussion has suggested some of the potential usefulness to Southwestern archaeology of sophisticated geological analysis. This is a field that appears to be appreciated by many archaeologists at the present time, but is rarely utilized to its full extent. It would also appear that attempts to perform intra-site systemic analyses, such as was done at Broken K Pueblo (Hill, 1970) would benefit greatly from the consideration of geological materials as they illuminate archaeological questions. Inter-site systemic analyses and the study of regional trade networks will also benefit greatly.

In terms of intra-site analysis, the minute taxonomic units provided by the conjunction of each meaningful artifact class with each significant geological type gives a tool for mapping the structure of the socio-cultural system that may ultimately prove as valuable as ceramic analysis. It may even be more valuable, since it illuminates a wide variety of functions, which ceramics do not. It is certain that once the structure of the system as implied by the rock-plus-artifact taxonomic units is compared with the structure implied by a detailed ceramic analysis (Schaafsma, n.d.), we will have a far greater grip on the actual structure of the system, insofar as archaeological materials can illuminate this in the first place, than we would if we ignored the information lying latent in the geological materials.

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Mogollon Red-on-brown was first described by Bradfield as "early red bowls, well polished, sometimes crudely decorated with red lines inside, having a rubbed-corrugated or 'blind' corrugated surface on the outside" (1927, p. 556). This pottery was referred to by Mera (1931, p. 4) when searching for possible ancestors of Three Rivers Red-on-terracotta. The name was first used by Gladwin and Gladwin (1934, Figs. 3, 9), and the first type description was published by Haury (1936a, p. 10).

There seems to have persisted, for whatever reasons, some misunderstanding about the history and the dating of this pottery type. For example, LeBlanc says that the first bowls were painted with red designs on brown backgrounds—Mogollon Red-on-brown—which continued to be made with little change for 75 to 100 years (1983, p. 116). And again:

Mogollon Red-on-brown pottery was produced for about fifty to a hundred years...this color change from Red on Brown to Red on White took place around AD 700 (Ibid., p. 73).

Then Catherine Scott (following Haury, 1936a) says:

This early pottery style, termed Mogollon Red-on-brown was produced for no more than a hundred years...almost one hundred years later, sometime before A.D. 750, techniques were developed to make the painted designs stand out more clearly (Scott, 1983, p. 40. See Withers, 1985).

I think that Brody comments more sanely about the people who made this pottery when he says:

They first painted pottery in about A.D. 700 in a style similar to the painted wares made to the west and south by other Mogollones and by the Hohokam people of south-central Arizona (1984, p. 38).

Using tree-ring dates, it is difficult to project the occurrence of Mogollon Red-on-brown to a time any earlier than A.D. 700. The earliest clear association of this pottery type with a dated structure seems to be House 18 at Harris Village (Haury, 1936b, pp. 61-62) with a date of 708vv (Bannister et al., 1970, p. 64). Here Mogollon Red-on-brown occurs in abundance along with an almost equal amount of Three Circle Red-on-white and a small amount of Bold Face Black-on-white (Haury, 1936b, p. 132). The next earliest and apparently reliable dates are 715vv (Bannister et al., 1970, p. 64) for House 26 at Harris Village, with no information on pottery associations, and House K at Starkweather Ruin (Nesbitt, 1938, pp. 17, 87), dated at 718vv (Bannister et al., 1970, p. 60) with Mogollon Red-on-brown and Three Circle Red-on-white and no black-on-white.

Following these are the best of the early dates for this type: Structure 201A at the Cerro Colorado Site (Wasley, 1959, p. 109) and House 4 at Mogollon Village. The former produced a bark date of 734rB and a cutting date of 737r (Bannister et al., 1970, p. 19), with a Mogollon Red-on-brown seed jar, a Three Circle Red-on-white bowl, and a Whitemound Black-on-white bowl on the floor; the latter had a presumed construction date of 736r (Ibid., p. 49), and floor sherds of Mogollon Red-on-brown as the only decorated type present. It may be of interest that all of these houses are rectangular with rounded corners, except House K at the Starkweather Ruin which is circular with a four post pattern of roof supports.

Of course it is possible to base a chronological picture for this pottery type and for the beginning of the San Francisco Phase on House 28 at Harris.
Village (Haury, 1936b, p. 61), which has been dated at A.D. 624v (Bannister et al., 1970, p. 64). This house is shown to have contained four Mogollon Red-on-brown sherds, the only decorated pottery (Haury, 1936b, p. 132). But, as Haury says, "the showing of Mogollon Red-on-brown in Three Circle Phase houses and in the upper layers of the test is undoubtedly an intrusion of sherds already abundantly spread over the site" (Ibid., pp. 96-98). This is also likely to be true for House 28. We think that these four sherds must be regarded as out of context. The only other pre-700 occurrence is one sherd from the floor of Structure 203 at the Cerro Colorado Site (Wasley, 1959, p. 88). The latest date associated with this house is 691vv (Bannister et al., 1970, p. 48), which seems acceptably close to the arbitrary A.D. 700 date. Others might be tempted by the association of a radiocarbon date of A.D. 340±200 at Bat Cave (Dick, 1965, pp. 21, 62-64) or one of A.D. 620±130 from a nearby site in the Burro Mountains (Fitting, 1971, pp. 5-7).

Cross-dating with Anasazi pottery types offers other clues. On the northwest and northeast edges of its distribution, Mogollon Red-on-brown has been found in good association with Whitemound Black-on-white in the Forestdale Valley (Haury, 1940) and at the Cerro Colorado Site (Wasley, 1959). Haury (1940, p. 82) found this same association at Mogollon Village. These villages were inhabited during the first half of the 8th century (Bannister et al., 1966, p. 30; Bannister et al., 1970, pp. 19, 48). Whitemound Black-on-white had reached the Hopi country by about A.D. 730 (Daifuku, 1961, p. 16; Bannister et al., 1967, p. 40). In the Lupton-Houck area, the Whitemound Phase featuring this pottery type was initially dated by Gladwin (1945) at A.D. 750-800. This dating has been revised by some to A.D. 700-800 (Ferg, 1978, p. 7; Wasley, 1960, p. 36), and it might be tightened up even more to perhaps A.D. 725-785 (Ferg, 1978, p. 136; Gladwin, 1945, p. 37). Most regard the Whitemound Phase as terminal Basketmaker III in the Pecos Classification (Wasley, 1959, p. 174), but the dates would be in the early Pueblo I period in the San Juan Basin (Marshall et al., 1979).

Other associations with La Plata Black-on-white have been reported for Mogollon Red-on-brown (Martin et al., 1952). However the dates and ceramic associations make it abundantly clear that this pottery type was well developed by the early years of the 8th century, and its production continued until at least A.D. 900.

House 2 at Mogollon Village (Haury, 1936b, p. 101) contained Mogollon Red-on-brown at 898r (Bannister et al., 1970, p. 48), and it was probably found in House 5 at the Wheatley Ridge Site at 899r (Ibid., p. 59). It was surely present at this site in House 4 at A.D. 860 (Rowe, 1947, p. 41). This has been responsible for the placement of the end of the San Francisco Phase at about A.D. 900 (Haury, 1936b, p. 116).

But whether or not this time marks the end of the life of Mogollon Red-on-brown is more difficult to pin down. For one thing, there are no guiding tree-ring dates for the 10th century in the Mimbres area, as LeBlanc has pointed out (1977, p. 8). Haury would now seem to favor an even earlier demise of the type when he says that with current tree-ring evaluation, "an age range of about 625 to 850 fits much better" (1976, p. 330). Breternitz says that it lasts "until at least 950" (1966, p. 87). But this date seems to be erased by the revision of the Wheatley Ridge Site dates to pre-900 (Bannister et al., 1970, p. 59). Wheat (1955) skirts this point, and Brody (1977, p. 82) straddles the issue, saying that "Three Circle Red-on-white replaced Mogollon Red-on-brown ... during the ninth century, its production period beginning late in San Francisco times and continuing into the Three Circle Phase" (which would
carry it into the 10th century according to his chart, p. 18).

The Mogollon Red-on-brown pottery found at Snaketown has been placed by Haury in the late Gila Butte and Santa Cruz Phases (1976, p. 328). But my impression is that stylistically, from Estrella Red-on-gray to the ceramics of the Mimbres area, there was a fairly homogeneous design family during the early years of the life of this pottery type.

Synonyms:

San Lorenzo Red-on-brown (Haury, 1936a, p. 6).
Winona Red-on-tan (Colton and Hargrave, 1937, p. 54; Colton, 1941, p. 29; Colton, 1955, p. 4).
Winona Brown-on-red (Colton, 1941, p. 29).

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Withers, Arnold

This paper briefly discusses the ceramics recovered from Pueblo del Arroyo, Chaco Canyon, in the 1920s. The collection is particularly intriguing for the large number of carbon-painted Cibola ceramics found, now designated Chaco-McElmo Black-on-white. A revised description for this type, based on reanalysis, is presented here as an aid to field workers investigating archaeological sites in the San Juan Basin and beyond.

In 1923, excavations by Karl Ruppert at Pueblo del Arroyo, in conjunction with work at nearby Pueblo Bonito, commenced under the auspices of the National Geographic Society. Work continued intermittently at the site until 1926, with approximately half of the rooms excavated in the main pueblo and the clearing of extramural structures (Judd, 1959). In 1925, Frank H. H. Roberts, Jr. and Monroe Amsden sorted and analyzed the ceramics from both sites. Roberts (1927) used this information and that collected from tests at Penasco Blanco and Pueblo Alto in his unpublished Harvard University dissertation on Chaco ceramics. This work is an important contribution, for it is the first to describe in detail the ceramics from Chaco Canyon from controlled stratigraphic contexts. In addition, the study provided the earliest description of a carbon-painted pottery type designated by Roberts as "Chaco-San Juan" because of its similarity to pottery made north of the San Juan River in the LaPlata-Mesa Verde District. Despite this fine early work, knowledge of Roberts' work remained in obscurity until the late 1950s when it was released from the confines of Harvard archives. Because of its unavailability, Roberts' work was not utilized by Hawley (1936) when she produced her "Chaco District" typology. Subsequently, the two typologies were merged (i.e., Vivian, 1959) which obscured accurate definition of the Chaco Cibola ceramic series.

The massive ceramic collections derived from early Chaco excavations failed to survive for present or future analysis. Little was known of the whereabouts of the bulk material from Pueblo del Arroyo until Gordon Vivian's ruins stabilization crew recovered a huge mass of sherds in Room 139 at that site in 1952. A bottle with a note linking the sherds to the Ruppert excavations was found at the bottom of the pile (Robert Lister, personal communication, 1983). Vivian placed the sherds in three 55 gallon drums where they were recovered by the Chaco Center in 1983 and reanalyzed (Table 1).

Roberts' tallies of the original sherd lot from Pueblo del Arroyo (Roberts, n.d.) allow some means of comparison between the initial and present collections (Table 2), but first we must ask ourselves if the oral history for the source of the collection is correct and, if so, is it representative of the original lot? It is known that the sherd drums were stored behind Vivian's office in the old trading post next to Pueblo del Arroyo and that sherds were given away to anyone who wanted some (Robert Lister, personal communication, 1983). Later, probably in the 1960s, the drums were moved to the Mockingbird Canyon dump in Chaco Canyon where they resided until moved to Albuquerque. Although considerable numbers of sherds must have disappeared from the drums over the decades, the drums could not have held them all. It is possible that yet another cache is buried at Pueblo del Arroyo, perhaps in Room 141, adjacent to where this collection was recovered.

Comparisons of the two collections show remarkable similarity and would seem to substantiate the origin of the present sample as coming from Pueblo del Arroyo or a site amazingly like it. License has been taken in
Table 1. Ceramics at the Chaco Center from the 1923-1926 excavations at Pueblo del Arroyo.

<table>
<thead>
<tr>
<th>Ceramic type</th>
<th>Frequency</th>
<th>Percent</th>
<th>Weight (gm)</th>
<th>Percent</th>
<th>Bowl #</th>
<th>Jar #</th>
<th>Ladle</th>
</tr>
</thead>
<tbody>
<tr>
<td>Plain gray</td>
<td>336</td>
<td>1.6%</td>
<td>2856</td>
<td>1.4%</td>
<td>2856</td>
<td>1.4%</td>
<td></td>
</tr>
<tr>
<td>Wide neckbanded</td>
<td>4</td>
<td>T</td>
<td>36</td>
<td>T</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Narrow neckbanded</td>
<td>249</td>
<td>1.2%</td>
<td>2271</td>
<td>1.1%</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Neck Indented corrug</td>
<td>3</td>
<td>0.2%</td>
<td>50</td>
<td>T</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Unclass Indented corrug</td>
<td>8390</td>
<td>39.2%</td>
<td>81340</td>
<td>40.6%</td>
<td>all jars</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Pill Indented corrug rims</td>
<td>51</td>
<td>0.2%</td>
<td>816</td>
<td>0.4%</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Pill-Ill Indented corr rims</td>
<td>210</td>
<td>1.0%</td>
<td>4602</td>
<td>2.3%</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Pill Indented corrug rims</td>
<td>55</td>
<td>0.3%</td>
<td>1305</td>
<td>0.7%</td>
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<td></td>
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<tr>
<td>Mummy Lake Gray rims</td>
<td>8</td>
<td>T</td>
<td>173</td>
<td>T</td>
<td></td>
<td></td>
<td></td>
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<tr>
<td>Unclass rim Filllets</td>
<td>385</td>
<td>1.8%</td>
<td>3173</td>
<td>1.6%</td>
<td></td>
<td></td>
<td></td>
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<tr>
<td><strong>Culinary Subtotal</strong></td>
<td>9691</td>
<td>46.3%</td>
<td>96,522</td>
<td>48.2%</td>
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<td></td>
<td></td>
</tr>
<tr>
<td>LaPlata B/w</td>
<td>7</td>
<td>T</td>
<td>48</td>
<td>T</td>
<td>5</td>
<td>2</td>
<td></td>
</tr>
<tr>
<td>Red Mesa B/w</td>
<td>137</td>
<td>0.6%</td>
<td>1198</td>
<td>0.6%</td>
<td>74</td>
<td>58</td>
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<tr>
<td>Escavada/Puerco B/w</td>
<td>831</td>
<td>3.9%</td>
<td>10,355</td>
<td>5.2%</td>
<td>365</td>
<td>411</td>
<td></td>
</tr>
<tr>
<td>Gallup B/w</td>
<td>1902</td>
<td>8.9%</td>
<td>16,606</td>
<td>8.3%</td>
<td>532</td>
<td>1348</td>
<td></td>
</tr>
<tr>
<td>Chaco B/w</td>
<td>425</td>
<td>2.0%</td>
<td>4604</td>
<td>2.3%</td>
<td>69</td>
<td>356</td>
<td></td>
</tr>
<tr>
<td>Chaco-McElmo B/w</td>
<td>1190</td>
<td>5.6%</td>
<td>10,937</td>
<td>5.2%</td>
<td>594</td>
<td>550</td>
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<tr>
<td>Cibola WW Subtotal</td>
<td>4492</td>
<td>21.0%</td>
<td>43,208</td>
<td>21.5%</td>
<td>1639</td>
<td>2725</td>
<td></td>
</tr>
<tr>
<td>Unclass Mineral</td>
<td>1553</td>
<td>7.3%</td>
<td>10,534</td>
<td>5.3%</td>
<td>574</td>
<td>921</td>
<td></td>
</tr>
<tr>
<td>Plain Whiteware</td>
<td>2941</td>
<td>13.8%</td>
<td>26,663</td>
<td>13.3%</td>
<td>861</td>
<td>2037</td>
<td></td>
</tr>
<tr>
<td>Unclass Carbon</td>
<td>153</td>
<td>0.7%</td>
<td>1103</td>
<td>0.5%</td>
<td>87</td>
<td>64</td>
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</tr>
<tr>
<td>Unclassified Subtotal</td>
<td>4647</td>
<td>21.7%</td>
<td>38,300</td>
<td>19.1%</td>
<td>1522</td>
<td>2958</td>
<td></td>
</tr>
<tr>
<td>Newcomb B/w</td>
<td>10</td>
<td>T</td>
<td>119</td>
<td>0.1%</td>
<td>4</td>
<td>2</td>
<td></td>
</tr>
<tr>
<td>Chuska B/w</td>
<td>128</td>
<td>0.6%</td>
<td>1292</td>
<td>0.6%</td>
<td>68</td>
<td>56</td>
<td></td>
</tr>
<tr>
<td>Toadlena B/w</td>
<td>279</td>
<td>1.3%</td>
<td>2589</td>
<td>1.3%</td>
<td>218</td>
<td>49</td>
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</tr>
<tr>
<td>Nave B/w</td>
<td>12</td>
<td>0.1%</td>
<td>123</td>
<td>0.1%</td>
<td>8</td>
<td>3</td>
<td></td>
</tr>
<tr>
<td>Crumbled House B/w</td>
<td>1</td>
<td>T</td>
<td>30</td>
<td>T</td>
<td>1</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Unclass Chuskan Carbon</td>
<td>169</td>
<td>0.7%</td>
<td>1192</td>
<td>0.6%</td>
<td>94</td>
<td>65</td>
<td></td>
</tr>
<tr>
<td>Chuskan WW Subtotal</td>
<td>599</td>
<td>2.8%</td>
<td>5345</td>
<td>2.7%</td>
<td>395</td>
<td>175</td>
<td></td>
</tr>
<tr>
<td>Kana'a B/w</td>
<td>2</td>
<td>T</td>
<td>11</td>
<td>T</td>
<td>1</td>
<td>1</td>
<td></td>
</tr>
<tr>
<td>Dogoshzhi B/w</td>
<td>14</td>
<td>0.1%</td>
<td>101</td>
<td>0.1%</td>
<td>4</td>
<td>10</td>
<td></td>
</tr>
<tr>
<td>Black Mesa B/w</td>
<td>48</td>
<td>0.2%</td>
<td>411</td>
<td>0.2%</td>
<td>42</td>
<td>2</td>
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<tr>
<td>Black Mesa/Sosi B/w</td>
<td>108</td>
<td>0.5%</td>
<td>540</td>
<td>0.3%</td>
<td>94</td>
<td>14</td>
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</tr>
<tr>
<td>SoSi B/w</td>
<td>82</td>
<td>0.4%</td>
<td>930</td>
<td>0.5%</td>
<td>77</td>
<td>1</td>
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<tr>
<td>Flagstaff B/w</td>
<td>4</td>
<td>T</td>
<td>56</td>
<td>T</td>
<td>1</td>
<td>3</td>
<td></td>
</tr>
<tr>
<td>Tusayan WW Subtotal</td>
<td>258</td>
<td>1.2%</td>
<td>2049</td>
<td>1.0%</td>
<td>219</td>
<td>31</td>
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</tr>
<tr>
<td>Mancos B/w</td>
<td>236</td>
<td>1.1%</td>
<td>2683</td>
<td>1.3%</td>
<td>160</td>
<td>65</td>
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</tr>
<tr>
<td>McElmo B/w</td>
<td>257</td>
<td>1.2%</td>
<td>3124</td>
<td>1.6%</td>
<td>182</td>
<td>62</td>
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<tr>
<td>McElmo/Mesa Verde B/w</td>
<td>122</td>
<td>0.6%</td>
<td>943</td>
<td>0.5%</td>
<td>102</td>
<td>5</td>
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<tr>
<td>Mesa Verde B/w</td>
<td>26</td>
<td>0.1%</td>
<td>477</td>
<td>0.2%</td>
<td>25</td>
<td>1</td>
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</tr>
<tr>
<td>Mesa Verde WW Subtotal</td>
<td>641</td>
<td>3.0%</td>
<td>7227</td>
<td>3.6%</td>
<td>469</td>
<td>135</td>
<td></td>
</tr>
<tr>
<td><strong>San Juan Redware</strong></td>
<td>11</td>
<td>0.1%</td>
<td>63</td>
<td>T</td>
<td>4</td>
<td>7</td>
<td></td>
</tr>
<tr>
<td>Tusayan B/r</td>
<td>9</td>
<td>T</td>
<td>153</td>
<td>0.1%</td>
<td>9</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Tusayan Polychrome</td>
<td>9</td>
<td>T</td>
<td>43</td>
<td>T</td>
<td>8</td>
<td>1</td>
<td></td>
</tr>
<tr>
<td>Unclass Tsegl Orange ware</td>
<td>68</td>
<td>0.3%</td>
<td>447</td>
<td>0.2%</td>
<td>48</td>
<td>20</td>
<td></td>
</tr>
<tr>
<td>Tsegi OW Subtotal</td>
<td>86</td>
<td>0.4%</td>
<td>641</td>
<td>0.3%</td>
<td>65</td>
<td>21</td>
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<tr>
<td>Puerco B/r</td>
<td>59</td>
<td>0.3%</td>
<td>767</td>
<td>0.4%</td>
<td>56</td>
<td>3</td>
<td></td>
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<tr>
<td>Wingate B/r</td>
<td>60</td>
<td>0.3%</td>
<td>756</td>
<td>0.4%</td>
<td>60</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Wingate Polychrome</td>
<td>1</td>
<td>T</td>
<td>11</td>
<td>T</td>
<td>1</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Unclass White Mt Redware</td>
<td>223</td>
<td>1.0%</td>
<td>1522</td>
<td>0.8%</td>
<td>196</td>
<td>27</td>
<td></td>
</tr>
<tr>
<td>WH Mt Redware Subtotal</td>
<td>343</td>
<td>1.6%</td>
<td>3036</td>
<td>1.5%</td>
<td>313</td>
<td>30</td>
<td></td>
</tr>
<tr>
<td>Unclass Smudged</td>
<td>460</td>
<td>2.2%</td>
<td>2751</td>
<td>1.5%</td>
<td>460</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Unclass Brownware</td>
<td>33</td>
<td>0.2%</td>
<td>282</td>
<td>0.1%</td>
<td>32</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Unclass Redware</td>
<td>101</td>
<td>0.5%</td>
<td>721</td>
<td>0.4%</td>
<td>52</td>
<td>49</td>
<td></td>
</tr>
<tr>
<td>Glaze A, Misc Glazes</td>
<td>25</td>
<td>0.1%</td>
<td>311</td>
<td>0.2%</td>
<td>14</td>
<td>11</td>
<td></td>
</tr>
<tr>
<td><strong>Total</strong></td>
<td>21,387</td>
<td>100.0%</td>
<td>200,556 gm</td>
<td>100.0%</td>
<td>5178</td>
<td>15,824</td>
<td>309</td>
</tr>
<tr>
<td>Roberts' types:*</td>
<td>Freq</td>
<td>Percent</td>
<td>Equivalent types in sample (Table 1):</td>
<td>Freq</td>
<td>Percent</td>
<td></td>
<td></td>
</tr>
<tr>
<td>------------------</td>
<td>------</td>
<td>---------</td>
<td>---------------------------------------</td>
<td>------</td>
<td>---------</td>
<td></td>
<td></td>
</tr>
<tr>
<td>plain gray</td>
<td>207</td>
<td>0.3%</td>
<td>plain gray, Mummy Lake Gray</td>
<td>344</td>
<td>1.6%</td>
<td></td>
<td></td>
</tr>
<tr>
<td>plain bands</td>
<td>71</td>
<td>0.1%</td>
<td>wide and narrow neckbanded</td>
<td>253</td>
<td>1.2%</td>
<td></td>
<td></td>
</tr>
<tr>
<td>corrugated coils and incised coils</td>
<td>42,448</td>
<td>54.8%</td>
<td>unclassified indented corrugated</td>
<td>9094</td>
<td>42.5%</td>
<td></td>
<td></td>
</tr>
<tr>
<td>corrugated B/w</td>
<td>43</td>
<td>0.1%</td>
<td>B/w bowls with indented corrugated exteriors#</td>
<td>14</td>
<td>0.1%</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Pre-Pueblo</td>
<td>0</td>
<td>-</td>
<td>LaPlata B/w</td>
<td>7</td>
<td>T</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Transitional and Degenerate Transitional</td>
<td>391</td>
<td>0.5%</td>
<td>Kana'a, Newcomb, Red Mesa B/w</td>
<td>149</td>
<td>0.7%</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Solid Designs</td>
<td>1360</td>
<td>1.8%</td>
<td>Escavada/Puerco, Mancos, Toadlena (1/2) B/w</td>
<td>1060</td>
<td>5.0%</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Early Hatchure and Late Hatchure</td>
<td>6362</td>
<td>8.2%</td>
<td>Gallup, Chaco, Chuska, Dogspzhl B/w</td>
<td>2467</td>
<td>11.5%</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Chaco-San Juan</td>
<td>6614</td>
<td>8.5%</td>
<td>Chaco-McElmo, Sosl, Black Mesa, Flagstaff, Toadlena (1/2), Nava B/w</td>
<td>2100</td>
<td>9.8%</td>
<td></td>
<td></td>
</tr>
<tr>
<td>not determinable B/w</td>
<td>17,205</td>
<td>22.2%</td>
<td>plain whiteware, unclassified mineral, unclass carbon, unclass Chuskan</td>
<td>4813</td>
<td>22.5%</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Foreign (not Mesa Verde)</td>
<td>20</td>
<td>T = ?????</td>
<td>-</td>
<td>-</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Mesa Verde</td>
<td>58</td>
<td>0.1%</td>
<td>Mesa Verde B/w, Crumbled House B/w</td>
<td>27</td>
<td>0.1%</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Early Red Wares</td>
<td>2</td>
<td>T</td>
<td>San Juan Redware</td>
<td>11</td>
<td>T</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Late Red Wares</td>
<td>1076</td>
<td>1.4%</td>
<td>White Mountain Redware</td>
<td>343</td>
<td>1.6%</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Polychromes (Proto-Kayenta)</td>
<td>39</td>
<td>T = Tusayan Polychrome</td>
<td>9</td>
<td>T</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Foreign Reds</td>
<td>51</td>
<td>0.1%</td>
<td>Tsengl Orangeware except polychromes</td>
<td>77</td>
<td>0.4%</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Polished Black Interior</td>
<td>1458</td>
<td>1.9%</td>
<td>unclass smudged</td>
<td>460</td>
<td>2.2%</td>
<td></td>
<td></td>
</tr>
<tr>
<td>?????</td>
<td>-</td>
<td>-</td>
<td>unclass brownware, unclass redware, glazes</td>
<td>159</td>
<td>0.7%</td>
<td></td>
<td></td>
</tr>
<tr>
<td><strong>TOTALS</strong></td>
<td>77,405</td>
<td>100.0%</td>
<td></td>
<td>21,387</td>
<td>99.8%</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

* After Roberts (n.d.).
# B/w corrugated subtracted from other type totals in sample.
Note: Toadlena B/w totals split in half to account for similarity of design to "Solid Designs" and "Chaco-San Juan".

Table 2. Comparison of the Roberts and the Chaco Center ceramic samples from Pueblo del Arroyo.
classifying the present sample in groups comparable to Roberts'. One must be cautious, however, when assuming the comparability of Roberts' types and present typologies. Apparently, Amsden and Roberts sorted primarily on ceramic design without regard to paint type and temper. All three attributes, however, were used for classifying the present sample (Table 1).

Nevertheless, the proportion of Roberts' types agrees quite closely with the material taken from Vivian's 55 gallon drums, but there are some discrepancies which cannot be resolved. The frequencies of plain gray and banded in the present smaller sample exceed those reported by Roberts, while the percent of indented corrugated from the "drum" sample is much less than the original percentage. These are types that are most likely to be sorted the same, so that the discrepancy here is more surprising than had it been for the decorated types. Breakage does not seem to reconcile the differences, because both frequencies and weight show little proportional variation among the same types. The original sorting by Roberts and Amsden, that may have left types clustered when they were reburied, also does not seem to be a contributing factor. Based on the present information about the two samples, the problem must be left unresolved. Finally, there are clearly some aberrant types that are unlikely to have come from Pueblo del Arroyo. These are the mix of glaze sherds which Vivian may have deposited in the drums from other sources.

CHACO-McELMO BLACK-ON-WHITE: ROBERTS' STUDY

The importance of the Pueblo del Arroyo collection for the present analysis lies in the very large Chaco-McElmo Black-on-white sample on which Roberts (1927) based part of his description of "Chaco-San Juan" Black-on-white. Amsden and Roberts tabulated 20,055 "Chaco-San Juan" sherds from Pueblos Bonito and del Arroyo and the resulting descriptions and stratigraphic placement of the type were remarkably perceptive. Thus, it is somewhat perplexing to discover that Roberts' type collection of Chaco-San Juan (Chaco-McElmo) and Mesa Verde Black-on-whites, obtained from the Peabody Museum, reveals a mixture of types from a number of different pottery traditions (Figure 1). In these cases it is clear that design motifs and slip were Roberts' critical attributes for sorting, and not the paint and temper type.

Most likely, Tusayan Whiteware and some Mesa Verde and Chuskan Whitewares, along with some Puerco Black-on-white, were included under Roberts' tallies for Chaco-McElmo. Tusayan Whiteware most closely resembles Chaco-McElmo, yet Roberts (Ibid., pp. 193-194) was aware of their different origins. Some late mineral-painted Puerco Black-on-white vessels duplicate Chaco-McElmo in all attributes except paint type (Figure 1b-c), so it is understandable that they were classified as Chaco-McElmo.

Given the paucity of published descriptions for Chaco-McElmo, it is not unexpected that many archaeologists have had trouble identifying the type from others (Anyon, 1983, Plate 6.1; Bice and Sundt, 1972, pp. 102-120; Lang, 1983, Figure 4.24-25, 4.27-28; Warren, 1979, Figure 55k-5; Windes, 1977, Figure 10.40g) or in placing it with the proper ceramic tradition (Marshall et al., 1979). The potential for refining chronology, particularly for survey, and assessing trade and interaction is lost, or worse, biased, by inadequate knowledge of the type. Chaco-McElmo has the potential for a much wider understanding of prehistoric Chacoan behavior as the terminal Chacoan ceramic type (i.e., Toll et al., 1980) during the Chaco Phenomenon. Chaco-McElmo appears concurrently with the period of major shifts in architecture, ceramics, lithics, and trash disposal that herald
Figure 1. Roberts' type sherd collection from the Peabody Museum. a-i - "Chaco-San Juan" B/W; j-l - "Mesa Verde" B/W. Today, these are designated Puerco B/W (a-c), Escavada B/W (d), Mesa Verde B/W (e, k-l), Black Mesa/Sosi B/W (f), and Chaco-McElmo B/W (g-j). Note rim ticking on a-b, d, h-k. Rim painted solid on g.
Figure 2a. Chaco-McElmo Black-on-white bowl rim sherds from Pueblo del Arroyo.
Figure 2b. Chaco-McElmo Black-on-white bowl rim sherds from Pueblo del Arroyo. Reverse of Figure 2a. Note band of white slip below rims on most sherds.
changes in the social-religious-economic organization of the Bonito Phase and the Chacoan system in the early A.D. 1100s. Thus, it is an important element for study marking the critical period of decline in the Chacoan system.

Gordon Vivian (Vivian, 1959; Vivian and Mathews, 1965) is responsible for renewing interest in Chaco-McElmo after becoming acquainted with Roberts' work, although it is clear Vivian was unsure of its identity and subsequently lumped all carbon-painted ceramics from the Cibola, Tusayan, Chuska, and Mesa Verde Whitewares under a "Chaco Variety" of McElmo Black-on-white. Although Vivian was the current ceramic authority for many investigators working in the San Juan Basin, new sources have helped unscramble the confusion of the Chaco Cibola Series. Happily, Franklin (1980, 1982a, 1982b) and Toll and McKenna (1983) have done much to accurately portray Chaco-McElmo in recent years.

CHACO-McELMO BLACK-ON-WHITE: ITS PLACE IN CERAMIC ASSEMBLAGES

Chaco-McElmo was so abundant at Pueblo del Arroyo in relation to other types that Roberts (1927, p. 240) considered the site a potential manufacturing locus. According to Roberts the outstanding ware of the black-on-white group (at Pueblo del Arroyo) is the Chaco-San Juan. Not only was this true of the material from the rooms, but a large refuse deposit at the west side of the ruin carried this type almost to the exclusion of all others (Ibid., p. 239). In raw numbers it certainly is common, although Pueblo Bonito yielded far more. Its relative frequency in contemporary deposits at Bonito would certainly equal or exceed Pueblo del Arroyo's. Both pueblos might be considered type sites for Chaco-McElmo considering the high numbers found during excavations.

Generally, Chaco-McElmo from Chaco Canyon is associated with large frequencies of Gallup (often the dominant painted type) followed by lesser, and perhaps equal, quantities of Puerco and McElmo Black-on-whites, although during the early A.D. 1100s it may be the most prevalent whiteware type. Nevertheless, despite a reexamination of ceramics on 20 percent of the small houses and all of the greathouse middens in Chaco Canyon since the initial 1970s inventory survey, only a single site (29SJ1939) near Pueblo del Arroyo and Pueblo Bonito yielded dominant numbers of Chaco-McElmo (from three transects totaling 657 sherds). Chaco-McElmo appears during a period of widespread construction and remodeling among small sites and greathouses in Chaco Canyon and at some outliers.

CHACO-McELMO BLACK-ON-WHITE: THE TYPE DESCRIPTION

Franklin's (1982a, 1982b) and Franklin and Ford's (1982) recent work with the Bis sa'ani community ceramics clearly identify a type that closely resembles that from Pueblo del Arroyo. Many of the similarities and contrasts noted below between Chaco-McElmo and other types are supported by results in Table 3.

Chaco-McElmo Black-on-white is placed with the Chaco Cibola White-ware here because of the number of attributes it shares with the Chaco ceramic tradition, particularly with contemporary Chaco Black-on-white. The latter type is compared with Chaco-McElmo to test for similarities to the Chacoan tradition (Tables 3 and 4). The attribute constellation of paste, temper, slip, and some vessel forms (pitchers and cylindrical vessels) mark Chaco-McElmo as of distinctly Chacoan manufacture. Chaco-McElmo is distinguished from other types in the Cibola series primarily on the attributes of paint type (carbon), design, and rim decoration.
Figure 3a. Chaco-McElmo Black-on-white bowl rim (top 3 rows) and jar rim (bottom row) sherds from Pueblo del Arroyo.
Figure 3b. Chaco-McElmo Black-on-white bowl rim (top 3 rows) and jar rim (bottom row) sherds from Pueblo del Arroyo. Reverse side of Figure 3a. Note band of white slip below rims on most sherds.
Designs: The range and popularity of designs found on Chaco-McElmo (Figures 2-8) have been well described by Roberts (1927), Franklin (1982a, 1982b), and Toll and McKenna (1983). The present sample agrees with the other studies in the paramount use of Sosi style wide line decoration and its lower popularity on McElmo vessels. Surprisingly, Franklin found a very low incidence of dot usage, whereas this element is common on Chaco Canyon examples. In particular, the use of dot groups, as fillers within blank space (i.e., parallelograms and checkerboards) or along the edges of thin oblique parallel lines seems common (see Vivian and Mathews, 1965, Figures 39-40).

Other major elements employed on Chaco-McElmo ceramics include sawteeth, often as opposing elements; checkerboards; a series of parallel lines within a band (Ibid., Figure 40) or unbounded (often on bowls and the flared basal part of pitchers); open and negative rectangles (parallelograms); and key figures ending in frets (often interlocking).

Hatched elements are rare in Chaco-McElmo (Figures 3a and 6). Even if the carbon-painted hatched sherds placed with the unclassified carbons were added to the Chaco-McElmo sample, hatched sherds would still comprise 1 percent or less of all the Chaco-McElmo.

Chaco Cibola Whiteware rims are usually painted solid or left unpainted. Rarely, rim ticking occurs on Gallup, Escavada, and Puerco Black-on-whites (Figure 1). The normal Chaco-McElmo pattern, however, is to ornament segments of rim with small ticks (dots), often in groups.

Forms: In the early A.D. 1100s in Chaco Canyon, Chaco-McElmo Black-on-white almost stands alone for its near parity of open and closed sherd frequencies in assemblages with other contemporary painted types. Puerco and Escavada Black-on-whites combined have a similar ratio of forms to Chaco-McElmo in this study. Wares imported from around the periphery of the San Juan Basin or beyond (i.e., Tusayan, Chuskan, and Mesa Verde Whitewares and all redwares) are always principally open (bowl and ladle) forms when found in Chaco Canyon. The preponderance of bowls traded into the Canyon may simply reflect transportation consideration—bowls can be nested to facilitate transport over long distances (Whittlesey, 1974). Jars, on the other hand, typify the Chaco Cibola Whitewares for the same period, except for Chaco-McElmo. Both Gallup and Chaco Black-on-white sherds are overwhelmingly jars and, in the case of Gallup at least, these are commonly fragments of large storage jars, or ollas.

Sherd numbers give us a distorted picture of whole vessel ratios, but there is probably some truth to the proportions they reveal. Although some ollas are classified as Chaco-McElmo, the primary forms of the type seem to be bowls and pitchers (at least at Pueblo del Arroyo and Pueblo Alto). Pitchers are roughly equivalent in surface area to bowls, and thus the dichotomy in forms seems to suggest about equal numbers of bowls and pitchers of Chaco-McElmo with smaller frequencies of ladles, seed jars, ollas, effigies, and nondescript small jars (Figures 7 and 8). Effigies, flat 'plates' (Judd, 1954, Plate 62d-e), and cylindrical jars (Judd, 1954, Plate 67b) occur as unusual forms. Roberts (1927, p. 97), in addition, lists the occurrence of Chaco-McElmo mugs, but these are undoubtedly all Mesa Verde or Chuska Whitewares instead. Rim forms of Chaco-McElmo trend towards tapered, narrow flat rims, although most are tapered and rounded (70% of 168 rims).

Temper: A variety of temper inclusions characterizes Chaco-McElmo (Table 3). Much of the variety, however, may be due to inclusions in sherds crushed for tempering material. Sand (or crushed sandstone) comprises a large minority (23%) of the tempering
Figure 4. Chaco-McElmo Black-on-white bowl body sherds from Pueblo del Arroyo.
materials, but sherd temper is found in the vast majority (68%). Probably a larger frequency than noted (3%) may be unknown and should, in fact, be designated as having no temper. In some cases sand may have been recorded that was not an additive but occurred naturally in the potter's clay (Vivian and Mathews, 1965, p. 69). Of interest is the number of sherds tempered solely with fine sparse fragments of trachyte, a finding documented by others (Toll et al., 1980; Toll and McKenna, 1983). Trachyte also occurs in other classic Chaco Cibola Whites. Trachyte temper, of course, is the primary criterion for identifying Chuskan tradition pottery, although fine quantities of it (often with sherd temper) may be found in both Chaco Cibola and Mesa Verde Whites (Ibid., Windes, 1977). Aside from the lack of trachyte in the Chaco Black-on-white rim sample, probably due to sampling error, there is a close correspondence in temper use between Chaco-McElmo and Chaco Black-on-white in the Pueblo del Arroyo collection (Tables 3 and 4).

Paint: Unlike other Cibola Whites, Chaco-McElmo is decorated with carbon paint. Coincident with the appearance of Chaco-McElmo in the very late A.D. 1000s or early A.D. 1100s, the region north of Chaco Canyon also shifts from mineral to carbon-base painted pottery (i.e., the Mesa Verde Whites). The carbon paint is often a faded dull purplish color (e.g., Munsell 7.5R N4/ or 2.5Y N4/) with indistinct edges. There are a few instances where Chaco Black-on-white exhibits blurry paint. This type of decoration could easily be mistaken for carbon paint (i.e., Franklin, 1982, Figure 205) and, thus, for Chaco-McElmo, although it probably represents an atypical carbon-mineral mixture. A chemical analysis of the paint is needed to resolve this problem.

Slip: Slip application is one of the most distinctive attributes of Chaco Cibola Whiteware, and Chaco-McElmo continues the tradition (Windes, 1984). The thin, white, wash application and the practice of leaving unpainted portions of the vessel unsipped characterizes types in the late Chaco Cibola Whites. The conservative use of slip is such that it is typical for potters to leave the lower area on jars (Figure 8k), ladles (Figure 7b), and bowls (Figure 7c–d) unsnipped. Often a narrow ribbon of slip is applied just under the rim on bowl exteriors and jar interiors (Figures 2b and 3b) or across or around the bottom (Ibid., Figure 6.3). Roberts (1927) referred to this partial coverage as "slip-slop." In the Pueblo del Arroyo collection, Chaco and Chaco-McElmo Black-on-whites are similar in this regard, both in the presence and the mean width of the band on bowl exteriors and jar interiors (Tables 3 and 4).

In some cases the bowl exterior of Chaco-McElmo is so highly polished that it is difficult to distinguish slip from the vessel paste and, with some, no slip is evident. Franklin and Ford (1982, pp. 951-952) use these minor variations of slip and polish to separate out a part of Chaco-McElmo as "Cibola carbon."

COMPARISONS WITH OTHER TYPES

Chaco-McElmo can be confused with a number of ceramic types found within the San Juan Basin. The most prevalent of these are Black Mesa, Sosi, Toadlena, Nava, and McElmo Black-on-whites. The Chuskan types (notably Toadlena and Nava) can usually be distinguished simply on the basis of the dark gray paste packed with fragments of crushed trachyte. The thick, yellow tint slip and the carbon paint that reflects pinpricks of gold biotite are also clues for its separation (Windes, 1977). The greatest problem occurs when trying to separate Chaco-McElmo from McElmo Black-on-white of the Mesa Verde Whiteware, or from
Table 3. Chi-square and T-test results for selected type attribute comparisons.

<table>
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<tr>
<th>Attribute</th>
<th>Comparison</th>
<th>Sample number</th>
<th>(X^2)</th>
<th>DF</th>
<th>P</th>
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<td><strong>CHI-SQUARE RESULTS</strong> (tables derived from Table 4, unknowns excluded).</td>
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<td></td>
<td></td>
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<tr>
<td><strong>FORMS</strong></td>
<td>Chaco-McElmo &amp; McElmo</td>
<td>280</td>
<td>5.6c</td>
<td>1</td>
<td>.02*</td>
</tr>
<tr>
<td></td>
<td>Chaco-McElmo &amp; Chaco</td>
<td>235</td>
<td>12.4c</td>
<td>1</td>
<td>.000*</td>
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<tr>
<td><strong>TEMPER</strong></td>
<td>Chaco-McElmo &amp; McElmo</td>
<td>268</td>
<td>111.9</td>
<td>3</td>
<td>.000*</td>
</tr>
<tr>
<td></td>
<td>Chaco-McElmo &amp; Chaco</td>
<td>223</td>
<td>2.1</td>
<td>2</td>
<td>.35</td>
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<td><strong>SLIP TYPE</strong></td>
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<td>211.0</td>
<td>2</td>
<td>.000*</td>
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<tr>
<td></td>
<td>Chaco-McElmo &amp; Chaco</td>
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<td>0.79</td>
<td>2</td>
<td>.67</td>
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<td>113.1</td>
<td>3</td>
<td>.000*</td>
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<td>Chaco-McElmo &amp; Chaco</td>
<td>173</td>
<td>7.25</td>
<td>3</td>
<td>.064</td>
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<td></td>
<td>jars: Chaco-McElmo &amp; Chaco</td>
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<td>5.9</td>
<td>3</td>
<td>.12</td>
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<td>Chaco-McElmo &amp; McElmo</td>
<td>211</td>
<td>57.3</td>
<td>6</td>
<td>.000*</td>
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<tr>
<td></td>
<td>Chaco-McElmo &amp; BM/SoS1</td>
<td>208</td>
<td>18.9</td>
<td>6</td>
<td>.004*</td>
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<td>Chaco-McElmo &amp; McElmo</td>
<td>214</td>
<td>1.6</td>
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<td>.45</td>
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<td></td>
<td>Chaco-McElmo &amp; Chaco</td>
<td>186</td>
<td>91.8</td>
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<td>.000*</td>
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<td><strong>T-TEST RESULTS</strong> for vessel wall thickness</td>
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<td></td>
<td></td>
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<tr>
<td>bowls:</td>
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<td>97</td>
<td>2.17</td>
<td>95</td>
<td>.03*</td>
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<td></td>
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<td>Chaco-McElmo &amp; SoS1</td>
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<td>-1.39</td>
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<td>jars:</td>
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<td>.72</td>
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<td><strong>T-TEST RESULTS</strong> for slip slope band width</td>
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<td>jars:</td>
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<td>0.13</td>
<td>17</td>
<td>.90</td>
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@ Rock not included in test.  c = corrected score.
* significant difference between types.
Table 4. Selected trait comparisons among Chaco-McElmo, Chaco, Black Mesa/Sosi/Dogoszhi and McElmo Black-on-white rims from Pueblo del Arroyo.

<table>
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<td>closed</td>
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<td><strong>TEMPER</strong></td>
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<tr>
<td>sherd &amp; sherdsand</td>
<td>99</td>
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<td>25</td>
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<tr>
<td>sand</td>
<td>43</td>
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<td>7</td>
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<td>Sis rock, rock-sand</td>
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<td>1</td>
<td>100</td>
<td>42</td>
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<td>2</td>
<td>0</td>
<td>8</td>
<td>9</td>
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<td>thin, streaky wash</td>
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<td>Sosi style wide line</td>
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<td>41</td>
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<td>narrow line &lt;5mm thick</td>
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<tr>
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<td>14</td>
<td>7</td>
<td>0</td>
<td>4</td>
</tr>
<tr>
<td>ticked</td>
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<td>40</td>
<td>37</td>
<td>41</td>
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<tr>
<td>unknown/ground</td>
<td>47</td>
<td>25</td>
<td>4</td>
<td>19</td>
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<tr>
<td><strong>EXTERIOR BOWL DESIGNS</strong></td>
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<tr>
<td><strong>VESSEL WALL THICKNESS</strong></td>
<td></td>
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<td></td>
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<tr>
<td>all forms mean (N)</td>
<td>5.1mm (97)</td>
<td>4.6mm (46)</td>
<td>5.0mm (53)</td>
<td>5.5mm (91)</td>
</tr>
<tr>
<td>(taken 2cm below rim)</td>
<td>sd .7</td>
<td>sd .8</td>
<td>sd .7</td>
<td>sd .8</td>
</tr>
<tr>
<td><strong>SLIP-SLOP BAND WIDTH</strong></td>
<td></td>
<td></td>
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<tr>
<td>bowl mean (N)</td>
<td>26.1mm (47)</td>
<td>22.3mm (18)</td>
<td>-</td>
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<tr>
<td>sd</td>
<td>9.8</td>
<td>11.3</td>
<td>-</td>
<td>-</td>
</tr>
<tr>
<td>jar mean (N)</td>
<td>17.8mm (8)</td>
<td>18.5mm (11)</td>
<td>-</td>
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<tr>
<td>sd</td>
<td>16.8</td>
<td>10.6</td>
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</table>

* Note: No Dogoszhi B/w rims were recorded. Sosi B/w = 52 rims.
Figure 5. Chaco-McElmo Black-on-white jar body sherds from Pueblo del Arroyo.
Figure 6. Chaco-McElmo Black-on-white pitcher body sherds from Pueblo del Arroyo.
the Tusayan Whitewares (Black Mesa and Sosi Black-on-whites). Selected attributes from the problem types that reveal differences and similarities to Chaco-McElmo are presented in Tables 3 and 4.

McElmo Black-on-white: While the two types share many decorative concepts, the major differences lie in the frequencies of the design elements employed (Table 4; Franklin and Ford, 1982, p. 945). An earlier study suggested close continuity between McElmo and its Chacoan namesake (Toll et al., 1980, pp. 110-113), but the larger sample examined from Pueblo del Arroyo suggests less continuity between the two types, a finding echoed by Franklin and Ford (1982). McElmo often exhibits a thick, crazed, pearly, highly polished slip, and crushed andesite/diorite temper in thick-walled vessels that contrasts noticeably with Chaco-McElmo. Temper is not a good criterion for separating the two types, except when abundant crushed San Juan rock is present that disqualifies Chaco-McElmo from consideration. The primary attributes for separating McElmo from Chaco-McElmo are the slip, polish, and vessel wall thickness. To a lesser extent, some design elements and vessel forms are also useful indicators for separating the two types.

Black Mesa and Sosi Black-on-whites: In many regards, Chaco-McElmo is most similar to Sosi and Black Mesa Black-on-whites. It is unlikely to be a coincidence that the Tusayan Whitewares reach their highest frequencies around A.D. 1100, coeval with the appearance of Chaco-McElmo, but slightly before the appearance of McElmo Black-on-white. The use of parallel wide lines (or stripes), so frequently employed on Sosi and Black Mesa that it is often referred to as Sosi style decoration, is also the overwhelming preference for Chaco-McElmo. The use of other elements, however, particularly with checkerboards and dots, offers some contrasts. The Chacoan potters often used dots in isolated contexts (i.e., unattached to another element) or appended to narrow lines ("ticked lines"), but rarely on the triangles and broad lines in the style preferred by Tusayan potters for Black Mesa Black-on-white.

Rim decoration is also a major difference between Chaco-McElmo and the Tusayan vessels. No Tusayan rims found in Chaco exhibit paint, but over half of the Chaco-McElmo ones are painted solid or ticked.

Sand temper is common to both groups, although it can usually be separated on the basis of grain color and density: clear, abundant quartz grains in the Tusayan types and opaque, multi-colored, sparse grains in the Chacoan type.

Less useful to the classifier are the subtle differences in the slip and paste between Chaco-McElmo and contemporary Tusayan types. The Tusayan slip is often an egg-shell white color that covers jar exteriors and both sides of bowls. Sides left unslipped on Tusayan vessels are also poorly scraped and polished, leaving sand grains protruding slightly through the paste. The latter trait seems particularly common to Tusayan sherds but not to Chaco-McElmo.

**DATING CHACO-McELMO BLACK-ON-WHITE**

The last Chaco Cibola Whiteware type appears near the end of the Classic Bonito Phase at about A.D. 1090 or 1100. It was found in small amounts in association with the A.D. 1088 construction of the Salmon Ruin, a Chacoan greathouse near Bloomfield, New Mexico (Franklin, 1980, pp. 155-156). In Chaco Canyon, a small Chaco-McElmo pitcher came from the west wall core in Room 38 at Chetro Ketl (Figure 8f). The wall was constructed with the Kiva G complex, tree-ring dated between A.D. 1090 and 1095, with second story walls added between A.D. 1095 and 1105 (Lekson, 1984;
Figure 7. Chaco-McElmo Black-on-white ladles and bowls from: a - Chaco Canyon (C1999); b - Kin Kletso, Rm. 41 (C560); c - Be59 with burial (C1791); d - Chetro Ketl, Rm. 61 (C1610); e - unknown location (73.9.73); f - unknown location (72.43.200); g - unknown location (79.40.10); h - Be59 (47.11.31).
Figure 8. Chaco-McElmo Black-on-white jars from: a - unknown location (C2048); b - Bc53, Burial 8 (40.29.23); c - unknown location (C98); d - Una Vida, Rm. 83 (C24-19); g - Talus Unit 1, Rm. 18b (C551); h - Una Vida (C2002); i - Bc51, Rm. 15 (C1318); j - Kin Kletso, Kiva D (C1013); k - Pueblo Alto, Rm. 221 (FS618); l - Talus Unit 1, Rm. 19b (C1935).
Lekson, personal communication, 1984). Chaco-McElmo appears on the middens in front of Tsin Kletzin and Kin Bineola, each of which yielded a late cluster of tree-ring dated timbers, between A.D. 1111 and 1113, and A.D. 1111 and 1124, respectively (Lekson, 1984, p. 230; Bannister et al., 1970, pp. 20–21). I assume that the middens and timbers are coeval; the association of the dates and midden at Tsin Kletzin is strengthened by the late appearance and short occupation of the site in the early A.D. 1100s (Lekson, 1984, p. 231, 238).

At Pueblo del Arroyo, tree-ring dates extend from A.D. 1029 to A.D. 1109 (Ibid., p. 221), but a direct association with the pottery cannot be demonstrated. A number of small rooms appended to the south wall of the house, however, are clearly late architecturally (Ibid., p. 223), and an archaeomagnetic date from the Room 3 firepit of A.D. 1140±21 (ESO #1474) apparently confirms their temporal placement. Tree-ring dates of A.D. 1132 came from large ovens that contained sherd of Chaco-McElmo at Pueblo Alto (Windes, 1980). The latest tree-ring dates in association with Chaco-McElmo, A.D. 1139, came from Bis sa'ani and a nearby small house (Franklin, 1982b; Franklin and Ford, 1982).

Thus, from the available data, Chaco-McElmo Black-on-white can best be assigned a temporal range of about A.D. 1090/1100 to about A.D. 1140/1150. It may enjoy a short period of maximum, if not dominant, popularity soon after its inception until about A.D. 1120 when McElmo Black-on-white makes its appearance.

SUMMARY AND SPECULATIONS

Chaco-McElmo Black-on-white is a distinct Cibola Whiteware type that occurs near the close of the Chaco Phenomenon in the early A.D. 1100s. Its sudden appearance and demise make it extremely useful for temporal control. Although it reveals some attributes that are similar to other white-ware tradition types, technologically Chaco-McElmo is a Cibola Whiteware. It, along with Chaco Black-on-white, seems to be of limited production and distribution and, thus, it may be a product of craft specialization (Franklin and Ford, 1982, p. 953; Toll et al., 1980, p. 97). At least some Chaco-McElmo appears to be tempered solely with sparse trachyte temper that suggests manufacture in the Chuska Valley over 50 km. west of Chaco Canyon.

The dichotomy between imported open and closed forms among types probably reflects economic realities of exchange such that if massive local pottery manufacture was unfeasible because of a paucity of fuel (Toll, 1981, pp. 93–94) and other factors, then bowls could be transported greater distances than jars (see Toll and McKenna, 1983, p. 138). Note in Table 1 how jar/bowl ratios change as ceramics come from pottery traditions at an increasing distance from Chaco Canyon. Chaco-McElmo, however, yields approximately equal numbers of open and closed forms in Chaco Canyon. In contrast, at the Chacoan greathouse of Bis sa'ani and in the surrounding community, a mere 11 km. away, and beyond, Chaco-McElmo behaves like a tradeware because open forms (mainly bowls) are far more abundant than jars (Franklin, 1982a, Table 39; 1982b, Table 199; Franklin and Ford, 1982, Table 206) even though jars were imported into Chaco Canyon from much greater distances (e.g., the Chuska Valley).

The majority of Chaco-McElmo vessels were produced as bowls and pitchers. If Chaco-McElmo ollas are rare, perhaps there was a shift in group versus personal ceramic usage coincident with other changes in the Chaco system that appear to take effect around A.D. 1100. Because of their size and probable use for long-term storage, ollas seem suitable for corporate ownership. However, bowls and pitchers are more suited for individual ownership because of their
smaller volume and short term use. Unfortunately, we know little of the contexts in which most Chaco-McElmo vessels were recovered. Bowls, pitchers, and ladles most often accompany the Chaco dead. While the proportion of forms by type can be meaningful, the functional role of forms within any ceramic assemblage (composed of a number of types) should be the ultimate concern. Still, Chaco-McElmo promises to be a useful tool for further investigation into understanding the Chacoan system.

Albuquerque, N.M.

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THE ARCHITECTURE OF TALUS UNIT, CHACO CANYON, NEW MEXICO

STEPHEN H. LEKSON

INTRODUCTION

Talus Unit is one of the infamous unreported Chacoan sites. Excavated in the 1930s and central to some of the most heated taxonomic debates in Chacoan archaeology, Talus Unit was never comprehensively described in print. This paper summarizes a longer unpublished study of the ruin and its archaeology (Lekson, 1984a).

Talus Unit is located about 35 m. northwest of Chetro Ketl (Figure 1), as its name suggests, at the base of the sandstone cliff that forms the canyon wall. There are a number of ruins along the base of the cliff (Figure 1), and they were originally thought to form a class: "As there are many sites of this kind to be excavated in the future, this one has been designated in our reports Talus Unit No. 1" (Hewett, 1936, p. 106). Other small sites at the base of the cliff have been excavated at Chaco, but we search the literature in vain for Talus Unit No. 2. Thus we have dropped the numerical suffix, and call the ruin simply Talus Unit.

Prior to excavation, only a single massive wall (called the Pier in this report) stood above the Talus Unit rubble mound. This probably explains why the ruin was not mentioned by early explorers.

Archaeological research at Talus Unit was undertaken by two institutions, the School of American Research and the National Park Service. SAR, under the direction of Edgar L. Hewett, sponsored research in the 1930s; the Park Service was responsible for excavations incidental to stabilization in 1958 and 1959. Some details of this work will be found in Table 1.

Talus Unit consists of two main units, the East Block and the West Block (Figure 2). The East Block includes Rooms 1 through 8 and Kivas A and B; the West Block is the larger part of the ruin, including everything west of Kiva C. Two rooms, 9 and 11, connect the East and West Blocks.

For Chaco Canyon, Talus Unit is either a large small site or a small large site. Parts of it stood at least three stories tall. There were 30 to 35 ground floor rectangular rooms, and perhaps seven elevated round rooms. In the plaza of the West Block, there is a very large subterranean round room, almost large enough to be considered a Great Kiva. Talus Unit is a big building. Anywhere else in the San Juan Basin, it would be a shoo-in candidate for a Chacoan Outlier; but that could be said of many of the "small" sites at Chaco. Talus Unit's place in the Chacoan architectural pecking order will be discussed in the conclusions of this paper.

TREE-RING DATING

Tree-ring dates have been reported in four places:

1. CK-600 to CK-609, collected in 1933, reported by Hawley (1934, Protocol 1).
2. "GP" samples collected by Deric O'Bryan for Gila Pueblo in 1940, reported by Gladwin (1945, p. 93).
4. Re-dating of Laboratory of Tree-ring collections (Gila Pueblo samples plus "CK" samples obtained from Kiva J in 1959) reported by Robinson, Harrill, and Warren (1974).

Re-dating by the Laboratory of Tree-ring Research produced the definitive list; earlier dates were confirmed, changed, or discounted. Eight dates are listed by Robinson et al. (1974), but two of these (TN-1, GP-2213) are of no use for chronology. Thus, there are only six dates listed by the Tree-ring Lab with which to build a Talus Unit construction chronology.
Figure 1. Map of Talus Unit vicinity, Chaco Canyon, New Mexico. Site 1927 is 29 SJ 1927.

Table 1. Field Work at Talus Unit.

<table>
<thead>
<tr>
<th>Year</th>
<th>Institution</th>
<th>Personnel</th>
<th>Rooms</th>
<th>Excavations</th>
<th>Etc.</th>
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<td>1933</td>
<td>SAR</td>
<td>Walter</td>
<td>1-6</td>
<td>A, B</td>
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<td>Walter 1933</td>
</tr>
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<td>1934</td>
<td>SAR</td>
<td>Woods</td>
<td>5, 7-9</td>
<td>C</td>
<td>H</td>
<td>Woods, 1934a, 1934b</td>
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<tr>
<td>1935</td>
<td>SAR</td>
<td>Woods</td>
<td>7-14</td>
<td>D, E</td>
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<td>1940</td>
<td>Gila Pueblo</td>
<td>O'Bryan</td>
<td></td>
<td>(tree-ring samples)</td>
<td></td>
<td>O'Bryan 1940</td>
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<tr>
<td>1958</td>
<td>NPS</td>
<td>Vivian</td>
<td></td>
<td>(stabilization)</td>
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<td>NPS</td>
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<td>J</td>
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<td>Shiner 1959, 1961</td>
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</table>
However, this total does not include nine tree-ring dates from the 1933 collections: "the location of the collection made by Hawley (1934) is not known and, therefore not presented..." by Robinson et al. (1974, p. 40). Past experience with Hawley's tree-ring dates (Lekson, 1983) has shown that differences between Hawley's dating and re-dating of the same samples by the Tree-ring Lab are generally minor. However, there are some serious problems with the records and proveniences of the 1933 Talus Unit samples (Dean and Warren in Lekson, 1983, p. 181). Sample numbers CK-600 through CK-609 were assigned twice, once to the samples from Talus Unit, and again to a series of samples from nearby Chetro Ketl. In addition, there are occasional typographical errors and inconsistencies in the published dates (Hawley, 1933) due to the rushed production of this volume (Hawley in Lekson, 1983). Thus we are faced with a quandry; while the data presented by Hawley are fundamental to the dating of Talus Unit, the dates must be treated with rather more caution than the dates presented by Robinson et al. (1974). However, internal consistency in Hawley's dates suggests that both the dates and their proveniences (in Hawley, 1934, Protocol 1) are reliable.

The dates, with the proveniences used in this report, are listed in Table 2. Note that the proveniences in Table 2 are seldom the same as those given by either Bannister (1965, Table 43) or Robinson et al. (1974, p. 40). Proveniences of individual samples are discussed at length in Lekson (1984a).

ARCHITECTURE

As noted above, Talus Unit consists of two units, the East Block (Rooms 1-8, Kivas A and B), and the West Block (rooms west of Kiva C). These two blocks were connected by two rooms, Rooms 9 and 11. The architectural sequence will be discussed in terms of these units: East Block, West Block, and Rooms 9 and 11.

East Block

The construction sequence of the East Block seems relatively straightforward. It appears that Rooms 7-8 were a freestanding unit, and Rooms 3-6 a second unit, the two structures separated by an alley-like space. This "alley" later became Rooms 1 and 2 by the simple addition of three cross walls.

The only problem with this reconstruction is the southwest corner of Room 1. Figure 2 indicates that the south wall of Room 7 abuts its east wall. If the south wall of Room 1 did the same, all would be well; but as it currently stands, the stub of the south wall of Room 1 appears to be tied to the east wall of Room 7.

This contradicts the pattern of the other cross walls between Rooms 7-8 and Rooms 3-6; all these walls abut both units. Because the southwest corner of Room 1 is heavily stabilized, and because the other corners of Rooms 1 and 2 are consistent in their abutments, I believe that the offending southwest corner of Room 1 is an artifact of modern stabilization. If this is correct, then there are two main units in the East Wing, Rooms 7-8 and Rooms 3-6, both subsequently modified and eventually joined by Rooms 1 and 2.

Rooms 7 and 8. Rooms 7 and 8 are Talus Unit's most famous architectural feature—Stop #12 on the self-guided Chetro Ketl tour. The two rooms are actually one large room divided by an unusual low partition or stairway, which has been interpreted as a Meso-American-style platform mound.

No definite floor was found in either Room 7 or 8 (Woods, 1934a, p. 21). Ferdon felt that floor level of Room 8 was actually about 1 m. above the base of the walls, raised on fill retained behind the partition/stairs. "That such was not encountered can only be ascribed to the excavators'
having failed to recognize such a feature at a height considerably above where an ordinary Pueblo floor would normally be expected" (Ferdon, 1955, p. 7). In any event, no floor or floor features were described from Rooms 7 and 8.

The partition/stairs was built at the level of the base of the walls, the de facto floor. This feature abuts both the east and west walls; the internal sequence of construction (steps, wing walls, platforms, etc.) has been completely obscured by stabilization in the 1930s and after. Because this feature is rather important to our understanding of Talus Unit, Woods' description and Ferdon's and Vivian's interpretation will be quoted at length. When excavated, the "lower partition wall" was described as follows:

The central portion of the wall has a series of three steps, each 8'1" [2.5 m.] long and approximately 2' [0.6 m.] wide. A raised platform at each end of the lower two steps, slightly above the level of the central one, seems to have supported two vigas running lengthwise along the partition wall. The holes in which their ends were buried were visible in the masonry above the two platforms, with fragments of decayed wood still in them. The platforms were plastered over on the top, and carefully squared corners. The partition wall, about one foot [0.3 m.] wide at the ends, thickens in the central portion to support the upper two steps, and is there about 41" [1.0 m.] wide. On each side of the bench section [stairs] are additional sections of wall standing to the same height as the main partition and about 6'6" [2.0 m.] long, running parallel to the side walls of the room (Woods, 1934a, p. 7).

Ferdon noted that after stabilization in the 1930s, the appearance of the feature had changed. Woods' "platforms" at either side of the stairs had been eliminated: "...later stabilizing operations neglected to take them into account, and a vertical facing was reconstructed at both ends of the steps. Also...it would appear that in stabilizing activities the wall height was increased about one foot [3.0 m.]") (Ferdon, 1955, p. 7). Ferdon also added an important observation to Woods' description; he noted "the absence of any facing stones on the north, or back, side of the steps, which apparently were laid directly on the fill" (Ferdon, 1955, p. 7). The entire feature has been heavily stabilized, and Ferdon's observation cannot be confirmed today; but his is a compelling argument that the partition was indeed a retaining wall.

Vivian, on the other hand, suggested "there were quite possibly no steps here at all" (Vivian, 1959, p. 83). He based this remarkable assertion on the presence of the two beams "which ran lengthwise of the partition wall" over the steps, and which "in effect obscured their form and nullified their use" (Vivian, 1959, p. 83).

In making this argument, Vivian assumed that the paired sockets on either side of the steps represented ends of continuous beams; unfortunately, the beams associated with the sockets were reduced to "fragments of decayed wood" embedded in the masonry. The steps in the partition wall are probably unique in Anasazi building, and the structural implications of the troublesome sockets may be less familiar than the quick ascription of "viga" (which is obviously incorrect). But even if the beams did continue over and "nullify" the steps, this does not imply that there were "no steps here at all." In the discussion of Area H, below, a possibly parallel situation will be described in which a beam stairway either replaced or was replaced by masonry steps. Such may have been the case in Rooms 7 and 8, but this cannot be demonstrated. Positive evidence is limited to the undeniable presence of a set of masonry steps in
Figure 2. Plan of Talus Unit. Inset shows second story construction. (Base map by Joel Shiner.)
what is evidently a retaining wall. Perhaps in the 1950s, knowledge of Chacoan earthen architecture (and, in particular, filled platforms) was sufficiently undeveloped to explain Vivian's aggressive denial of Ferdon's reconstruction. From the evidence, Ferdon was correct: the structure was an unroofed room containing a platform in its north half, the surface of which was reached via masonry step. Although unusual (and in some details unique), this reconstruction is hardly controversial in the context of Chacoan building, with its repertory of mounds, platforms, ramps, stairways, etc. (Lekson, 1984b; John R. Stein, personal communication, 1983). What remains a matter for argument is the implication of this architectural form. Ferdon called it Meso-American, and it was this Mexicanist interpretation that Vivian attacked by discrediting Ferdon's reconstruction. Other interpretations of the same form are possible. In particular, one widely accepted suggestion is that Rooms 7 and 8 are part of an elaborate road complex. This interpretation will be discussed further in the section on Area H, below.

If the door in the east wall of Room 8 is original, and if my assignment of tree-ring samples CK-600 to CK-607 to that door is correct, Rooms 7 and 8 were probably built around A.D. 1032. Subsequently, a wall (perhaps very low) was built across the open south end of the room.

Rooms 3-6. These rooms began their career as a unit of three square rooms, Room 3-4, Room 5, and Room 6. If the dates from Room 3 refer to original construction (see next paragraph), then these rooms may have been built about 1076 (see Table 2).

Subsequently, each of the three square rooms was greatly modified. After Room 3-4 had filled 1.5 m. above the original floor level, it was subdivided by what was almost certainly a non-loadbearing wall. The structural function of this wall is important; if

<table>
<thead>
<tr>
<th>Table 2. Tree-ring dates from Talus Unit.</th>
</tr>
</thead>
<tbody>
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<td><strong>EAST WING</strong></td>
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<td>Room 3, first story primary beams</td>
</tr>
<tr>
<td>CK-608  959-1076 plus 1-5</td>
</tr>
<tr>
<td>CK-609  998-1066 plus 5-10</td>
</tr>
<tr>
<td>Outside Rooms 2 and 3, probably from Room 3</td>
</tr>
<tr>
<td>GP-2217  976p-1066vv</td>
</tr>
<tr>
<td>Room 8, door in east wall, lintels(?)</td>
</tr>
<tr>
<td>CK-600  966-1024 end</td>
</tr>
<tr>
<td>CK-602  982-1032 end</td>
</tr>
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<td>CK-603  998-1029 plus 1-5</td>
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<tr>
<td>CK-604  978-1030 plus 1-5</td>
</tr>
<tr>
<td>CK-605  962-1019 plus 10-20</td>
</tr>
<tr>
<td>CK-606  958-1032 end</td>
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<tr>
<td>CK-607  856-1018 plus 10-20</td>
</tr>
<tr>
<td><strong>WEST WING</strong></td>
</tr>
<tr>
<td>Room 20, first story primary beam(?)</td>
</tr>
<tr>
<td>TN-2   966p-1068v</td>
</tr>
<tr>
<td>GP-2219 1028p-1069 +r</td>
</tr>
<tr>
<td>Room 23-33, first story primary beam</td>
</tr>
<tr>
<td>GP-2214 967-1065r</td>
</tr>
<tr>
<td>Kiva J, roof</td>
</tr>
<tr>
<td>CK-1007 936p-1046 ++vv</td>
</tr>
<tr>
<td><strong>POOR PROVENIENCE</strong></td>
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<tr>
<td>Room 8 (stabilization element?) or Room 14</td>
</tr>
<tr>
<td>GP-2220 969p-1043v</td>
</tr>
<tr>
<td>No Provenience</td>
</tr>
<tr>
<td>TN-1   949p-1029vv</td>
</tr>
<tr>
<td>GP-2213 827-1031vv</td>
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</tbody>
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the wall was loadbearing, then Rooms 3-4 were almost certainly reroofed, and the tree-ring dates assigned to Room 3 might date the modification rather than the original construction. Because the cross wall was extremely flimsy, we can assume that the original roof continued in use over the subdivided room.

Rooms 5 and 6 were later converted to enclosures for Kivas A and B. Room 5 was altered very little, but Room 6 was almost rebuilt.

Rooms 1 and 2. As discussed below, Rooms 1 and 2 almost certainly postdate the initial construction of both Rooms 7-8 and Rooms 3-6. Since Rooms 1 and 2 are later than Rooms 3-6, they must have been constructed sometime after 1076.

Area H. Area H (Figures 3 and 4) consists of two complexes: first, the east complex, consisted of two piers and a retaining wall running to the cliff from the northeast corner of Room 8; and second, the west complex, a complicated structure with another retaining wall running to the cliff from the northwest corner of Room 8.

The west complex, partially rebuilt and heavily stabilized, has been variously interpreted as bins (Woods, 1934a) or ventilators (Shiner, 1959). At a 50-year remove from the original excavation, the interpretation presented below should be taken with more than the customary pinch of salt.

The west complex appears to be the base of a stairway; more precisely, the west complex was a landing for the base of a ladder or stairs (probably of wood) rising up to the base of a stairway pecked in the cliff, 3.7 m. above the landing. Elements of this landing include steps on its western face and a retaining wall enclosing a filled area (Figures 3 and 4).

A series of steps lead to the top of the landing from a masonry platform to the west. This platform was:

- about six feet [1.8 m.] long.
- The platform projected nearly four feet [1.2 m.] from the corner along the rock wall [base of the cliff], and narrowed down to two and a third feet [0.7 m.] at the other [southeast] end. It was plastered over the top with a hard whitish plaster. Near the narrow end were two deep holes, evidently once occupied by upright beams, each roughly 3” [7.4 cm.] in diameter. Their depth was not ascertained. The surface of the platform was uneven, sloping somewhat toward the south...

Two sets of steps, parallel and sequential in construction, lead up from this platform to the landing. The earlier of the two was a series of beam steps, indicated by sockets pecked into the cliff wall. Beams from these sockets were probably seated in a massive wedge of masonry at (and possibly tied into) the northeast corner of Room 8.

The beam steps were later replaced by a series of masonry steps, exposed in 1934 in a "bin" just north of this wedge (Ibid., p. 14). At least one of these steps is visible in the heavily stabilized "ventilator shaft" between Woods' bin and the cliff.

Both beam and masonry steps lead up to landing atop a retaining wall, running from a massive pier at its west end to the cliff face. The fill behind this retaining wall "was full of large shapeless stones, not of the type usually used for building wall" (Ibid., p. 21). This was almost certainly a rubble fill.

The west complex was subsequently altered and modified, primarily by extending the east wall of Room 8 north to the cliff face. It is currently impossible to untangle later building.

The west complex stairway system required an awkward right-angle turn from the wooden intermediate structure to the base of the pecked stairway high on the cliff. The east complex may have been an attempt to
create a landing to support an intermediate structure better aligned with the cliff stairway. The east complex consists of a retaining wall, anchored at its west end in a massive masonry complex of two superimposed piers. These incorporated alternating courses of stone and horizontal poles, much like Great Kiva piers (e.g., Great Kivas at Chetro Ketl, Pueblo Bonito, and Aztec Ruin).

Fill behind the east complex retaining wall was not described. Most likely it was not rubble fill, since that type of fill was described at length in other contexts (Woods, 1934a).

The configuration of the north wall of Room 2 (see Figure 3) suggests that it predates the east complex of Area H. Thus the east complex was relatively late in the history of the East Block.

The position of the west complex is less clear. If it is tied, as it appears to be (Woods, 1934b, p. 16), to the north wall of Rooms 7-8, then at least part of the west complex could be as early as those rooms, or about 1032.

West Block

The West Block is much larger than the East Block, and much less of it was excavated. Thus my West Block construction history is slightly more conjectural than that of the East Block.

The Old Building. Excavators referred to the rooms west of Kiva C as "the old building," and these rooms do appear to be the initial construction in the West Block. There may be a rear row of rooms, behind Rooms 26, 27, 39, 40, and 41; but there is little positive evidence for this row. Accordingly, I assume that the main part of the Old Building was only two rooms deep, with the front row (Room 36 to Room 22-33) standing two stories at its east end, and the rear row (Room 39 to Room 30) standing three stories at its east end. Rooms to the west were demonstrably two stories tall, and the rear row could easily have
continued the three story height of Room 30.

One date from Room 23-33, and two dates that may come from Room 20 suggest construction about 1065 to 1070 (see Table 2).

Bonding at the common corner of Rooms 23-33, 14, 10, and 18 indicates that an east wing (Rooms 10 and 15) was part of the Old Building, as was the Kiva C2 enclosure. Presumably, Kiva C2 was an original feature; if rooms west of Rooms 23-33 and 30 were multi-storied, the Kiva C2 enclosure almost certainly was not. Unless the upper stories of the enclosure were razed prior to Kiva C2 construction (which seems unlikely), the enclosure must have been single story, and probably designed to house a round room.

Symmetry suggests that a west wing (later to become an enclosure for Kiva G) matched the east wing of the West Block (Rooms 10 and 15). Placement of Kiva J suggests that it, too, was part of the Old Building.

Shiner (1961, pp. 21-22) argued that Kiva J was very late. He felt that late ceramics in the fill of Kiva J demonstrated a late abandonment, and that when Kiva J was built "it forced back the walls of two other kivas [D and F], thus post-dating those units." The lateness of the ceramic assemblage cannot be denied (we will return to these ceramics below) but, of course, ceramics from fill do nothing to date construction of Kiva J. My reading of the architecture of Kivas D, F and J is exactly opposite Shiner's; I see D and F enclosures superimposed on the Kiva J walls, making D and F later than J. Subterranean Chaco-type kivas are almost always earlier than elevated kivas; armed with this tendency, the symmetry of the Old Building, my reading of wall superimposition, and the single tree-ring date (1046vv) from Kiva J, I assign Kiva J to the Old Building.

Later Modifications I (The Pier and Kiva C1). Subsequently, the Pier and Kiva C1 were added to the Old Build-
Both involved substantial modifications. Kiva C1 apparently required the construction of an enlarged enclosure (Rooms 11, 12, and perhaps 28 and 29). The Pier eliminated first story Rooms 23-33 and 30, and all but eliminated their upper stories.

The most conspicuous feature of the Talus Unit is a mass of masonry projecting from the center of the site. Walter (1933) called it "The Buttress," without being too specific about what it might have buttressed. Woods (1937, p. 321) disagreed with "Buttress," arguing "that the wall was not intended as a mere support." She suggested that it might be a lookout or watchtower, or a platform for a town crier (Ibid., p. 322). "Watch Tower" replaced "Buttress" as the popular sobriquet. Fifty years' reflection by Chacoan scholars has not elucidated the function of this feature, but somehow "Watch Tower" rings a bit false—there's really not much evidence to support that use. The feature must be called something, so I have renamed it "the Pier," implying a standing, vertical mass of masonry, and nothing more.

Woods described the Pier in 1934:

Its north end disappeared into the debris of the mound several feet [1 m.] from the rock wall of the mesa. Enough of the stones remained in place to show that the south end had been squared off and faced in its present position. A gaping hole in the south end showed where a large viga had been set into the masonry along its main axis, and plastered thickly around with adobe to hold it firmly in place (Woods, 1934a, p. 20).

We now know that Woods' viga was in fact a primary beam of the second story roof of Room 23-33, which had been incorporated into the Pier (Figure 5).

The Pier was a three story mass of masonry (Figure 5). The base of the Pier used the first stories of existing Rooms 23-33 and 30 as a shell. These rooms were packed, floor to ceiling, with rubble and mortar, turning them into a 4 m. (E-W) by 7 m. (N-S) by 2.5 m. tall block of masonry.

Upon this substantial foundation sat the second and third stories. The upper part of the pier, while quite massive, was not so wide as its base. Instead of 4 m., the second story of the Pier was about 1.6 m. in width (E-W). The reduction in width created four very narrow rooms between the Piers and the walls of the second stories of the rooms in which it was built (Rooms 23, 30, 33, and the unnumbered room east of Room 27). In the "new" rooms, the sides of the Pier were faced from floor to ceiling, like any other wall.

On the third story, construction became slightly more complicated. The rear room (Room 30) was three stories tall, but the front room (Room 23-33) was only two. The Pier continued upward, about the same size as its second story, within the rear third story Room 30. But the south end of the Pier, on the roof of Room 23-33, was now free-standing; as Woods noted, its south end and sides were finished faces. Presumably, the Pier continued the full height of the third story, but it is now reduced below that.

The Pier certainly looks like a buttress. The only problem with this interpretation is that there is no obvious candidate for structural support. Kiva C is an adjacent, massive construction, and the first story of the Pier might have supported the west wall of Kiva C2. But on the second story level, the Pier is recessed from the common wall between the two structures—thus the Pier did not support Kiva C1. Nor would Kiva C explain the third story of the Pier.

The east wall of Kiva E abuts directly against the second story Pier. But buttressing was obviously not a major worry of the designers of Kiva E: witness the west half of this round room, built out over the poorly consoli-
Figure 5. Section of Pier, Talus Unit, along east-west centerline of Rooms 20, 23, 33, and 14. "A", "B", and "C" refer to excavators' designations of stories. Dashed lines indicate projected walls and beams.
dated fill of Room 20. Moreover, second story Kiva E, alone, would not have required the massive construction of the Pier in Room 30, or any of its third story. While part of Kiva E clearly enjoyed the Pier's support, I suspect that Kiva E was a later inspiration, and not the structural problem being addressed by the Pier's designers.

It has been suggested (anonymously and casually) that the Pier supported the framework of a stairway to the cliff above the Talus Unit. This is certainly possible, but a known stairway system just 35 m. east (Area H) makes a second (and necessarily much more elaborate) stairway at the Pier superfluous. However, at Chaco arguments of simplicity or efficiency are always suspect, and perhaps the Pier did support such a stairway. Certainly I can offer no more plausible function for this massive feature.

There are no tree-ring dates from this construction. Masonry for both the Pier and Kiva C2 was of the distinctive "McElmo" style, usually denoting construction of the early 1100s (Lekson, 1984).

Later Modifications II (Kivas D-G).

Kiva E clearly post-dates the Pier; probably it was part of construction creating a series of small round rooms (Kivas D, F, and G) along the front of the Old Building. Kivas F and G used existing walls for their enclosures; Kiva D was enclosed in a structure specifically built for the purpose.

Rooms 9 and 11

Rooms 9 and 11 connect the East and West Blocks. The south wall of these two rooms is a continuation of the rear wall of the Old Building. Presumably, it was built at the same time as the Old Building, about 1065 to 1070.

The north walls of these rooms, and the cross wall between them, are later. The north wall of Room 11 is an extension of the north wall of the Kiva C1 enclosure, and dates to the construction of that round room, presumably in the early 1100s.

Abutments suggest that the north wall of Room 9 and the cross wall between Rooms 9 and 11 are both later than the north wall of Room 11 (see Figure 2).

THE PLACE OF TALUS UNIT IN THE CHACO CULTURE PATTERN

Paul Walter, Jr., Talus Unit's first excavator, thought that the ruin was the one of several "outlying communities which obviously are related to and a portion of the Chetro Ketl town. Such a subsidiary unit is Talus Unit..." (Walter, 1933, p. 1). This soon became (or perhaps already was) the Hewett party line. Three years later, Hewett delivered himself of this prediction:

The ruin will be found to contain probably thirty rooms. While of small extent, it contains living and storage rooms, kivas, and all the domestic features of a complete village. Of somewhat cruder masonry, there is nothing to set it apart as distinct from the main town...It probably was the home of a Chetro Ketl clan (Hewett, 1936, p. 106).

Margaret Woods, who assumed responsibility for the Talus Unit after Walter, was less certain that the East Block of Talus Unit was Chetro Ketl en petit. She had reservations, noting that:

The unusual size of Rooms [7-8 and 9], the peculiar features of the masonry structures in [Area H], suggest that this may not have been an ordinary house unit for living purposes alone (Woods, 1934a, p. 11).

Woods was a lone voice crying in the wilderness. Hewett abhorred any suggestion that affairs at a Chacoan pueblo could have been any different than life at, say, Taos; and he apparently considered anything with walls
a pueblo: Talus Unit was a little Chetro Ketl, and that was that. Woods' subsequent reports (1935, 1937, 1938) stuck pretty much to the facts and left interpretation to others.

Talus Unit initially was hailed as proof that small sites and big sites at Chaco were occupied at the same time (Lister and Lister, 1981, p. 249). But then Hewett's excavations shifted to small sites on the south side of the canyon; the ramshackle $B_c$ sites there looked very little like the substantial Talus Unit. If the $B_c$ sites were villages, what was the Talus Unit? Hewett's students returned to a modification of his original position: "nothing sets it apart as distinct from the main town." Talus Unit wasn't a big small site, it was a small big site. Hawley, discussing "The Place of Tseh So in the Chaco Culture Pattern," had this to say about Talus Unit: "Talus Unit No. 1, built against the cliff just to the west of Chetro Ketl, is an example of a Pueblo III structure [read: Chacoan great house] as small as any of those of PII" (Hawley in Brand et al., 1937, p. 117).

This view did not suit Harold S. Gladwin, who at that time was tracing the evolution of the Chacoan great houses. Gladwin thought that the great houses (which he grouped together as the Bonito Phase) were preceded by a stage of little pueblos (which he called the Hosta Butte Phase). Talus Unit entered the Chacoan Taxonomic Wars as a type site of Gladwin's new Hosta Butte Phase.

In fact, Talus Unit was Gladwin's star witness. He nominated three sites as Hosta Butte candidates: Tseh So, $B_c$ 51, and Talus Unit (Gladwin, 1945, pp. 92-94). Gladwin assumed all three were little pueblos—function was not at issue. Instead, Gladwin's argument was chronological, based on tree-ring dates, almost all from Talus Unit: Tseh So had only one date, while $B_c$ 51 had two; Talus Unit had 14 (Ibid., p. 93).

Hewett's camp had once cited Talus Unit as evidence that little pueblos were contemporary with Bonito-class cruisers like Chetro Ketl. Gladwin countered that Talus Unit and its fellows were actually earlier; at the heart of every great house like Chetro Ketl was a little Hosta Butte Phase pueblo that—like a sand grain in an oyster—became encased in an enormous Bonito Phase pearl.

...$B_c$ 51 and Tseh So, and Talus Unit were all undoubtedly contemporaneous with those sections of Pueblo Bonito, and Chetro Ketl, and other great pueblos in Chaco Canyon, which had been built during the Hosta Butte Phase from 1010 to 1080 ...

After establishing that the little pueblos dated 1010 to 1080, Gladwin tried to show that the big sites were later. How he did this is amusing, but beyond the scope of this paper (see Ibid., p. 123ff); it is sufficient to note that his conclusions were incorrect (Bannister, 1965, pp. 150-151; Dean and Warren, in Lekson, 1983). In the end, Gladwin's developmental scheme has been discarded, but his original premise—that Talus Unit was contemporaneous with parts of the big sites is, of course, quite true. After the Gladwinian red herring, Talus Unit had been returned to its original status as a satellite of Chetro Ketl.

The site's next excavator disagreed. Joel Shiner felt that Talus Unit deserved better than subsidiary status. He saw Talus Unit as an independent town, basing his argument on kivas (Kiva J, which he had discovered, and Kiva K, a companion to J which he simply assumed existed south of Rooms 9 and 11). "Two large kivas in one pueblo would almost certainly rule out the possibility of the people of Talus Unit owing political and ceremonial allegiance to another pueblo" (Shiner, 1959, p. 2). He concluded that Talus Unit should not be studied as part of Chetro Ketl, but rather as a "cultural entity" (Ibid.).

And there, for the moment, the
matter sat. In all this argument, archaeologists had simply assumed that the building was a little pueblo. One shoe had been dropped by Margaret Woods, but her question had been ignored: was Talus Unit "an ordinary house unit for living purposes alone"? or was there more to the place than that?

When the other shoe fell, it was really something. In 1955, Edwin Ferdon claimed that part of Talus Unit (Rooms 7 and 8) was a Mexican platform mound (Ferdon, 1955). This was as far as one could go in 1950s Southwestern archaeology without being relegated to the lunatic fringe. Ferdon's "platform mound" provoked strong reaction, particularly from Gordon Vivian, a student of Hewett's. Vivian questioned Ferdon's reconstruction of Rooms 7 and 8, casting doubt on the steps between the two rooms; but, allowed Vivian, even if the broad steps were real and

...even if we admit the Meso-American origin of the possible wide stairway at Talus Unit ...these are all elements of Meso-American religious architecture. Among the Anasazi they are parts of dwellings (Vivian, 1959, p. 84).

By the Hewett doctrine of prima facie pueblo—anything with walls, rooms, and "kivas" was a dwelling—Talus Unit was a pueblo and architectural oddities like broad stairways were just that: architectural oddities. But the stairway in Rooms 7 and 8 was a bit too odd for most archaeologists, and whatever the merits of Ferdon's argument, his contention that Talus Unit was something more than just a little pueblo reached a far larger audience than Woods' unpublished report. The idea took hold, and is now part of Chaco Canyon tradition: there is something strange about Talus Unit; it's a weird site.

Reflecting this feeling (exaggerated slightly here), the most recent NPS interpretation of Talus Unit lists it as one of a group of "separate, esoteric sites" (Hayes, 1981, p. 57). Alden C. Hayes felt that Talus Unit was a little unusual. He saw one of the site's peculiarities as too many kivas: "Talus Unit No. 1 has five kivas to only 30 rooms—an unusually high ratio for habitations of this phase" (Ibid.).

Do more kivas equal more esoterica? Just as Hewett saw anything with walls as a pueblo, his students and their academic generation label any round, masonry-walled room a kiva. Presumably, when calling something a kiva, they mean that it is in some way analogous to historic rooms called kivas. If this were true, then more kivas might equal more esoterica. But there is little evidence that the round rooms built into the East Block and along the front of the West Block were anything but residential structures.

Perhaps we can discern domesticity (blissful or otherwise) in the trash it invariably produces. Given the changing techniques of archaeological recovery, we can most profitably look at sherds from trash. A critical analysis of Talus Unit ceramics is well beyond the purposes of this paper, but a few aspects of the pottery assemblage are pertinent.

In most discussions of Chacoan ceramics, it is customary to cherchez la mound. But there is no trash mound at the Talus Unit; in fact, there are few ceramics anywhere at the site from the period of dated construction (Woods, 1934a, 1938; Shiner, 1961). The first good evidence for anyone really living in the Talus Unit is actually rather late, considerably later than the last dated construction (Shiner, 1961).

It is no coincidence that most of the round rooms that give Talus Unit that religious look were the last things built at the site. The trash that filled Kiva J probably came from households centered in "Kivas" D and G. The architecture associated with the trash, when people are demonstrably living at Talus Unit, is round rooms. Perhaps they are kivas, but I see no reason to
see them as anything other than aboveground, masonry versions of the standard Anasazi pit houses.

Hayes' unease with the Talus Unit was not limited to its residents' possible pre-occupation with the esoteric. He also noted (correctly, in my opinion) that the Talus Unit held a unique position in the intra-canyon road network. In his survey report, he noted that Talus Unit was "a landing at the end of the roadway from Pueblo Alto" (Hayes, 1981, p. 57). Hayes expanded on the implications of this in the Chetro Ketl trail guide:

[Talus Unit] is quite unusual in the canyon and may not have been conventional housing, but instead may have served as a staging area for ceremonial processions on the road above (Hayes, n.d., p. 10).

While our knowledge of the extra-canyon road network is largely the product of recent research (see Vivian in Kincaid, 1983), the intra-canyon road system is old news and no secret (for the best summary, see Windes, 1982). In 1934, Woods saw that something strange was going on in Area H, but it was not until Hayes' work in the 1970s that the East Block was recognized as a roadway structure, and not a little pueblo after all. The long wait was almost certainly the result of Hewett's doctrines and teachings. As it turns out, Talus Unit was not simply Taos writ small.

One problem with Talus Unit is that it is actually three buildings: first, a road feature (East Block); second, a Great House (Old Building); and third, a residence (the late "kivas" and related construction). These three buildings have always been compressed into a single archaeological entity, "the Talus Unit," and the resulting complexity glossed over more often by doctrine than analysis.

The East Block was clearly a road feature, or rather a series of such features. Area H and Rooms 7-8 were both parts of an important intra-canyon road. The block of three rooms (3-4, 5 and 6), built in obvious conformity with Rooms 7-8, almost certainly were associated with this road. We are only beginning to understand the range and variety of the building associated with Chacoan roads (Windes, 1982; Kincaid, 1983); we are a long way from understanding their functions. At Talus Unit, at least one function is pretty obvious: part of the East Block was the base of an elaborate stairway. But I suspect that it was more than just that.

One of the more intriguing aspects of the East Block is its early date. If the 1032 date is correct, this indicates formalization of the intra-canyon road network almost a century before the explosion of Chacoan construction along extra-canyon roads (John R. Stein, personal communication, 1983). The possibility of an early date is supported by early ceramics from other intra-canyon roads (Windes, 1982).

The Old Building was a small Great House, built about 1065-1070. It was larger than most "outliers," and larger than several "great houses" in the canyon (e.g., New Alto, Tsin Kletsin); it was much smaller than buildings like Wijiji. The Old Building is about the same size as individual building stages at many of the larger sites. Room size, layout, and the associated subterranean Kiva J recall earlier construction at Chetro Ketl (Lekson, 1983). But Talus Unit most closely resembles the original structure at Pueblo del Arroyo, constructed about 1065-1075 (Lekson, 1984b). Before the addition of its massive north and south wings in the early 1100s, Pueblo del Arroyo must have looked very much like the Old Building at Talus Unit.

As to its function: we have a pretty good notion of the range and variety of building forms called great houses, and even some idea of how their general functions changed broadly through time (Ibid.). But we cannot claim that we understand precisely the relationship of their forms and their functions. We cannot look at the
ground plan of Talus Unit and say: it was this or it was that. One thing Old House probably did not do was house large, permanent domestic groups.

Later in its existence, Talus Unit did precisely that: with the introduction of numerous small "kivas" and the deposition of quantities of domestic refuse, it is clear that people were living in the Talus Unit. This occupation is not directly dated, but must be very late in the Chacoan sequence, sometime after 1100.

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Nominees for general inspiration and conscience-pricking: MARCIA TRUELL NEWREN, our lady of the small sites, and PETER J. MCKENNA, Oi-boy extraordinaire.

And the winner is:

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This report is also inventoried as Contribution No. 49 of the Chaco Center, National Park Service and the University of New Mexico, for purposes of bibliographic control of research relating to Chaco Canyon.
Figure 1. Side view of the famous Jackson Staircase, illustrating steepness of slope. Photograph by Pattison.
The justly famous prehistoric Indian ruins (period before the entrance of Francisco Vasquez de Coronado in A.D. 1540) of Chaco Canyon in west-central New Mexico have for over 100 years captured the interest of scholars from varied disciplines, as well as of local residents and national and international visitors. Recent and continuing research has been conducted by personnel of the National Park Service, the Bureau of Land Management, the Office of Contract Archaeology of the University of New Mexico, the Santa Fe Historic Preservation Bureau, the Public Service Company of New Mexico, and other institutions. Added to this, significant work by individual investigators has increased the knowledge of what is now known as the "Chacoan Phenomenon." Much emphasis has been placed upon the discovery of a prehistoric road system which positions the multi-storied and multiroomed pueblos of Chaco Canyon at the center of an elaborate economic, social, and religious system.

ROADWAYS

A concentrated effort to discover more about the road system was probably given the greatest impetus by R. Gordon Vivian, particularly after he had interviewed Mrs. Richard Wetherill. Marietta Wetherill homesteaded with her husband in Chaco Canyon, near the Pueblo Bonito ruin, at the turn of the 20th century. She spoke of the difference in vegetation growth stretching in long straight pathways north from Pueblo Alto (Wetherill Tapes, 1948), and of the patterns of roadways seen in strong moonlight. As early as 1874, members of the Wheeler Survey described "trails" and "paths" which ran in direct lines from pueblo to pueblo in New Mexico (Thompson, 1879). Oscar Loew (1879) of the same party mentioned a road in the vicinity of Pueblo Pintado, the most eastern of the canyon pueblos. Other observers in the canyon, Lt. James Simpson in 1894 (Simpson, 1850), photographer William Henry Jackson with the Hayden Survey (Jackson, 1878), and photographer Victor Mindeleff in 1888 (Mindeleff, 1891), must have noticed the unusual alignments of boulders or apparent terraced areas in the inner canyon. S. J. Holsinger, a Special Agent of the General Land Office, was sent in 1901 by the United States government to investigate, among other things, the actions of the Hyde Exploring Expedition and Richard Wetherill in the major ruin of Pueblo Bonito. His lengthy report (Holsinger, 1901) called attention to several roadways and their associated features.

STAIRWAYS

The explorers also were aware of the elaborate stairways carved into the bedrock of the cliffs behind each major ruin in the inner canyon. The aforementioned Jackson (1878) recorded several, including the famous Jackson Staircase (Figure 1). Fortunately, he sketched this and other stairways, since his photography was not successful. Holsinger (1901) mentioned Pueblo Bonito, Pueblo Arroyo, Kin Kletso, Hungo Pavi, "Chettro Kettle," Penasco Blanco, and "Sin-Kle-sin" as having stone stairways, and described the Jackson Staircase as the finest. This major stairway of rock-carved steps with hand holds on either side is an exception to the rule, as no major ruin has been found in direct association with it.

The pretentious rock-cut steps are by no means the only vertical transport devices used by the occupants of the canyon. In addition to the major stairways there are dozens of step and hand and foot hold sites scattered on the benches, ledges, and talus slopes, as well as on the more steeply defined cliffs. The National
Figure 2.

Stairway is above, and sharpening grooves are directly below, person seated on boulder. Courtesy National Park Service.

Figure 3

Hand and foot holds lead at a diagonal to rock art on ledge. Courtesy National Park Service.
Park Service and the University of New Mexico survey of the 1970s recorded 2200 sites of which 249 were roads, trails, and stairways (Hayes, 1981). They included those of the historic Navajo as well as the prehistoric Anasazi. One hundred and forty-eight of these are actual steps or stairways. If one excludes the vehicle roads and combines stairways (including the more recently recorded ones), modified passageways, and foot trails, the total amounts to about 200.

Hayes described the stairways as being of three types: 1) shallow cavities pecked into the bedrock for hand or foot holds; 2) wide steps with definite tread and risers, also pecked into bedrock; and 3) masonry steps ranging from a small pile of stones to "formally laid flights of wide stairs" (Hayes, 1981). A further breakdown of stairway types was made by Joan Mathien of the "Chaco Center" and me in order to facilitate entry into the computer system. Our categories include: 1) hand grips only, 2) hand and foot holds, 3) stacked masonry of one or two steps, 4) stacked masonry of multiple steps, 5) major rubble masonry, 6) ramps, 7) rock-cut minor steps, 8) rock-cut major steps, 9) modified crevices or passageways, 10) Anasazi only trails, 11) Navajo only trails (excluding wheeled vehicles), and 12) trails used by both Anasazi and Navajos. When it became necessary to further categorize these as to function and access, the breakdown almost became another kind! In spite of the extremely helpful publications and advice of R. Gwinn Vivian (1983) and Tom Windes (1983), the process was like playing mental jackstraws. A tight typology becomes restrictive when one is discussing 200 disparate sites, especially when one has not personally seen all the described stairways. What are hand grips as contrasted to hand and foot holds? When does a foot hold become a small bedrock-carved step? How must stones be piled to become a masonry step? What is rubble, anyhow—collapsed stacked masonry steps? How collapsed

Figure 4
Hand and foot holds on each side of crevice are common. Also note stacked rock masonry step. Courtesy National Park Service.

Figure 5
Access is to first bench. These hand and foot holds resemble those at Mesa Verde. Courtesy National Park Service.
does it have to be before it becomes rubble rather than a fallen masonry step? If "major" stairways are conceived as having treads longer than one meter and as being associated with the prehistoric road system, what about those directly associated with the roadways that are less than one meter? What are the criteria for a "modified" crevice? What about trails? Is a foot, stock, or even wagon trail used by the Navajos in an obvious Pueblo site locale an original Anasazi trail? How can one tell? Does it matter? Can we compare these stairways with those of the Glen Canyon Kayenta Anasazi, or the Canyon de Chelly people, or the Incas of Peru? The real importance is how this all fits together in a communications system within the circle of an important Anasazi trade and religious center.

The most common type of stairway in Chaco Canyon has small cavities pecked into the rock face, called variously hand and foot holds, hand and toe holds, or even hand and toe holes. These vary in length (horizontally) from 6 to 33 cm. and in depth vary from 1 to 21 cm. These pecked stairways serve all kinds of purposes. They go up the surface of boulders to rock art or to sharpening groove sites (Figure 2); they go up (or down) to ledges containing storage areas and, again, rock art sites (Figure 3); they act as assists when climbing a narrow and steep passageway (Figure 4); they traverse or vertically traverse extremely steep cliff faces (Figure 5); they facilitate access to important water sources or water control areas; and they are often adjunct to a rock-cut stairway (Figure 6). Ninety-four of the stairways in Chaco Canyon are either the hand and foot hold type or contain these holds in addition to another type of steps. This form appears more in Anasazi sites than in Navajo.

Stacked masonry stairways can be described as follows: 1) stones stacked one upon another to reach a higher (or lower) level (Figure 7), or often there is only one, used as a
Figure 7

A stacked masonry step at the base of a stairway. Courtesy National Park Service.

Figure 8

Chetro Ketl ramp which is part of the Chetro Ketl-Pueblo Alto road. Ramp probably led to a platform or landing which served as a base for a log ladder. Courtesy National Park Service.
Figure 9. Stone laid in prepared excavation. Stairway is on the Ashislepah Road across Chaco Wash from Penasco Blanco. Courtesy National Park Service.

Figure 10. Masonry steps in a crevice; stairway leads to a natural catch basin. Courtesy National Park Service.
Figure 11.

Navajo foot and horse trail with masonry stacked steps and retaining wall. Courtesy National Park Service.

Figure 12

One section of major rock-cut steps near Casa Rinconada. Connecting prehistoric road leads to Tsin Kletzin. Courtesy National Park Service.
Figure 14.

Figure 13
Stairway east of Chetro Ketl. Connects with Pueblo Alto-Chetro Ketl road system. Photograph by Pattison.
stepping stone; 2) stones laid in a pattern of ascending steps; 3) ramps, of which the Chetro Ketl ramp is one example (Figure 8) and rubble filled "causeways" another; 4) possible platforms at the base of a stairway; and 5) a unique stairway in which a paving stone type of step is laid into a prepared excavation (Figure 9). As can be imagined, the masonry steps vary considerably, but it may be safe to state that the stairways average from 0.5 to 1.0 m. in horizontal length (width). There are probably more stacked masonry steps than are indicated by the stairway count which was 29 for those with one or two steps and 31 for those with multiple steps. Stacked masonry steps lead from one bench to another, smooth out talus slopes, fill crevices (Figure 10), provide access out of rock corrals, ascend steep slopes, and form doorway steps for hogans. Ramps, ramp/masonry combinations, and possible platform devices are often located at the base of the major rock-cut stairways. Masonry stairways are probably the most functional for the amount of energy expended to build.

Navajos probably used masonry stairways more than the Anasazi to facilitate travel in and out of habitation sites and the canyon. For one thing, they had horses, and many of the masonry stairways could just as well be called ramps for the use of these animals (Figure 11). Tumbled walls along Navajo routes are often part of a masonry-walled trail used for horses as well as for people.

The rock-cut stairways are spectacular, especially the major steps associated with Casa Rinconada (Figure 12), Hungo Pavi, Chetro Ketl (Figure 13), and Pueblo Bonito. There are at least 11 of these impressive stairs. Smaller carved stairways are often just as spectacular (Figure 14). They are associated with Anasazi sites more than Navajo; the Navajos used them, of course, and sometimes enlarged or modified them, but many were so eroded as to be unsafe or unusable. In
Figure 16

Figure 17.
Minor rock-cut steps and foot holds to seep and water supply. Courtesy National Park Service.
addition to being the termini of the wide roadways they lead over cliffs and benches from one level of the roadway to another (Figures 15 and 16), to seeps (Figure 17) and other water supply and control sites, to shrines, and appear unexpectedly in relatively flat terrain (Figure 18).

Major rock-cut stairways and ramps are features primarily associated with the roadway network which played so important a role in the complex cultural ecosystem of the Chaco Basin. But the dozens of hand and foot holds; the widely scattered masonry steps and stairways; the additional, but smaller, conveniently located rock-cut stairs; and the connecting trails function as important factors in the intra-community movements in Chaco Canyon. They tie together the life of the entire society. It is not unlike a freeway system either imposed upon or integrated with the country roads which still exist and serve as the vital connecting links between a populace and its social, religious, and economic needs.

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INTRODUCTION

To reconstruct the prehistoric past is not the easiest of tasks; however, this is precisely what archaeologists attempt to do by utilizing data amassed from excavation and any other knowledge pertinent to understanding the life ways of those who came before. Without question, it is a formidable task, particularly when one considers the myriad of possibilities associated with human behavior. To interpret the archaeological record with precision, clarity, and acumen has taken a quantum jump in the past 25 years because the proponents of anthropological archaeology have altered many old views through a far more rigorous scientifically-oriented, interdisciplinary approach. In sum, it is now possible to provide interpretations far more compatible with how prehistoric people lived and died than ever before.

The task of Southwesternists is eased somewhat from the outset because we have a cultural continuum of great depth; for the Puebloan peoples, for example, it is possible to trace their development several millennia B.C. from Archaic times, through the Basketmaker periods, and then, throughout the Pueblo sequence. Of course, the archaeological record may also incorporate the ethnohistoric period commencing with the Spanish Entrada of 1540 and continue with the historic period beginning ca. 1850 with American involvement. But perhaps the greatest boon of all relates to the fact that there is the ethnographic present (1875-1930), as well as the extant Pueblos. With all of these data from which to draw it is little wonder the Southwest has assumed a special place in the annals of archaeological method and theory; however, interpretation of the archaeological record requires some degree of caution (see Frisbie, 1983a). That one-to-one analogues are an accurate reflection of past and present behavioral patterns may not hold true; indeed, historic process and culture change must be considered as well.

If we are to comprehend fully that which is contained within the archaeological record, there might well be a few areas wherein the continuum offers more than researchers are willing to acknowledge. In large measure their reluctance bears on the fact that there is no apparent model on which to base alternative possibilities. Certainly one who utilizes written history/ethnology can account for the development of Puebloan culture through time by building, incrementally, from the present. In this endeavor mention must be made of Mexico and a number of traits and trait complexes which derived from this source, far to the south. However, the degree to which these emanations influenced the Puebloan peoples varies greatly according to the mental set of the researcher and his/her colleagues or close associates. Clearly it is impossible to doubt Mexican influences, but the specific mechanisms for their introduction are open to speculation. It is, in reality, here that imaginary lines are drawn between schools of thought. Thus there are those who envision the Mexican connection as of minimal importance versus those who view it as a paramount concern (see Frisbie, 1983b).

Much of the current debate con-
cerns the Chaco Phenomenon and precisely how much influence derived from Mexico to give rise to the cultural efflorescence within the Chaco Basin, as well as outside of it, beginning in the 10th century. The "official" interpretation as presented by the Chaco Project relies on an internally derived model for the development of a complex ecosystem (Judge, 1979; Judge et al., 1981). Herein a highly formalized redistributive network was established which apparently transcends egalitarianism. The idea of social ranking within Chacoan development has been the most widely accepted departure from the classic pueblo model; however, after a review of these proposals, I suggested a Mesoamerican derived ranked society model might well be more fitting with the data (Frisbie, 1980; see also Frisbie, 1983a, p. 28).

Mesoamerican influence in the Southwest, nonetheless, continues to be a topic of dubious interest to many (so they claim). This is primarily because they feel the data are too inconclusive, the subject "dead" (or should be dead!), and consequently, those of us who persist are "beating a dead horse." While speculation is definitely present, it is not of the "wild" variety; in point of fact, when the past 25 years plus are scrutinized carefully with respect to Mesoamerican-Southwestern contact a number of highly germane issues have not only been raised, but also delineated with considerable clarity. In other words, the case becomes stronger with the progression of time. I might add, somewhat parenthetically, for the Chaco Phenomenon, discussions with Bob Lister (1978a) and Jim Judge (1981) gave me the very distinct impression that each personally accepts far greater Mesoamerican influence than either will (or would) acknowledge in print—again because the data are largely speculative. And of course, for the Chaco Project, working out internally derived models which appear sound take precedence.

Mesoamerican Influence in the American Southwest: The Problem

Although it is not my purpose at present to review the literature pertaining to Mesoamerican-Southwestern contacts, there are a number of pertinent and recent papers which must be cited wherein these data are considered. Three of them relate to the concept of the Mesoamerican "World" System and are authored by Pailles and Whitecotton (1979), Di Peso (1983), and Lekson (1983). This theoretical perspective will be treated further, among other topics, in the forthcoming volume entitled: Ripples in the Chichimec Sea, edited by McGuire and Mathien (1985); it represents a collection of papers presented at a Society for American Archeology symposium during the 1983 Annual Meeting. Of equal interest are papers by Riley (1976) and Doolittle (1984) pertaining to the development of Sonoran "statelets," which help flesh out the work of Di Peso (1974) at Casas Grandes and in other parts of northern Mexico. Additionally, Riley (1982) has provided a monograph entitled: The Frontier People: The Greater Southwest in the Protohistoric Period. And finally, Ellis has authored a monograph entitled: Some Notable Parallels Between Pueblo and Mexican Pantheons and Ceremonies which is currently in press.

All of the above are important contributions to our understanding of interaction between Mesoamerica and the Southwest. Two other papers should be noted; the first, by McGuire (1980), minimizes actual Mexican contact, while the second, by myself, maximizes it (Frisbie, 1983b). I might add, too, that the present paper is a direct spinoff from the latter. In preparing an overview wherein one takes a positional stance, "little things" stated in the text often can and should be developed to strengthen one's position. On occasion these "little things" stick in the proverbial "craw" and in this instance there has been occasion to delve a bit more deeply. The occasion
is this volume which honors Al Hayes, and specifically because he has done archaeology in the two areas of concern; namely, Chaco Canyon and Spanish Colonial Mission sites.3

The "problem" considered in the remaining portion of this paper relates to the most frequently uttered objection to Mesoamerican influence of any consequence in the Chacoan Phenomenon. It is the simple fact that there are so few objects excavated which are of unquestioned Mesoamerican origin. My solution to the problem was briefly stated in 1981 at the first Anasazi Symposium (Frisbie, 1983b) and will be cited shortly. In the interim, artifacts per se are but one type of evidence available for interpretation, especially when one considers the multitude of data amassed with modern interdisciplinary research. Therefore I shall attempt to deal not only with artifacts, but also with the contextual frame into which they fit. The most important data, however, relate to the formation of the archaeological record.

COMMONALITY THROUGH PAUCITY: THE CASE IN POINT

Truth and ultimate reality may not always be what they seem. For truth is based on facts which are in and of themselves undeniable. If one does not properly look at the facts, then truth may not be what it seems. Archaeologists invariably look at incomplete records of the past and excavate ruins in order to reconstruct the past. To do so, artifacts and other remains are analyzed, pondered, discussed, and then, most frequently, written up for dissemination. With the passage of time knowledge accrues based on additional exploration and recovery of the archaeological record. A more complete understanding is the obvious outcome and with it, hopefully, interpretations which are a truer reflection of past events. Although the lack of artifacts has a detrimental effect and tends to blur the truth, even here one may pursue alternative viewpoints. This, I believe, is precisely our understanding of what really happened during the Chaco Phenomenon.

While everyone familiar with the Chaco Phenomenon is willing to concede that something unique occurred to give rise to this highly integrated and advanced cultural expression, few are willing to accept anything but minor input from an outside source. The latter, of course, is Mesoamerica. Invariably the reason given is the paucity of artifactual materials derived from that source. Regardless of the extensive list of traits and trait complexes (Lister, 1978b; see also Frisbie, 1978, pp. 210-216; 1983b, pp. 220-224), the paucity of artifacts dulls enthusiasm. Indeed, Haury (1976, p. 347) is not infrequently cited.

If all the Mexican goods recovered from precontact sites are brought together...the combined weight would not exceed a few kilograms, barely enough to make it worth a trader's time and effort to pack it northward over an arduous trail...The evidence we do have does not support the idea of large scale importations by the pochtecas. Haury concedes perishable items such as feathers and textiles, as well as others must be considered; nevertheless, his opinion is firm. In contrast, Riley (1982, p. 145) would disagree, and strongly:

I have a somewhat different evaluation of the known trade to the Southwest, especially since I consider it necessary to think of trade entering the Southwest from all sides. If all the trade goods recovered from southwestern archaeological sites were put together, it would amount to considerably more than "a few kilograms," and if those items going from one side of the Southwest to the other are added in, we are dealing with a considerable aggregate of goods. In other words, we are dealing with a flow, not a trickle. Add to this
the statements of early Spanish explorers about the movement of trade goods, and the flow becomes something of a flood.

While I wholeheartedly concur with Riley, the actual artifacts recovered from Chacoan sites of undeniable Mesoamerican derivation are not terribly numerous. Copper bells and macaws provide the primary evidence. Thirty-two copper bells were excavated from Pueblo Bonito, and others have been found in Pueblo del Arroyo and Pueblo Alto. During the Chaco Project excavations, three copper bells were unearthed from Pueblo Alto and an additional one from 29SJ633 (Judge, 1982). Of macaws, 29 were excavated from Pueblo Bonito, and others were found in Kin Kletso and Pueblo del Arroyo. These represent skeletons; macaw bones may also be noted, and a single one was unearthed during Chaco Project work at 29SJ1360 (Judge, 1982; cf. Mathien, 1983, p. 203). Additionally, mention should be made of conch shell trumpets, iron pyrite mirrors, pseudo-cloissoine, and stamps or seals.

This meager inventory is precisely "it"! How then is it possible to suggest Mesoamerica played a significant role in the Chacoan Phenomenon? As noted previously, one must also look at the total configuration of the Chacoan accomplishment. But is this truly sufficient to merit the suggestion that Mesoamerican trocadores or pochteca-like traders were responsible for the meteoritic rise of the Chacoan Phenomenon ca. A.D. 1050? I believe it is, particularly if one recognizes the fact that artifacts reflect an incomplete record of what was extant during Chacoan occupation. My point, quite simply, is that we have data on "exotics" from limited context—generally speaking one which reflects high status areas, and often accompanies the burial record. These items are not part of the "usual" inventory of material goods found deposited in trash; rather, they are more frequently associated with ceremonial deposits or on occasion represent an item which was lost. Further, I would argue that "exotics" not ceremonially deposited or lost would have been removed by their owners when they departed or abandoned the sites in question; if this were not the case, then others would have "liberated" them shortly thereafter. Therefore, the truth of the matter is that those who insist that the paucity of Mesoamerican artifacts is sufficient reason to deny a pochteca-like outpost centered in Chaco Canyon need to realign their theoretical bias.

I would further suggest that the notion of a Mesoamerican pochteca-like outpost (Frisbie, 1972, 1978, 1980, 1983b; see also DiPeso, 1974; DiPeso et al., 1974; Kelley and Kelley, 1975; and Reyman, 1978) may be shown to have an exceedingly close parallel within the Anasazi or Pueblo Southwest several centuries later. The primary difference between them is essentially that one is prehistoric while the other is historic. In other words, we have historic documentation for the latter, as well as the archaeological record, while for the former the archaeological record is our sole basis for interpretation. The historic example is, of course, the Spanish Colonial period and, in particular, the Franciscan Missions which were an integral part of it. My initial statement (Frisbie, 1983b, p. 222) follows:

The general paucity of material has dulled the enthusiasm for the pochteca-like model for some; however, I would call attention to a somewhat analogous situation: Spanish mission sites in the Southwest. How many Spanish/European artifacts are actually unearthed during excavation? Recall, too, that wagons and beasts of burden were available for transport in addition to human backs. From such reports findings are, indeed, not terribly numerous or exciting. Nevertheless, missionization activities coupled with
related political action exerted a profound effect on native peoples.

The comparisons between the cultural remains of the Chaco Phenomenon and Spanish Colonial Missions should provide the reader with an obvious and logical series of precise parallels which will clearly indicate that a paucity of artifacts need not preclude the presence of a foreign group. Further, I believe the comparison is particularly cogent because we are dealing with Puebloan or Anasazi peoples who derive from the same basic stock. The primary difference is a matter of time, in this instance approximately five centuries, as well as the fact that historic documentation is present to verify my contention in the latter instance. It is also instructive to note that the dominant foreign power or groups were exploitative and offered a new religious experience involving conversion which provided sufficient subjugation to make each of the systems operant. I would suggest, here, that the Mesoamerican pochteca-like group inaugurated the priesthood concept with the Cult of Quetzalcoatl at its base which somewhat later perhaps shifted to Tezcatlipoca's cult.\(^4\)

At the time of inception (ca. A.D. 1050) the initial cult was quite likely compatible with extant religious beliefs; the introduction of Christianity by the Franciscans, on the other hand, was diametrically opposed to native religious beliefs. In fact, the combined oppression of civil and religious authority led to the Pueblo Revolt of 1680, as well as both earlier and later uprisings.

The history of the missionary effort among the Pueblos has been summarized by Simmons (1979) and there is ample discussion of how the missions operated in conjunction with the exploitative encomienda system between 1600 and 1680, as well as the repartimiento which involved forced labor. Although native rights were supposedly well known and protected, infractions (and frequently to excess) were the common rule.\(^5\)

The center of missionary activity was Santo Domingo Pueblo throughout the early colonial period.\(^6\) Franciscans administered from this "central place," and missions sprang up dotting the landscape from Pecos on the east to Zuni and Hopi on the west and from Taos on the north to Senecu in the south. They are frequently of monumental-type architecture; indeed, Simmons (1979, p. 182) notes mission churches and conventos were commonly far larger and more sumptuous than demands required. However, this is not surprising in view of the fact that a large native force was available, as well as support from the Spanish Crown. Mission church plans for the early period are of two types—cruciform or continuous nave; on the other hand, the integrally associated conventos varied considerably based on the whims of the attendant friars and associates. Baptistry, campo santos, and other features might be added or altered as dictated by need. Need might also dictate the building of an entirely new mission church; for example, when Domínguez visited Santo Domingo in 1776 the old mission church was in a good state of repair but was used as a burial area. One passed through it as well as across a ceilinged, closed-ended "alley" between the two buildings to enter the new church (Adams and Chavez, 1956, p. 130).\(^7\) Domínguez might also be credited with data indicating there were four churches at Pecos (Hayes, 1974).

General architectural styles of the mission churches utilizing local construction materials (stone and/or adobe and timber) are easily segregated from native structures. Associated structures—the conventos with presidios, stables, cloisters, etc.—may be less obvious, although excavation generally provides clarification as does, of course, historic documentation.

Kessel (1979, pp. 132-133) documents the fact that Fray Andrés Suárez (or Juárez) secured the service
of Spanish craftsmen to train the natives and to assist with the construction of his mission at Pecos sometime between 1621 and 1625. The tools of the trade, axes, adzes, saws (small hand and two-man varieties), chisels, augers, and planes—were common items freighted north from Mexico, as were tacks, nails, and spikes. Carpentry became a major trade for a number of Pecos Indians, and their services were in great demand throughout the province of Nuevo Mexico throughout the Colonial Period. Additionally, Simmons (1979, p. 181) states for the missions in general:

As a corollary to the missionary program, the friars introduced economic changes meant to wean the Pueblo people away from native life and incorporate them into provincial society. Schools and shops, teaching mechanical arts as well as religion, became important adjuncts of the mission. Native smiths made and repaired iron tools, improved methods of agriculture and new crops were introduced, and development of animal husbandry allowed use of extensive rangelands adjacent to the Indian communities.

Missions were established slowly; Hayes (1974, pp. 1-6) provides an overview beginning with Coronado's Entrada of 1540. As might be expected, the movement grew following the colonizing efforts of Oñate begun in 1598, with missions being built and maintained as friars and necessary materials became available. Supply trains from Mexico were somewhat uncertain until after the visitation by Benavides from 1626-1629. Not only did he provide extensive information pertaining to the missions—he himself established 10—but he also set in motion the Mission Supply Contract of 1631 (Kessel, 1979, pp. 138-150). At this time 46 friars were in the field and 20 more were added; this provided the ceiling figure of 66 the Crown was willing to support. The figure was raised to 70 in the 1650s specifically for the El Paso district.

For New Mexican missions, 32 large iron-wheeled wagons drawn by teams of eight mules brought needed materials every three years; one wagon supplied goods for two missions. Kessel (1979, pp. 148-149) characterizes these caravans as "New Mexico's life-line," and rightly so! He (Ibid.) describes them as follows:

It was the only scheduled freight, mail, and passenger service between the colony and points south. Outbound, royal wagons and Franciscans on muleback, attended by military escort, hundreds of spare mules, and meat on the hoof, were joined by everyone else going to New Mexico, from royal governor to merchants to penniless hangers-on. It was a motley, boisterous train.

On the way back, a similar conglomeration formed around the king's wagons. Governors and ex-governors, claiming the right to use the emptied wagons for shipment of hides, salt, piñon nuts, and other produce of the province, wrangled with the friars who saw these exports as fruits of the unlawful exploitation of Indians.

The latter statement is of interest because missionaries, too, capitalized on what the Indians collected and produced. Abó, for example, was one of the wealthier missions and exported both salt and piñons; it was, in fact, piñons which provided funds for the purchase of the mission organ from Sonora in 1661 (Toulouse, 1949, p. 4; see also Hackett, 1937, p. 192). The prime purpose of saving souls might well be invoked in this instance, for music is a prime mover!

Let us now consider the material aspects of the missions. One should recall, here, that MANY more exist than the implied 64 based on the 32 freight wagons. Kubler (1940),
Simmons (1979 and elsewhere), Hayes (1974), and numerous others elsewhere, clearly indicate many locations had several missions built or rebuilt throughout their histories. Recall, too, there were many visitas which were administered by friars who resided, most frequently, at the closest mission proper. Tabira (or Pueblo Blanco) as recorded in 1641 was a visita of Abó (as was Pueblo de las Humanas or Gran Quivira). Later, in 1659, Tabira became the visita of Gran Quivira (Hackett, 1937, p. 135; Scholes, 1929, p. 48; see also Wilson, Leslie, and Warren, 1983). Ferguson and Mills (1982, p. 44) suggest the possibility that missions at Hawikuh and Halona (Zuni) may well have had a visita at Kayakima, in addition to the well documented one at Kechiapa-wa. These are but a few of the examples which could be cited.

We must consider, without question, well over 100 mission and visita sites which are part of the archaeological record, i.e., ruins. We know a considerable amount about their building, how they were supplied, and how they operated. Historical documentation concerning the early missions, including their contents, has been supplied by Benavides in his Memorial of 1630 (Forrestal and Lynch, 1954; Ayer, 1916) and in his Revised Memorial of 1634 (Hodge et al., 1945). The latter includes, "Supplies for Benavides and Companions going to New Mexico, 1624-1626" (Hodge et al., 1945, pp. 109-124). Similarly, the Domínguez descriptions based on his report of 1776 are exceedingly rich in detail of mission contents and other features (Adams and Chavez, 1956). We are privy also to translations of documents housed in the Biblioteca Nacional, Mexico, which provide the furnishings of nine New Mexico missions in 1672, just eight years before the Pueblo Revolt (Scholes and Adams, 1952).

With regard to the supply caravans, beginning in 1609 the Crown took over support of the missions; from that date, every three or four years, until firmed to every three years by the 1631 Contract, supplies were shipped (Scholes, 1930). The Contract held unbroken for 30 years. Just considering this time span, 1609 to 1661 (52 years), there were up to 14—but let us say 13 supply trains to allow for the four year spans. If, as we know, there were 32 large wagons fitted out during the 30 years of the Contract, 10 trips or 320 wagonloads (10 x 32 = 320) reached New Mexico during this period alone. If there were at least three pre-1631 Contract supply trains having half or less the number of wagons, we can still with relative safety assume the total number would be in excess of 350 wagonloads of goods—I might add, LARGE wagonloads. These reached New Mexico, as noted above, between 1609 and 1661. By the time of the Pueblo Revolt of 1680 the number would have surpassed 400, unquestionably. Suffice it to say, there were many more supplies shipped to New Mexico following the 1692 Reconquest.

Scholes (1937) provides the specific details of the 1631 Mission Supply Contract signed April 30th in Mexico City. By its viceregal terms, each friar departing for New Mexico for the first time would receive tools and materials to build his mission: 10 axes, 3 adzes, 3 spades, 10 hoes, 1 mediumsized saw, 2 augers, 1 chisel, and 1 plane. And, of hardware, 1 large latch to be used for the church door, 2 small locks, some small latches, a dozen hinges, and 6000 nails of different sizes. Other specifications involved food, clothing, and other personal accessories. For the mission church per se (soon to be constructed) were vestments and ecclesiastical items. Goods for extant missions were also provided.

Aside from the Pueblo Revolt, totally devastating to the Spaniards, there were numerous other difficulties which beset the friars and their missions. Among them were epidemic diseases, famine, raids by Apaches, Navajos, Comanches, and others—including insurrections of their wards and other vicissitudes. The Franciscans remain
to this day, but many of their labors are part of the archaeological heritage of New Mexico. It is to these that I now wish to turn.

Based on the foregoing it is obvious that each mission was sufficiently supplied to function in its religious capacity. Each recruited labor for building and maintenance and collected foodstuffs and other materials either directly or through donations from encomienderos. The training of Indians to assist in various tasks directly and indirectly involved or associated with the mission was commonplace. The results of all of these endeavors provide us with the remains within the archaeological record, a number of which are of such monumental nature that they are currently recognized as such and maintained by state and/or federal funds for public enjoyment (as is Chaco National Monument). In a number of other instances, the missions continue to function.

It is instructive to examine the remains from excavated sites and to consider, for the moment, the artifacts recovered. Let us begin with Pecos which was extensively excavated—all four churches and associated structures, as well as much of the Pueblo-occupied sections. Kidder (1932, pp. 305-308) notes that there were 344 pieces of metal. Of that number 132 are sheet scraps of copper, brass, or bronze, and there are 14 pieces of unidentifiable iron. Of items identified, iron awls and nails are by far most abundant, but one might add 5 knife blades, as well as one or two each of the following: spoons, keys, fragments of armor(?) , buckle, pot handle, arrow-points, chisel blade(?) , plane blade, hoe fragments(?) , harness items, and a few others. Copper, brass, and bronze items include 15 crosses, 9 rings8, 8 fragments of church bells, 8 candlestick fragments, 7 awls, 5 bells, 5 buttons, 5 religious medals, 2 arrow-points, 6 gun(?) ornaments, and a few other items. One silver cross and a few pieces of lead complete the inventory of metal artifacts recovered.

China of apparent Majolica (preponderantly Blue-on-white and other types), as well as some pieces of Oriental origin and "olive jar" remains occurred in the upper trash levels; several hundred small, nonrestorable fragments are noted. Rare were china and glass beads; these accompanied burials in the mission.

Stubbs, Ellis, and Dittert (1957) report on the discovery and excavation of the "Lost Pecos Church." Although a thorough collection of the surface area was made, not a single European artifact was found; the same may be said of the results of excavation.

With the work at the Pecos Mission reported by Hayes (1974) we may add several pieces of oxidized iron, a dark green sherd of bottle glass, a "thick fragment of a bronze bell," the blade of a clasp knife (steel), a copper button, and an arrowhead of steel. A "crudely-cut" brass pendant; Majolica and Chinese porcelain sherds; turned, green interior glazed olive jar sherds, a Spanish real dated 1784, and a rosary complete the artifacts.

The Pecos artifactual record of European/Spanish derived items is quite impressive; however, it should be realized that this site has had a great deal of archaeological attention, and is one that was occupied for most of the Spanish Colonial period. Occupation continued well into the Mexican period until Pecos was abandoned in 1838. In addition, the missionization process was quite successful at Pecos. Nonetheless, a cardboard carton of not very large size would permit one to haul off the Spanish derived finds.

From Ab6, Toulouse (1949) reports sherds of Mexican Majolica, as well as several restorable vessels. The preponderance of a Puebla Blue-on-cream type is noted for soup-plate and bowl shapes from Mission trash; polychromes, as well as Blue-on-white are also noted. A single olive jar (green glaze) sherd and two types of Chinese porcelain (Blue-on-white King-te-chen ware and two large fragments of a jar from Shantung province) complete the
ceramics. A copper button with ornate decoration, the hinges of the nave door, many hand-wrought nails varying in length from 2.2 to 6 inches, fragments of a spur rowel, a pair of fire tongs, and a .50 caliber round shot complete the inventory of metal objects. The final artifact is a single blue-green glass seed bead.

Wilson, Leslie, and Warren (1983) cite the following Spanish materials for Tabira: a brass cross, an iron blade, possible chain mail, 2 nails, an iron awl, a sheet iron scrap, 3 pieces of sheet brass scrap, fragments of 2 hawk bells, a possible vial or crucet fragment of yellow glass, a single yellow glass bead, and, finally, 5 Majolica sherds of three types, as well as 5 sherds of Blue-on-white Chinese porcelain.9

The Franciscan report of Montgomery, Smith, and Brew (1949) will conclude this examination of artifacts recovered from Mission sites. Unfortunately, the Hawikuh mission, though excavated and reported, has yet to have its artifacts analyzed. They are housed, as received (well over 50 years ago), in the Heye Foundation, Museum of the American Indian in New York (Smith, Woodbury, and Woodbury, 1966, p. xv). Mention, however, is made of a few artifacts of European/Spanish derivation; for example, a juniper cross with terraced top, and two copper bells (prehistoric?). All of these items were associated with burials.10 In contrast, Awatovi offers an interesting collection. The first is a bell fragment which appears to have been cast in the same mold as one of the two, still hanging, Acoma bells.

The Awatovi burial record produced six burials with only European objects, and 59 with both European and Hopi artifacts. Included were a wooden cross of ebony with Christ in copper, 7 other crosses of native woods, 2 medals, cloth wrapping or covering in 24 instances, 9 with beads, 1 with ring and needle, 1 with copper pin, 1 with spectacles, and 6 with knitted or cloth bags.

In addition to the medals noted above, one other was found. Analysis of them indicates they were "cheap but durable"—precisely what one should expect in a far removed mission. It is likewise suggested that the remoteness of the mission might account for the mere 104 tiny sherds of European origin recovered during five years of excavation. The great majority were Mexican Majolica, particularly Blue-on-white (Puebla), but there were some of Oriental origin, as well as one which possibly came from Spain. The coarse green glaze olive jar was also represented. The paucity of artifacts from Awatovi is somewhat mitigated by reading the section relating to "A functional analysis of Franciscan buildings." It is, in essence, a reconstruction of what should be expected based on the architectural remains fully fitted with materials which supply trains provided from their inventories. In full context, Montgomery's section, "Part III: San Bernardo de Aguatubi: an analytical restoration," (Ibid., pp.11-239) provides the type of vehicle which is readily acceptable simply because it is based on historical documentation. Were this not so, the architecture and artifactual record could not speak so eloquently. The archaeological record simply wouldn't merit such an undertaking; it is completely untenable.

THE QUESTION; THE ANSWER; THE CONCLUSION

Throughout the body of this paper I have tried to emphasize the simple fact that a paucity of artifacts of "foreign" or "exotic" nature is not the exception but the rule. In one example we deal with prehistory, in the other, history; both represent an outside force, one Mesoamerican, the other, Spanish. Both, however, emanated from the south with headquarters in the heartland of Mexico and both utilized the same routes of access with some possible variation. Both brought with
them items of material culture, an ideology, and an architectural heritage different from the indigenous one, and both were exploitative in total configuration. Both had a profound effect. But both are not fully acceptable; while one is historical fact, the other is speculative in nature. By comparing the two, however, commonality is inextricably entwined through the paucity of artifacts recovered at excavated sites.

The question: "Where have all the artifacts gone?" The answer: "They are not part of the discernible archaeological record; the great majority were removed by human agents prior to the last departures from the living sites." Those items which did remain were already deposited within the archaeological record as grave goods, as "untouchable" ceremonial deposits, as trash since their usefulness had been expended, or through loss. Material objects which were, in fact, left in situ could have been forgotten (i.e., through negligence) or, more likely, not considered worth transporting elsewhere because of size, weight, ease of replacement, or lack of usefulness coupled with disinterest.

A relevant factor in connection with removal of artifacts, particularly when consideration is given to "foreign" versus "local" material objects, is the fact that many "exotics" are kept as heirlooms—long past periods of active use, including re-use in altered form. "One man's trash is another's treasure," conveys meaning here; phrased differently, "dump picking." "Hand-me-downs" are another excellent possibility; once again, the practice keeps items within the active inventory.

There are yet other considerations which must be noted in these two specific instances. In each case an outside group was present, each had brought material possessions and through time added to them. Since these objects were being transported over great distances their value was enhanced. Were it not for the fact that they were not locally available, hauling them would not have been necessary. I would argue that their owners were of higher social standing, served in special religious and political capacities, and used many "exotic" items to further their work. I would argue, further, that the missionaries of the Franciscan Order possessed a far greater proportion of items considered "exotic" than did the Mesoamericans who preceded them. Beasts of burden, wagons, and a very different complex of material goods point to this. For the Chacoan period, not only were humans the agents of transport, but with the exception of ornamental copper bells, metals were unknown. Additionally, the Franciscans not only had access to metals in a myriad of types (copper, brass, bronze, iron, silver, gold, and others), but their everyday lives were highly involved with their use. While they utilized native ceramics, they also had access to considerable quantities of Majolica, Oriental porcelain, as well as other "exotic" types; glass might also be cited. In contrast, the Mesoamerican's "home" goods were considerably closer in content and kind, so that transporting ceramics would seem almost ridiculous. "Exotics" in this case would involve such items as copper bells, macaws/parrots, feathers, and a sprinkling of other items. In other words, the Franciscan's use of imported goods involved all aspects of their lives while for the resident Mesoamericans their goods pertained almost exclusively to a religio-political sphere and their positions therein.

As already suggested, when possible, "exotics" would be removed by their owners when it was decided to abandon a given site. This was not always possible, however. Missionaries were martyred and their missions sacked and burned. In other instances, such as the Pueblo Revolt, many missionaries and associates fled, unable to take much in the way of personal or religious items. Such materials should not be expected to become part
of the archaeological record. Indeed, as Hackett and Shelby (1942, pp. lii-liii) state:

The churches, where not burned, had been stripped of their sacred vessels, robbed of their ornaments, and in every way as completely and fully desecrated as Indian sacrilege and indecency could suggest, while sacred vestments had been made use of by the indians as trophies in the dances and festivities celebrating their success.

When Diego de Vargas mounted the Reconquest in 1692 he reported the Zunis living atop their sacred mesa, Dowayalanne; he also found they had preserved with great care the ritual objects which they had taken from the churches at the time of the Revolt (Espinosa, 1940). Parenthetically, I have heard rumors at Zuni that Spanish ritual objects including Bible, vestments, bell, and other items are retained in an underground room within the old part of the village. If these items are extant, they are most probably from the mission abandoned in 1821 which has been excavated and restored (Caywood, 1972).

In contrast, Fray Custos Vargas, serving at Pecos, feared for his life when the possibility of a second revolt arose in 1696. He contacted Diego de Vargas in Santa Fe, who provided immediate assistance. The guard, muleteers, and mules were dispatched to remove all religious objects, provisions, and sheep, thereby avoiding the possible loss of life and material items as occurred in 1680 (Kessel, 1979, p. 291). The point, again, is to suggest good reason why the archaeological record appears so "pristine."

As already indicated, we may surmise that a great many "would-be artifacts" not removed by the Spanish became incorporated into Pueblo households when circumstances permitted. One can well imagine, for example, a metal cooking pot's worth over a Pueblo utility vessel, or the value of a metal "anything" compared to its native counterpart. While knives and religious medals—granted of "cheap" variety—were given to the Indians, possessing additional items by whatever means would appear to have been favored. Otermin, for example, noted the writing desks from the Santo Domingo convento were in use within a Pueblo domicile during his 1681 visit (Twitchell, 1916, p. 87).

Let us not forget that Pueblo (and other native) peoples are highly resourceful, and anything but wasteful. At Zuni I have assisted in garden plots using a hoe which is of 19th century design and which has been rehandled numerous times. The handle, of local manufacture, secured while hunting, gathering piñon or other resources, or while herding sheep will, undoubtedly, be replaced many more times. But the hoe will continue in use as long as the family plots are planted, tended, and ultimately, harvested. Such items simply move from generation to generation—it is a useful tool, one which was made to last.

But what of items which have become part of the archaeological record? They have not officially become so UNLESS they fall under archaeological scrutiny. A marvelous 17th century tea kettle, for example, was found by a Hopi at Awatovi weathering from the surface many years ago. It was retained by the family for a considerable period of time but was finally sold to an Anglo for a good price—it remains in this private collection. Certainly numerous other items from both mission and Chacoan sites have been removed by Indians and others. Collection by Zunis at Hawikuh, Village of the Great Kivas, and elsewhere is specifically oriented toward beads and arrowpoints which are used ceremonially (i.e, prayersticks of certain types, fetishes, and other uses). At such times anything else of interest is collected as well.

Although not a pleasant subject, excavated items may disappear upon (or after) being found, prior to "offi-
cial" incorporation in the archaeological record. The classic example is the Chacoan frog (toad) which was pocketed by a Navajo workman, sold at a trading post, and later "discovered" by Pepper. Thus this artifact was recovered and is now housed in a vault at the American Museum of Natural History in New York, where it continues to have a fascinating story to tell (Frisbie, n.d.). While Roberts (1932) lamented the lack of expected turquoise at Village of the Great Kivas, much of what was found was "liberated" by Zuni workmen, the inner lining of a hat or a shoe being the temporary repository. At least one complete turquoise necklace continues to adorn the neck of a Zuni.

Another interesting topic in relation to artifacts is that of "foreign" shapes produced by local potters. For the missions, the soup-plate is best known; however candlesticks of probable local manufacture (see Hayes, 1974, p. 56; Toulouse, 1949, p. 16), baptismal fonts (Montgomery et al., 1949, p. 89; Smith et al., 1966, p. 148), and a variety of cups in Spanish-derived shapes are worthy of note. Similarly, mention can be made of Mesoamerican shapes in connection with the Chaco Phenomenon. Cylindrical vessels are most noteworthy as these were found cached, as well as in other contexts. Washburn (1980) provides a comparative study for many of the almost 200 within this class of artifacts, and finds a Mesoamerican origin for the shape as well as a number of specific attributes relating to construction. Other shapes include what were probably incense burners (shallow lidded bowls with handles), stamps or seals, and a great variety of effigy vessels, mostly fragmentary. The most exciting has been described as follows:

Perhaps the most astounding evidence to come to light relates to one of the fragmentary painted anthropomorphic effigy vessels analyzed and reported by Washburn (1978). She found the rather peculiar arrangement for typing the sandal to the figure to be precisely like that shown in Mexican Codices for pochteca sandal ties. Such attachment is highly distinctive (Frisbie, 1983b, p. 221).

Similarly, architectural and other traits of "foreign" introduction could be enumerated for both the Chaco Phenomenon (see Frisbie, 1983b) and mission sites (see Toulouse, 1949, p. 33); however, since it is my purpose to treat the paucity of artifacts, I shall refrain.

While I have not attempted to cite all of the non-indigenous artifacts from all of the sites which have been excavated in this comparative endeavor, I believe the picture which emerges from the sampling provided is instructive. There is without question a small percentage of artifacts of "exotic" nature in each of the cases being compared. This is highly expectable based on the way in which the archaeological record was formed. As demonstrated by the historic example, the disparity between what we might expect to find and what is actually unearthed provides a lesson to be well learned. I believe the truth of the matter is that were it not for historic documentation and a continuing Franciscan legacy we might well be considering a second "unusual phenomenon" and seeking yet additional internally derived models to account for an almost pan-Pueblo "second high." Does this one have roads for internal communication? Does it have one leading south into the "supposed" hearthland of Mesoamerica?

And last, but not least, someone might say:

Where are all of the Spanish artifacts? You could load all of those now known into one wagon—perhaps on the back of one hefty friar—and transport them back to Compostela, and thence to Mexico City from whence they came. Influence definitely, but presence questionable.
NOTES

1. This and several other papers cited herein are part of the continuing series entitled: Contributions of the Chaco Center. At present the number exceeds 40, and anyone interested in obtaining a current list may obtain one by writing to the Division of Cultural Research, University of New Mexico, P.O. Box 26176, Albuquerque, New Mexico 87125.

2. Lister and Judge have both served as "Chief" of the Chaco Center. Judge has provided a comment to my comment following Lekson's (1983, p. 194) paper at the first Anasazi Symposium:
"As you know, Ted, I'm not the world's strongest proponent of the Mexican Connection, but I'm being made a believer in some of those things. It's hard to ignore those copper bells and some other things." This, however, is not the reference to which I refer; rather it relates to a conversation following formal conclusion of the symposium.

3. The extent of his work within these two areas is obvious in his preceding Bibliography. I would like to note, here, that his comments on the Mesoamerican-Southwestern connection appear in Hayes (1981).

4. I shall refrain from presenting the full pochteca-like model for the sake of brevity and because the literature is recent and cited herein. In brief I would like to note, however, that it involves a small migrant group of mobile traders who established a trading/religious center within the Chaco Canyon at about A.D. 1050 based on previous contact and developments. This group, with ties to a string of similar centers extending from central Mexico into the Southwest, then instituted what we now term the Chacoan Phenomenon with a constellation of Mesoamerican traits of considerable magnitude (see Lister, 1978, and Frisbie, 1983b). While exploitative in gross outline, with turquoise of primary interest and piñons, salt, possibly slaves, hides, and other materials providing the exports, there was also institution of great culture change centering on religious conversion. Centralized administrative authority held high status positions which utilized the majority of "exotic" imports. Those located within the core can be assumed to possess more power than those controlling outlier (satellite) sites; Powers et al. (1983, p. 6) note their study of 36 of these probably represents 20-30% of the total. I suggest power resided within lineage (or clan) affiliation which incorporated the indigenous population through intermarriage. Based on a system of redistribution, obvious from the types of areas selected for outliers which vary considerably in ecozone, religio-political leaders controlled production and a considerable portion of its use. The indigenous people, scattered in hamlets about Chacoan outliers, as well as within the Chaco in similar fashion were, in part, residents of Chacoan structures too where they performed specialized functions. I would suggest, here, that the only probable "exotics" provided these people were brightly colored feathers for ceremonial use. Suffice it to say, one human bearer could transport a tremendous quantity, and yet not be taxed by weight. (A typical pack almost certainly contained weightier objects at the lower end and feathers at the top to total 100 or more pounds. The pack would be carried upon the back supported by a tump strap.)

5. Interestingly, many data tend to support apparent native willingness to construct the mission sites, suggesting "gentle persuasion" on the part of the friars over the local populace.

6. Could this in some way be connected to this pueblo's highly conservative nature throughout the period of anthropological inquiry?

7. Dominguez seems to intimate there may not have been a need to con-
struct the "new" mission church he visited—he certainly wasn't impressed with the "cellar"-like quality of the enclosed "alley."

8. The smashing of mission bells during the Pueblo Revolt was a widespread phenomenon. This behavior expresses in yet another way the total rejection of the imposed Spanish system.

9. An appendix to the Tabira report reprints, in full, Johnnie Cress' 1957 article on his excavations. Since additional Spanish items are noted from the "lesser trash pile," they should be noted. Included are: 12 square iron nails, probable knife or sword blade fragments, copper fragments, many "bits of fine quality china," one of which bore an incomplete drill hole, pewter medal (cross), 3 iron grapeshot, copper button, a yellow glass bead, and copper disc (1½ inch in diameter) with "punched center hole."

10. Inclusion of copper bells with burials is of considerable interest at this site but further research is needed to establish whether they are of prehistoric (possible Chacoan times) or historic origin. From observations of numerous ceremonial occasions at Zuni, I would guess that some of the bells currently being worn by dancers are quite old. Precisely how old remains a question. It is also interesting to note that the presence of clowning groups, here and elsewhere—sometimes standing in fixed locations or circulating among the dancers (normally dancing as well) as they perform—serves a highly specific additional function; namely to keep a watchful eye for any items which either fall or loosen during performance. Most commonly, strings of beads and bells or tinklers break loose, and at this point the clowning individuals (one or more depending on circumstance) collect any objects which have fallen. The dancer may be stopped at the outset until the problem is solved, or may continue performing and be handed whatever has fallen if immediate repair is not feasible. At Zuni, if both hands are holding objects, broken strings of beads and other items may be given to the Komohosonna (Katchina Father), normally being placed in his basket-handled bowl containing sacred meal. For this reason, loss during performance is minimized; occasionally, however, items become covered in sand or lost in mud—these may be retrieved by anyone should they reappear through wind and/or water action or via the shuffling of feet. Many years ago at Santo Domingo during their annual feast day, I noted a conus shell tinkler which had fallen from a dancer's costume. It was not noticed and was pushed slowly into the soft earth by dancing feet until totally obscured from view. Having noted its precise location, I "liberated" it following conclusion of the dancing. It is now housed in the SIU Anthropology Teaching Museum. This, however, is the only example of non-retrieval by the performers I have witnessed in over 25 years of observation—much of which relates to ethnoarchaeological concerns involving the formation of the archaeological record.

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continuing research interests among the Pueblos with considerable regularity.

I should also like to thank my two "homes away from home"—in particular Ernestine Evermon and family of Albuquerque, and Morenda Solomon and family of Zuni Pueblo. Each one had to put up with my erratic behavior during preparation of this paper—much of which consisted of cursing as typos were struck!

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EXCAVATION TESTS AT THE PUEBLO RUINS OF ABO, PART II

BERTHA P. DUTTON

As an expression of homage to Dr. Erik K. Reed, Part I of this report was devoted to three archaeological tests which I had made in the pueblo ruins of Abó in 1944 (Dutton, 1981). At the time of publication, the editor found that my contribution required more space than was then available. Thus provision was made for this continuation.

This, Part II, pertaining to a fourth and final test made at Abó, is presented as a tribute to Alden C. Hayes for his extensive excavations at Gran Quivira, or the Pueblo de las Humanas. The two highly significant volumes and related publications which he prepared thereon are of endless value to our Abó studies and those of other ruin sites in the region. Likewise, his years of work in the Mesa Verde, Chaco Canyon, Pecos, and elsewhere, with numerous published reports, add to the understanding of many archaeological problems and prehistoric happenings of the Southwest.

Since my Abó paper was originally readied, further attention has been directed toward ruin sites of areas around the salt lakes located near Willard, New Mexico, long recognized as the Salinas Province (Walter, 1931). The more widely known prehistoric and historic ruins are those of Gran Quivira, Quarai, and Abó (School of American Research, 1943, p. 25).

On October 1, 1981, Quarai and Abó State Monuments were transferred to the federal government. These, with Gran Quivira, then became a unit which could be administered and interpreted more effectively. Headquarters were established in Mountainair, a central location for the holdings of an area designated as the Salinas National Monument.

The release, then, of Mr. Hayes and his associates' volumes on Gran Quivira (awaited since 1969 and published in 1981) was a stimulating, authoritative source of information on many of the Salinas cultural and historical aspects. The disclosures of my Test IV came to be of especial benefit to the Monument personnel and to other archaeologists and visitors interested in the ruins of Abó.

Many similarities exist between Abó and Gran Quivira, and with Quarai to a lesser degree. Among differences now recognized are the languages which the various Pueblo peoples spoke. Although early residents of these sites talked dialects of the Azteco-Tanoan parent linguistic stock (Dutton, 1983, pp. 29-30), inhabitants of Gran Quivira and nearby pueblos came to speak a Jumano (or humano) tongue which is well explained by Hayes (1982, pp. 11-15). Quarai and the pueblos of Chilili and Tajique used the Southern Tiwa language, while the people of Abó and their close relatives spoke Tompiro (Schroeder, 1979, pp. 237-244). Maps (Hayes et al., 1981) show the distribution of these and other prehistoric villages (see also Riley map, 1982, Figure 13).

Following a schematic map which I prepared of Test IV, and a trail guide that the Monument has issued for the pueblo and the Mission of San Gregorio de Abó, it may be seen that this excavation, like that of my Test II of Part I (Dutton, 1981) was on the west side of the major arroyo (see map in Kubler, 1972, Pl. 49). This investigation was on a line of the big rock drop-off and potential waterfall, which has now been dammed at the point where the National Park Service interpretive trail crosses the arroyo. The location of our test trench was approximately 100 feet west of Test II, alongside the eastern end of an eminence presently designated as Mound B, south of its eastern plaza.

Dug in a northerly direction, in stratigraphic levels, a trench six feet wide disclosed a rich refuse accumulation which proved to be about four feet deep. Amongst the dirt were pot-
Figure 1. Schematic ground plan of dwelling rooms, Abo ruin, Unit IV, noting firepit locations, November 4, 1944.
sherds, bones (animal and human), carbon and ashes, stone spalls, and various artifacts. Within a few feet, however, a crude masonry wall was exposed, revealing a room which was ultimately designated IV-8. The exploratory trench was then turned westward, only soon to encounter several cobbles.

Those rocks proved to be all that remained of the east wall of a dwelling room. Otherwise, three low masonry walls were exposed to delineate Room IV-1. On the north, large rocks were placed vertically from the base, with small, horizontal rocks atop them, and miscellaneous boulders above those—all held together with adobe (Figure 2; compare Haury and Hargrave, 1931, p. 16, the oldest horizon at Showlow Ruin, Arizona). No evidence of roofing or plastering materials was detected. There was a well packed adobe floor, with a very fine rectangular firepit. As in previous tests, the upper surface of the pit was flush with the floor; it was five inches from the north wall and five feet from the west wall. Two discarded biface manos were set, 7.5 inches apart, in the north side of the firepit, which contained gray-white ashes 3.5 inches in depth. Below the ashes was dirt only. This pit was lined on all sides and base with flat rocks, or lajas. The corners were rounded and the entire interior covered with adobe (cf. Toulouse and Stephenson, 1960, pp. 9-15, 18; Barnett, 1969, pp. 78-80). With a movable rock at the fore (as shown in Figure 3), the mano "fire-dogs" provided support for a comal, or griddle, for cooking purposes. No comal was found in IV-1.

With several men excavating (Figures 6 and 7) the work progressed fairly rapidly. As far as determined, where walls remained they were constructed of irregular uncut blocks of Permian sandstone, with abundant red adobe mortar. All chambers were diverse in shape. Vertical joints were commonly unbroken, and corners were not bonded. Walls were primarily offset, thus many were jogged at the corners. This resulted in subrectangular structures. Because of the distortion, all room measurements in this unit were calculated; the dimensions were taken medially, north-south and east-west (see Figure 1). Units of these rooms frequently show one wall line, usually north-south direction, like a "backbone" of the assemblage. Rows of rooms might be built on either side of this prominent wall.

Numerous prehistoric pueblos that were constructed along similar plans, prior to those of Abó, may be identified in the Verde Valley of Arizona, e.g., Tuzigoot (Hartman, 1976, p. 15, Fig. 44) and Hawikuh near present Zuni (Smith et al., 1966, Fig. 1)—not oriented north-south but according to "in-between" the major directions, as some ancient Indians felt to be most significant. Although Pueblo Pardo, near Gran Quivira, was built of limestone, that pueblo was comparable to Abó in plan (Toulouse and Stephenson, 1960).

As the rooms were cleared it was disclosed that accumulated debris had been utilized commonly to attain horizontal planes upon which to build household rooms for the pueblo dwellers. Thus, as in Test III, it could be expected that refuse would be found below adobe floors. No large refuse mounds were discernible; rather, trash was scattered all over the site—dumped alongside walls or into depressions such as that of our test trench here, or into abandoned rooms.

Inasmuch as the firepit in IV-1 was the first of its sophisticated type that we had encountered, and the initial one of several used in this unit, it appeared evident that we had come upon an introduction of a well established culture trait which had evolved elsewhere. Table 1 presents comparative data for those firepits identified in rooms IV-1 through IV-7. No evidence of them was found in rooms IV-8 through IV-12. Prototypes have been exposed at ruin sites such as Tuzigoot, of the Sinagua peoples, which was abandoned around A.D. 1425 (Hartman,
Figure 2.

Firepit in IV-1, along north wall, showing use of "fire-dogs" in back. Note upright slabs in room wall.

Table 1. Distribution of firepits, LA 97, Abó Test IV, excavation of 1944.

<table>
<thead>
<tr>
<th>Excavation Date</th>
<th>Room No.</th>
<th>Form and Direction</th>
<th>Wall Paralleled(inches)</th>
<th>Size</th>
<th>Lining</th>
<th>Contents Condition</th>
<th>E 3.5&quot;</th>
</tr>
</thead>
<tbody>
<tr>
<td>2 Nov. 1</td>
<td>1</td>
<td></td>
<td>N L 22</td>
<td>FR P</td>
<td>fine</td>
<td>1 sherd</td>
<td>3.5&quot;</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>W 18</td>
<td>M</td>
<td></td>
<td>QWA charcoal, P</td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>D 5.25</td>
<td>M</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>2 Nov. 2</td>
<td>1</td>
<td></td>
<td>W L 21</td>
<td>FR</td>
<td>destroyed</td>
<td>#1, E</td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>W 14</td>
<td>FR</td>
<td>or poor</td>
<td>#2, FR A</td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>D ca. 5</td>
<td>FR</td>
<td></td>
<td>#3, EA stones</td>
<td></td>
</tr>
<tr>
<td>3 Nov. 3</td>
<td>1</td>
<td></td>
<td>N L 20</td>
<td>FR</td>
<td>good except</td>
<td>E/WA</td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>W 11.5</td>
<td>M</td>
<td>some basal</td>
<td>clean</td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>D 4</td>
<td></td>
<td>las</td>
<td></td>
<td></td>
</tr>
<tr>
<td>2-3 Nov. 4</td>
<td>1</td>
<td></td>
<td>S L 17</td>
<td>FR</td>
<td></td>
<td>WA</td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>W 10</td>
<td>M</td>
<td>good</td>
<td>clean</td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>D 4</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>3 Nov. 5</td>
<td>1</td>
<td></td>
<td>N L 17.5</td>
<td>C</td>
<td></td>
<td>QWA</td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>W 10</td>
<td>adobe</td>
<td>poor</td>
<td>base</td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>D ?</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>4 Nov. 6</td>
<td>1</td>
<td></td>
<td>N L 17.5</td>
<td>C</td>
<td></td>
<td>QWA</td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>W 11</td>
<td>adobe</td>
<td>good</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>D 5</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>4 Nov. 7</td>
<td>1</td>
<td></td>
<td>N indefinite</td>
<td></td>
<td></td>
<td>destroyed; similar to that of Rm. 6(?)</td>
<td></td>
</tr>
</tbody>
</table>

Code: A-ash; C-clay; D-depth; E-earth; FR-flat rocks; G-gray; L-length; M-manos reused as firedogs; P-plaster; R-rectangular; W-white; W-width; QWA-gray white ash.

The upper surface of each firepit (or abandoned pit) was flush with the floor; all firedogs extended 3.5-4 inches above the floor level. Corners of the rectangular firepits were conspicuously rounded.
Figure 3.
Firepit in IV-3 along north wall, facing western wall which exposes a slab of Abo sandstone widely known for its ripple marks. The pit has two brick-shaped "firedogs" providing a tripod for supporting cooking utensils over a burning fire.

1976, pp. 37-38). The trait can be traced through the Little Colorado drainage with its multitude of prehistoric folklands of the Cibola-Zuni and Acoma regions, down the Rio Grande and its tributaries, hence into the Salinas Province. The trait extended northward also, as illustrated by its appearance at the ancient Tano Indian settlement of Tunque, located east of San Felipe on the banks of El Rio de San Pedro (Barnett, 1969). As will be mentioned later, Tunque was contributing notable quantities of pottery to Abó and other Salinas communities during Glaze-paint periods C and D, A.D. 1425-1500 (Warren, 1969), thus it is known that Tunque had considerable influence upon Abó.

In reviewing the artifacts which Test IV produced (Table 2), it is seen that biface and triface manos are represented. Of course these and metates found on the surface, or in random digging, are easily carried away. They were still in use by the Spanish people living in the vicinity of Abó during and after our excavations. But why didn’t we find any metates such as those found at neighboring pueblo ruins of the area?

The rooms IV-2, 3, and 4 contained fine examples of large, thin, rectangular rock slabs which I innocently catalogued as metates. These specimens were fashioned by a pecked technique, as are all similar ones of which I have knowledge. Our slabs, with parallel faces, are in contrast to ordinary slab metates—for they are classified as technically ground objects. When I first saw the slabs of Abó, it crossed my mind that they had been used for covering hatchways in the roof, for these rooms must have seen many activities taking place on the roofs; and the residents must have had egress through the roof tops, inasmuch as only two doorways were indicated: one in the west wall of IV-1, and one in the north which was sealed.

These doorways signify a change of lifestyle and architectural modification at a time later than original
Table 2. Distribution of artifacts by rooms, LA 97, Abó Test IV, 1944.

<table>
<thead>
<tr>
<th>Classifications</th>
<th>Rooms</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>1 2 3 4 5 6 7 8 9 10 11 12</td>
</tr>
<tr>
<td>Pottery restorable</td>
<td>2* 2* 2**see p.</td>
</tr>
<tr>
<td>Significant sherds</td>
<td>See page</td>
</tr>
<tr>
<td>Worked sherds</td>
<td>4 3 3 2</td>
</tr>
<tr>
<td>Effigies, figurines</td>
<td>See Fig. 4c 1</td>
</tr>
<tr>
<td>Clay artifacts:</td>
<td></td>
</tr>
<tr>
<td>1-pipe; b-stopper?</td>
<td>b-1 a-1</td>
</tr>
<tr>
<td>Minerals: a-gyp.; b-limon; c-selen.</td>
<td>1 ea. a,b,c</td>
</tr>
<tr>
<td>Antler, bone, shell:</td>
<td>b-1 b-2 b-1 b-1</td>
</tr>
<tr>
<td>a-antler, b-awl, c-shell, d-spatula, e-bead(?)</td>
<td>e-1?</td>
</tr>
<tr>
<td>Raw rock specimens</td>
<td>1</td>
</tr>
<tr>
<td>Vegetal, charcoal</td>
<td>1</td>
</tr>
<tr>
<td>Stone artifacts, pecked flat rock</td>
<td>1 1 2</td>
</tr>
<tr>
<td>Stone artifacts, chipped, ground:</td>
<td></td>
</tr>
<tr>
<td>a-axe*</td>
<td>1</td>
</tr>
<tr>
<td>b-hammerstone</td>
<td>1</td>
</tr>
<tr>
<td>c-handstone</td>
<td>1 1</td>
</tr>
<tr>
<td>d-hoe/mano</td>
<td>1</td>
</tr>
<tr>
<td>e-mano: 2-face</td>
<td>2 1 2</td>
</tr>
<tr>
<td>3-face</td>
<td>1 1 2 1</td>
</tr>
<tr>
<td>f-maul</td>
<td>1</td>
</tr>
<tr>
<td>g-metate, slab</td>
<td>2 1 1?</td>
</tr>
<tr>
<td>h-projectile point</td>
<td>1 1 5 1 1 1,1? 1</td>
</tr>
<tr>
<td>i-scraper**, chalcedony, knife?</td>
<td>1</td>
</tr>
<tr>
<td>j-shaft straightener</td>
<td>1 2</td>
</tr>
<tr>
<td>k-sledge</td>
<td>1</td>
</tr>
<tr>
<td>l-tejos (for games)</td>
<td>2 2 2 1</td>
</tr>
</tbody>
</table>

* See Figure 5a, b
** See Figure 5i
occupancy and predominance of Chupadero Black-on-white and Corona pottery types of an A.D. 950-1200s span. It is clear that these rooms had been abandoned and filled with a mixture of early ceramic types, followed by an increasing array of imported glaze-decorated colorful varieties which first appeared here around A.D. 1300 (and soon began to be copied locally—see Baldwin, 1982, pp. 3, 16-17). It must be remembered that Pueblo peoples of those times had cultural strictures of clan organization. No more than a shortage of female children in a family could have accounted for abandonment of a segment of any village. Without daughters for males to marry and assure posterity to a family, all too many kin groups expired. Of course, other causes may have been responsible.

Research shows that slab metates recovered at Gran Quivira in quantity illustrate the need for a standard terminology (Van Valkenburgh, 1964, pp. 128, 131). None of those described or illustrated appear to be actually like those from Abó. Among the metates that Kidder found at Pecos (1932, pp. 66-69) it is difficult to distinguish any that are exactly like those at Abó.

Baldwin, who in 1982 did some excavating in three rooms of Mound J at Abó in a historic portion of the pueblo, has uncovered a large, rectangular firepit somewhat similar to those of our Test IV rooms. Likewise, in his Room 2, Baldwin found "an almost intact comal (griddle)" which appears to be like specimens from our Abó Mound B rooms. He calls attention to the similarity of a firepit in his Room 3 of Mound J to one which Hayes excavated in Mound 7 at Gran Quivira. Hayes (Hayes et al., 1981, Fig. 46) shows a rectangular firepit with "fire dogs" comparable to some of those of Mound B of Abó, but the workmanship is inferior. It may be recalled that firedogs were part of the complex in a small, rectangular firepit excavated in Room 1 of my Test III (1981, p. 191).

In addition, Hayes (Hayes et al., 1981, Fig. 46) shows one of the questionable slabs being used in a wall as cover of a large vent.

Although Baldwin found no comal near the firepit in Room 3, he comments that "it is easily big enough to use a stone comal...on" (1982, pp. 4-5). He assigned these rooms of Mound J to an "Early Occupation," prior to 1598, and said that they were definitely occupied by Indians (although they were already in possession of some Spanish things). His Early period appears to have dated during the 1630s or 1640s, the later from the late 1650s until 1672, when Abó was abandoned due to Apache Indian attacks.

I have noted the absence of comales in our excavations. In Figure 3, an arrangement is shown of the firepit in IV-3, with brick-shaped firedogs at the back and a moveable stone in front of the pit. This was a commonly used feature among the Indians of that time, especially those who had come under Spanish influences. In tripod patterns, I have seen circular comales of fired clay thus utilized, but I have not observed such non-round utensils so used. In bygone days, many of the Indian ladies used slick rectangular slabs of rock which they placed over their fires for baking "paper breads"—often known among the Pueblos as piki. The Laguna Indians, for instance, went from their village to the Jemez Pueblos to get trays fashioned from the volcanic pitchstone of that area.

All of our Abó slabs were recovered during the last two days of our excavating—when we were hurrying to finish the work, pack our finds and equipment, and make last minute notations. Everything was returned to the Museum of New Mexico. Due to the current ongoing transfer of many items to new storage quarters, the only records that I have of these four slabs at present are briefly written catalog sheets. I know that they were thin (ca. 1.5 inches) and rectangular, with parallel faces.
Is this seemingly ubiquitous household necessity a slab metate or is it a comal? Or did it serve some other purpose such as a kneading board (see Haury and Hargrave, 1931, Pl. 6), a vent cover or deflector, or all of these?

In discussing the artifacts, I must mention the shattered pipes which occasionally appeared, the fragments of which Indians commonly picked up carefully and used for ceremonial purposes. I was pleased also to find an animal fetish, modeled from claystone shale. It had been decorated by paint, and the sunken eye sockets were probably inset with turquoise or shell. Doubtless, this served an important ceremonial purpose also.

The scrapers that we recovered were probably utilized in skin dressing, as well as other domestic purposes.

The limonite mass found in IV-3
was surely used for decorating pottery, such as the polychrome glazes. The selenite may have served as a "window" pane, and the gypsum could have been used for whitening moccasins, or other household purposes. From all the items recovered from the rooms (see Table 2), it is evident that several of them were living and working quarters. Perhaps the lesser chambers were used for storage at some period of time.

Next, attention will be given to the potsherds. I prepared charts of representative sherds which were selected for a Guide Sherd Series of this Test IV, first of the strat test itself and then of the rooms excavated. Inasmuch as the latter were built upon and filled with refuse deliberately, the various layers have little significance. Toulouse found a like situation in his excavation at the Abó Mission (1949, p. 21).

Carlson has written of a population shift—after A.D. 1175—from the Puerco River Valley (of the west) toward the south and east to the Forked Lightning Ruin at Pecos (1961, p. 255). It is equally true that the hegira brought significant numbers of

Figure 5a. Platform scraper of milky white chalcedony from the fill in IV-4 (side view and platform); length at platform 1.5 inches by ca. 1.0 inch.

Figure 5b. Platform scraper of the gouge type; same material as "a"; greater length 2.5 inches.

Figure 5c. Axe of fibrolite from IV-5; greater length 4.75 inches, width 3.75 inches; this type said by Erik Reed to have come into the Rio Grande Valley circa A.D. 1350 (thus with the glaze decorated pottery and its other accompaniments).
westerners into the Galisteo Basin, and well along the eastern slopes of the mountains which lead to the Great Plains. The Manzano Redware which also occurs at Abó thus extends from southern Colorado into the Salinas Province (Dick, 1968, pp. 77-81).

More and more data are being assembled at the present time, making it increasingly possible to detect relationships between various groups who inhabited the Southwestern regions during succeeding periods. Wasley excavated three Late Pueblo II villages along I-40, or old Highway 66, which displayed a building pattern with a kiva "in front" of the domestic pueblo unit, a characteristic of the Anasazi villages through Pueblo III (1960, p. 40).

Dittert ascertained that these population movements extended eastward from the Cibola region about A.D. 1300, and he considers them responsible for the introduction of glaze-decorated pottery (1959, pp. 558, 563). Lambert (1954, pp. 175-176) mentions a drought between A.D. 1411 and 1424, which was followed by the abandonment of Paa-ko about 1425 (see also Riley, 1982, Fig. 13).

Thus, whenever these potsherds in our Mound B trenching at Abó prevailed, they came predominantly from the west, whatever their ancestry before their emergence from east-central Arizona, and despite the ultimate copying of their forms and designs with local materials at the home pueblo.

We recovered examples of Socorro Black-on-white, which merges into Chupadero Black-on-white to the extent that identifications are on the borderline in several instances. Berman (1979, p. 52) related that this type is of Anasazi origin, believed to have been derived from Chaco II. Dittert found that in the Cibola area it started in the A.D. 870-950 era and persisted through the A.D. 1200-1400 period (1959, p. 402). We found it at Abó ranging from A.D. 1050-1150. The Chupadero Black-on-white appeared from top to bottom of our levels, in
some quantity (see Hayes, 1981, pp. 67-73 for details).

Very obvious were the culinary deposits, sherds and clusters of shattered vessels which showed evidence of long use over fires. There was Corona Corrugated (A.D. 1150+) and Corrugated Plainware (A.D. 1225-1460) which Kidder (1932), Mera (1931), and others, as well as Hayes (1981, pp. 64-67) have discussed at length.

One of our sherds was identified as Affinis Mangus, ca. A.D. 950. Elmendorf Black-on-white appeared in the A.D. 1150-1200-1500 bracket. And some traces were found of black-on-white specimens suspected of being from the Mogollon region while Chupadero Black-on-white was present.

Then the glaze-paint decorated wares started with a St. Johns Glaze/Red sherd of the A.D. 1175-1300 epoch, to be followed by a scattering of Las Padillas Glaze/Red (A.D. 1300-1325), as an Aff. St. Johns with a Heshota-Pinedale Polychrome background (Lambert, 1954, p. 84). At the Forked Lightning Ruin at Pecos, Lambert says, St. Johns terminated ca. A.D. 1250. We found one example of Heshota from the A.D. 1200-1300 days. A thick piece of Rayo Glaze was recognized. These were followed by examples of Agua Fria Glaze/Red (some made locally), San Clemente of the Glaze A group (1315-1425), followed by Pottery Mound productions (1350-1400, or more probably 1450-1500), and Tunque Glaze/Polychrome of A.D. 1425-1500 (Warren, 1980, Tables 16 and 17).

From 1600 on we had Salinas Redware, even Salinas Glaze/Red. Based on a distinctive Salinas paste, Baldwin believes that a locally-made Agua Fria Glaze/Red variety is responsible for a derivative for which he proposes the historic name of Apéña Glaze/Red. He thinks that type, then, leads to a contemporary Apéña Glaze/Polychrome derived from San Clemente Glaze/Polychrome (see his comments, 1982, pp. 16-17). Of our fragmentary pottery examples from Test IV, Room
2, one was a mini-pot of brown-red (cf. Kidder, 1932, p. 135, Fig. 112), and one that Baldwin identified as Apeña Glaze/Polychrome. Room IV-3 produced one Tunque Glaze/Polychrome and one Apeña Glaze/Polychrome. From IV-4 came a Salinas Glaze/Polychrome piece, and one of Espinoso Glaze/Polychrome.

The excavated rooms reflected this pattern, but in lesser degrees. We found fragments of a Bandelier Black-on-gray vessel in Room IV-1 and more parts of it in IV-5.

Our bone collection totalled 563 from Test IV. These included 40 scraps of human remains. Turkeys were represented by 16, deer 118, bison 11, hare 11, rabbit 13, and some mammals; other remains occurred in lesser totals. This shows that considerable time was given to hunting as well as to horticulture. Dr. John Speth, who arranged the analysis (Hesse and Ballinger, 1982) gave us the information that our Test IV and Test I were "strikingly similar," thus providing some information that we had not had when Part I was prepared.

This report covers the high points of the two-week project at Abó, and reflects something of the research thereafter. Catalog sheets, notes, and photographs are in hand with further details for future use.

ACKNOWLEDGMENTS

I am particularly indebted to David Snow, A. Helene Warren, Stuart J. Baldwin, and David Brugge for the generous aid they gave me in identifying sherds and giving me counsel. And I express my thanks to Dr. John Speth, of the University of Michigan, for engineering the identification of the animal and human bones which I submitted for analyses. These were prepared by Brian Hesse with Diane Ballinger, of the University of Alabama at Birmingham. Funding for the analyses was provided by the Museum of Anthropology, University of Michigan, Ann Arbor.

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When one reads the recorded oral traditions of various Southwestern Pueblo Indians, the impression gained, particularly from those of the Rio Grande Valley, is that the ancestors of these people made a series of migrations over an unknown period of time from their place of origin, setting up a camp or a pueblo at stops along the way before reaching their present locale. Attempts to reconstruct directions and times of these moves in an effort to identify the pueblos at each of the stopping places usually meet with failure or frustration at the very least.

One can look hopefully for supporting data by comparing a tradition of one pueblo or clan to that of another also named or involved in a record of the wanderings from place to place. Such comparisons, however, are complicated by the occurrence of a number of conflicting statements such as those in the migration stories of Santo Domingo, Cochiti, and San Felipe Pueblos, which claim to have moved together for a good portion of their travels. Obvious discrepancies include situations where tree-ring or ceramic dates indicate near or contemporaneous construction for named sites, as well as considerable overlap in time, rather than stop-over occupational sequences as given in the tradition (Schroeder, 1976, 1977).

Such conflicts often are explained by some as due to memory lapses or embellishment of events by the tellers over the course of several generations of repeating the traditions. Parsons (1939, p. 17), in dealing with these problems of migrations, stated,

Pueblo "history" is so largely a tradition of migrations, not actual migrations, but of migrations according to traditional pattern. The Pueblo localizes his folk tales in the neighboring ruins...[and] (Ibid., p. 103) the very ruins of the country are little more than sites for mythical occurrences (underscoring supplied).

After referring to the "discredited migration-by-clan theory," she remarked in regard to fetishes and ceremonies of a clan (Ibid., p. 970) that the "tradition of provenience accompanies the wimi [fetish] and is more reliable than tradition of clan provenience of origin" (see also a Bear Clan statement at Hano regarding its fetish in Dozier, 1966, p. 42).

No one can doubt that occupants of various pueblos did move from one place to another in historic times. We have documented examples of large scale shifts on a temporary basis as a result of conflicts, such as those Pueblos that abandoned their homes and took refuge at other sites for varying periods of time during the 1680-1692 Pueblo Revolt, the 1640-1660s flight of the Taos peoples to the plains, and fleeing Picuris in 1696 to the same locale from where they returned in 1706. The best known move on a permanent basis is that of the Tanos, who went from Santa Cruz Arroyo of the Rio Grande to take refuge among the Hopis in 1700 or 1701 (Dozier, 1966). Permanent small scale moves, such as those of some Lagunas to Isleta about 1879 and that of a group of Hopis who left Oraibi in 1906 to found a new pueblo nearby, often resulted from factional splits within the pueblos involved, similar to such events mentioned in traditions.

We also have evidence of prehistoric group movements, for instance the entry from Mexico of the Hohokam into southern Arizona and the expansion south of the Sinagua out of the Flagstaff area into the Verde Valley and southern Arizona (Colton, 1946; Schroeder, 1975). Smaller scale moves of prehistoric times would include those of the Hohokam who established small colonies in central Arizona, small Chacoan colonies set up outside of the
the territory of the Chaco Branch such as the site at Chimney Rock, Colorado, and the small Kayenta group of northern Arizona which moved south and settled in the Point of Pines region of eastern Arizona (Ibid.; Eddy, 1977, pp. 48-59; Haury, 1958). These prehistoric migrations are deduced on the evidence of the pattern of material culture of one area appearing in that of a contemporary culture where the local pattern differs from that of the introduced set.

In the course of preparing background material on the Pueblo of Awa-tovi on Antelope Mesa, east of the Hopi Mesas, a number of traditions bearing on the settlement and/or related history of this pueblo were noted. In these traditions, references to groups arriving from the east and to the Asa (Tansy Mustard) Phratry are frequent. Since this phratry existed in a number of pueblos from the Rio Grande west to the Hopi villages, and since the events relating to the migrations of the Asa took place in historic times—and, as indicated below, appear to be quite accurate—an examination seems called for in regard to other Hopi Pueblo traditions of groups arriving from the east. If the times and places of events can be determined with any degree of accuracy, perhaps other contemporary events, perhaps related, might be interpreted in a better light.

Previous inquiry along these lines by Mindeleff (1891) and Fewkes (1898, 1900) has laid the groundwork for the interpretation of Hopi traditions. Today, we have more detailed data regarding ceramic chronologies and tree ring data by which to correlate and/or establish dates of traditional events and sites referred to within and without the Hopi region. On the other hand, details of the traditions, such as chronology, order of appearance of the groups involved, and directions of travel, also vary according to the clan membership of the teller or the mesa on which he lives. In addition, a comparison of the traditions as given almost 100 years ago with some of those recently recorded (Courlander, 1982) indicates that a number of changes and errors have crept in during the interim. Cattle and horses have been introduced in prehistoric contexts, some interpretations developed by archaeologists have appeared, and specifics are in error, such as Sikyatki being occupied when Awa-tovi was destroyed in 1700.

For this reason, the traditions as recorded in the late 1800s will be used in these discussions in order to eliminate more recent innovations or changes. Mindeleff's outline (1891, pp. 17-41), based on A. M. Stephen's recordings, will be followed in this attempt at reconstructing the time and place of events referred to. By avoiding the complications of differing details of several other clan traditions, the results may be skewed; but not until more traditions are examined in a similar manner will the relative degree of accuracy be established. Some of the most complex aspects are the moves of phratries or clans from one village to another after arrival in Hopi country. These occurrences are not treated to any degree in the review herein. The place of origin and settlements of these phratries are the major points of interest and investigation in the discussions below. In the text that follows, parenthetical statements rather than lengthy comments reflect attempts to identify what appear to be pertinent factors relating to the tradition's details.

According to these Hopi traditions, several groups from different directions came to the Hopi region at different times. It appears that those who are said to have had origins in the east are primarily associated with Antelope Mesa in one way or another. Other groups entering from the south, west, and north, most of whom will not be treated in these discussions, favored initial settlement in and around the three Hopi Mesas. These latter refer to themselves as Hopituh, but the term does not include the
people of Hano who came from the Rio Grande in the opening of the 1700s.

The acknowledged first arrivals (1100s), the Snake People according to Mindeleff’s outline, came from the north and west (Arizona strip—southwest Utah) (See Bannister et al., 1968, p. 20 on the late 1000 dates for a southwest Utah Virgin Branch type of structure—Schroeder, 1955, Fig. 3—in the Tsegi area, recorded by Beals, Brainard, and Smith—1945, Fig. 10—perhaps representing part of the general abandonment migration out of the Zion Canyon region in the 1100s). These people settled near Wepo Wash and later moved to the terrace on the west side of First Mesa at Kwetcap-tuwii. This west-to-east move seems to correspond with the 1100 to 1200 west-to-east reduction in the area occupied by the Kayenta Branch, which resulted in a concentration in the Hopi Mesas region. (See Colton, 1939, Figs. 11, 12).

The Cane Flute People, who later became associated with the Horn Phratry, came by way of Canyon de Chelly (late 1100s?) from a mountain range with snow, from the hillside of which one could see the plains with deer, antelope, and bison (ancestral Tewas, perhaps so named after reeds in the ponds on the east side of the Sangre de Cristo Mountains, an area that had undergone abandonment by about 1200?). While at Canyon de Chelly they were visited by Hopis from the south who lived on the edge of a beautiful canyon (in the Chavez Pass area?). These Cane Flute People first located on the brink of canyons near Keams Canyon.

The Bear People, possibly so named after bears in the Gallina country, the next to arrive, also came from mountains in the east not far from where the Cane Flute-Horn People had lived. After fighting with neighboring villages in their home area, these Bear People (possibly from the Gallina country where hostilities appear to have taken place and aban-
donments were underway in the 1200s) migrated west and also stopped at Canyon de Chelly, which was then unoccupied (early 1200s, the Cane Flute having already left). Here a split occurred, and one group went far to the west. Those who remained at Canyon de Chelly later moved across Chinle Wash to Eighteen Mile Spring before joining Firewood People (from Jemez?) at Fire House (Tebvuki-NA 996, Pueblo III ceramics), a short distance to the west.

The Bear People, however, moved out and took up residence at the head of Jeddito Canyon (possibly at Lululongturque, a large black-on-white site with an occasional showing of yellow ware—Hargrave, 1935). Later (1300s), some went to the rim of the canyon and then to Walpi which then—and when the Spaniards came (1540)—was located on the terrace of the mesa (Kuchaptuvela-NA1619, Pueblo IV-V ceramics). They then moved to the point of the mesa at Kisakovi (NA813, Pueblo IV-V ceramics).

The Firewood People also went to Walpi, where the Snake People allowed them to settle (late 1200s to early 1300s) at Sikyatki (Yellow House-NA814, Pueblo III-IV ceramics). Later, at Sikyatki's invitation, Coyote People from the north (who first came in the 1300s from the Tsegi Canyon region?) and Corn People, the latest of the Water People to come from the south (1400?) built on the mesa above to help protect Sikyatki in its then ongoing feud with Walpi concerning a recently established outlier below Sikyatki. Walpi, however, forced these two groups to abandon their sites, some going to Oraibi and others to Sikyatki. The latter was destroyed by Walpi (between 1450 and 1540?). Some of the Sikyatki men who had been absent during the attack took refuge at Oraibi and others at Awatovi. Soon after this event, the first attack by Utes occurred (early 1500s?). Later, Apaches were said to have made occasional attacks in Hopi country.

The Badger People who came from the Chama drainage of New Mexico (Tewa region) first settled at Awatovi (occupied from the 1200s to 1700—Smith 1952, p. viii). Later (1400), some Sun People came up from the Little Colorado River (possibly from the Homolovi Ruins area, sites of this drainage area having been abandoned about 1400) to join them. (The Awatovi dates suggest that the Badger People arrived in the 1200s or early 1300s if they were the first to settle there. Moreover, this was the period when the Tewas along the Rio Grande were consolidating into large pueblos—Wendorf and Reed, 1955. Perhaps some Tewas were being crowded out of arable lands along the rivers and moved west to the Hopi region.)

Another group in the Chama area, the Asa, perhaps named after the Tansy mustard plant of that area, left its village called Kaekibi by the Hopi (1590s to 1605, at the time pueblos along the Chama were being abandoned due to Apache attacks in the first decade of the 1600s and the droughts of the late 1500s. The latter also caused similar abandonments on the Pajarito Plateau—Hammond and Rey, 1953, p. 1059; Schroeder and Matson, 1965, pp. 131–134). These Asa migrated west by way of Tuwii (Santo Domingo) and Kaiwaika (Laguna, not recognized as a pueblo before 1697) where some Asa families stayed. After a short trip and stop at Acoma, the group proceeded to Zuni where it remained for a long time. On leaving Zuni (early to middle 1600s?), where the Asa left a number of people behind whom the Zunis call Aiyahokwi, they went to Awatovi (middle 1600s?) where they found the Badger People. The Magpie, Hunting Stick, and Field Mouse clans took up residence here. (Perhaps the crudely executed Hopi pottery known as San Bernardino Polychrome [1630–1700] resulted from this group, Tewa Polychrome of this period on the Rio Grande being crudely painted also.) The rest of the Asa
went on to First Mesa—where Walpi was on its present site (middle 1600s)—and built at Coyote Water just under the gap on the east side (see also Dozier, 1966, pp. 17-18, who ascribes a similar tradition to the Bear Clan). Tradition also states that some of the women at Walpi urged their men to move to the mesa top (middle 1600s?) in order to get away from the Spanish priests still living among them.

The Asa of Coyote Water later (middle to late 1600s?) repelled an attack by Utes and still later (middle to late 1600s?) another by Navajos (indicating that the latter were in the region prior to 1700, see below). In gratitude, the Hopis gave the victors a place on top of the mesa, and there they built on the site where Hano later was established on a permanent basis. After several seasons of drought, these Asa went (late 1600s) to Topkabi (Canyon de Chelly, where Hopi pottery of 1300 to 1700 occurs—Steen, 1966, p. 56) and lived among the Navajos. They planted peach seeds they had saved (which Navajos also credit to Hopis—Ibid, p. 55) and brought other new food plants. The Asa built their homes along the base of the cliffs. Some of the women married Navajos, their descendants constituting a new Navajo clan, the Kaini (High House People).

According to the Hopis, another group (Tiwas) left New Mexico at the time of the Pueblo Revolt (of 1680-1692) and arrived at Awatovi, where they held a great feast before building Payupki Pueblo on Second Mesa. Later after a feud with Mishongnovi, these people returned to the Rio Grande (in the 1740s, with priests from New Mexico) to settle at San Felipe (actually at Sandia—Harrington, 1916, p. 526). Since some of these who returned to the Rio Grande also settled at Abiquiu, I wonder if the latter might have been some of the Asa who went to the Hopi country in the 1590s and now were returning home.

While the Asa of Coyote Gap were in Canyon de Chelly (late 1600s) and before the arrival of the Tanos among the Hopi Pueblos (in 1700-1701), old feuds between various villages and Awatovi generated some of the enmities. Walpi, with the help of other villages, arranged to attack Awatovi and destroyed the pueblo (1700). Women and children captured were taken to various pueblos.

According to a Hano Pueblo Bear Clan tradition, the Tanos came from Tsawadeh Pueblo (Dozier, 1966, p. 17; also spelled Teedwadi by Fewkes—which seems to be the village called Tsawari Pueblo by Tewas—on the Santa Cruz Arroyo near Chimayo and east of Santa Clara Pueblo—Harrington, 1916, pp. 253-257). The Asa (Tewas) and the Hanos (Tanos) were said to have been friends in New Mexico (where they were geographically and linguistically close). Both are referred to as Hanomuh by Hopis. On their arrival, the Tanos built Hano on the site of the old Asa houses on First Mesa (which had been abandoned when the Asa went to Canyon de Chelly). After the arrival of the Tanos, the Asa in Canyon de Chelly, following a quarrel with the Navajos, returned (early 1700s) to Walpi on the summit.

The traditions briefly reviewed, along with the suggested dating of events and the interpretive remarks, seem to tie together fairly well. Unlike the Rio Grande Keres "migrations" mentioned in the beginning, which seem to refer to the movement of fetishes from one place or pueblo to another, the Hopi traditions seem to record the places of origin of the different migrations as well as a chronology of events.

This outline also has other implications. The sacking of Awatovi in 1700 represents a significant event among the Hopis. According to current historical interpretations, this unique occurrence appears to stem from a visit to Awatovi by some Spanish padres, supposedly leading to the conflict because of a clash between Catholicism and Hopi beliefs. However, the cause of the sacking may have had its origin in long term intertribal
hostilities. Not only do traditions indicate hostility did exist between the Hopi villages and those on Antelope Mesa, we also have other evidence to support this situation.

In 1540, the Zunis informed Coronado and his men that there were warlike people among the seven pueblos of Tusayan (Hopi area) to the west (Hammond and Rey, 1940, pp. 213–214, 253, 286, 299). It also was reported that there formerly were other pueblos there, but they had been destroyed by war (Ibid., p. 173). In 1583, the Espejo expedition reported only five pueblos: Oraibi, Walpi, Shungopavi, Mishongnovi, and Awatovi (Hammond and Rey, 1929, pp. 98–103). In short, two more pueblos had been abandoned between 1540 and 1583. One pueblo ruin referred to in 1583, one league from Awatovi, was reported to have been destroyed by Coronado’s men in 1540, though not one of Coronado’s many chroniclers even hint at such an episode.

This ruin, two and one-half miles from Awatovi, had to be Kawaiku Pueblo (Pueblo IV horizon ceramically, probably into the historic period—Smith, 1952, pp. 29, 262; with Kawaiku Polychrome, post-1450—Colton, 1956, Ware 7B). Tree-ring dates from this partly excavated site are as late as 1474 with an unknown number of rings missing (Bannister, Robinson, and Warren, 1967). The site of nearby Kokopnyama, with similar pottery associated, dates within the 1300 to 1600 period of the Jeddito and Sikyatki foei, and probably survived into historic times. These two pueblos would account for the decrease in other pueblos on Antelope Mesa in late prehistoric times for the same reason. The Hopis, it appears, were the warlike group mentioned by the Zunis in 1540. And perhaps because of a long term hostility toward the Antelope Mesa pueblos, Awatovi may have courted the Spanish priests in hopes that they would become allies to their cause against the Hopis to the west.

Several factors seem to support this interpretation of long term enmity. Awatovi, founded by groups from the east, the Badger People, undoubtedly absorbed refugees from the destruction or abandonment of Kokopnyama and Kawaiku, just as earlier refugees from Sikyatki, also founded by a group from the east, came to Awatovi.

When the Asa, another eastern contingent, arrived in the middle 1600s, some settled at Awatovi. The others went on to First Mesa where they were not accepted at first. Prior to 1700, they moved to Canyon de Chelly and did not return until after Awatovi was destroyed, and then they joined the Tanos from the Rio Grande who had come in a few years before and lived at Hano Pueblo.

Further indication of the "separateness" is the founding of Sichomovi in 1750. According to Fewkes (1900, p. 582), Walpi became overcrowded and the Bear and Lizard People began building at Sichomovi and were joined by Asa and Badger families, all but the Lizard being groups originally from the east. These factors seem to point to the Hopis as bringing about the demise of most of the Rio Grande immigrants on Antelope Mesa. And finally, the people on Antelope Mesa were not Hopis.

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Analyses of the Pueblo Revolt have appeared regularly since the late 19th century and one of the newer interpretations is quite similar to one of the oldest (Wilcox, 1981, pp. 394ff.; Bandelier, 1892b, pp. 101ff.). In presenting these, historians tend to choose a narrative framework and anthropologists a comparative one, but either choice is a legitimate one and the wealth of primary sources is what makes these studies possible at all (Hackett and Shelby, 1970).

Information for the decades before 1680 is less abundant and much more fragmentary than for later periods. The earlier records nonetheless can help us to sort out and document several trends that were present prior to the 1680 insurrection. Careful reading can also reduce the number of incorrect or unsupported assertions that have crept into the literature. This paper gives the findings from a pilot study of three topics: population trends, relations with the Apaches, and abandonment of New Mexico pueblos prior to the Revolt. These trends and events were not linked directly as cause and effect but neither were they unrelated.

The impact of Spanish colonization on native peoples of the Southwest is a current interest of many scholars (Fox, 1984; Riley, 1982; Wilcox and Masse, 1981; Zubrow, 1974). One well-recognized measure of this impact is the effect upon population numbers. Wilcox (1981, p. 397) pointed out that one study indicated a population decline and then a recovery by 1680, and another showed a gradual decrease through the 17th century, the result being a need for clarification. For New Mexico's Pueblo population, this need can be met by relying mainly upon actual counts or census data.

Father Escobar's diary gave a completely undorned picture of life in 1605, and his estimate that the provincies of New Mexico, whose people all lived in pueblos, must contain 30,000 souls or more. Four years later, in a similarly frank appraisal, Fray Francisco de Velasco said that "in more than one hundred pueblos there must be thirty thousand souls...not counting the many people living in rancherias" (Hammond and Rey, 1953, pp. 1012, 1095; Schaafsma, 1981, pp. 300-303). The first actual count may have been one by Fray Gerónimo de Zárate Salmerón ca. 1616. He said that there were 34,650 baptized souls, a number taken from the baptismal records (Milich, 1966, p. 35).

The next 15 years brought a substantial decline. A Pueblo census in ca. 1641 recorded 19,741 souls. Two years later Governor Alonso Pacheco de Heredia reported that he had counted 19,870 Indians in 43 pueblos (Scholes, 1929, p. 50; 1944). Vetancurt (1961, p. 269) mentioned a general census taken in 1660 that listed "more than 24,000 persons, small and large, Indian and Spanish." In one of his 1679 petitions, Fray Francisco de Ayeta said that "according to the registers, there are seventeen thousand Christian Indians, men and women" (Hackett, 1937, p. 299). This is confirmed by many passing references in the Pueblo Revolt documents to the number of apostates exceeding 16,000 (Hackett and Shelby, 1970).

Although there are many other estimates extending back to the time of the Coronado entrada in 1540, the figures just summarized are probably the most reliable. If so, then the
long-term population trend appears to have been actual stability among the New Mexico pueblos during the first 20 years or more of Spanish dominance, followed by a 30 to 40 percent loss in the 1620s and 1630s to a new level of around 20,000 individuals. This new total then remained relatively stable, declining by about 15 percent over the next 40 years. Kessell (1979a, p. 489) listed figures for the population of Pecos over this same period, but for most pueblos reliable data are probably not available. Why this decline in the Pueblo population came 25 to 40 years after the colonization in 1598 is not at all clear.

A NEW LOOK AT APACHE RELATIONS

In what must be one of the earliest references to the Navajo Indians, a 1679 document credited the Apaches Navajoes with forcing the abandonment of the first Spanish settlement at San Gabriel. This in turn led to the founding of Santa Fe ca. 1610 (Worcester, 1951, p. 104). The following three decades appear to have been generally peaceful, punctuated by occasional Apache raids on the Pueblos (Worcester, 1941; Reeve, 1957). From the 1650s on, New Mexico documents consistently played upon two themes—droughts and Apache hostilities—as leading causes of the province’s woes. The two, of course, may have been related (Scholes, 1930, pp. 400-401).

Yet in one of the earliest complaints, the father custodian and the definidores of New Mexico gathered at Santo Domingo on September 8, 1659, and wrote a long letter to the viceroy. They placed the blame for murders and pillaging by the unconverted natives squarely upon the governors of New Mexico and their expeditions to capture heathen Indians, who were then sold in the mines of El Parral (Hackett, 1937, pp. 186-187, 192; Worcester, 1941, pp. 10-12). The Indians understandably resisted and retaliated. This kind of abuse suggests that the ultimate cause of Apache raiding lay with the Spaniards themselves.

A decade later Fray Juan Bernal wrote about two calamities that had visited the kingdom, one being that the whole land was "at war with the wide-spread heathen nation of the Apache Indians," the second that for three years no crops had been harvested (Hackett, 1937, pp. 271-272). Yet in a petition dated May 10, 1679, the custodian Fray Francisco de Ayeta told us that "in the year 1672, the hostile Apaches who were then at peace rebelled and rose up, and the said province was totally sacked..." (Ibid., p. 302). About the end of August or the first of September, 1676, Ayeta laid his own petition before the viceroy, together with an opinion of the definidores and a report by the governor of New Mexico. In reviewing these, the royal fiscal, Don Martín Solís de Miranda, found that the Apaches and their allies "who having been at peace until the present time" were ruining the province, that they had completely destroyed five settlements (poblaciones) and gone on to burn churches, etc. (AGN; Hackett, 1912, p. 148). Four years later, in Governor Antonio de Otermin’s first comprehensive report after the Pueblo Revolt, he claimed that his news of the uprising

...was wholly contrary to the existing peace and tranquility in this miserable kingdom, not only among the Spaniards and natives, but even on the part of the heathen enemy, for it had been a long time since they had done us any considerable damage (Hackett and Shelby, 1970, p. 95).

Viewed in sequence, these claims add up to much less than the total sack and ruination of New Mexico. Prior to 1680, whenever a calamity visited New Mexico, we are told that the Apaches and their allies were quiet until that particular event. Whatever the Apaches’ actual role in unsettling conditions, they seem to have been convenient whipping boys for whatever...
ill befell "this miserable kingdom." It is notable, too, that the Apaches directed their hostilities at the mission establishments, the symbols of Spanish authority, and only indirectly at the Christian Pueblo Indians. The Pueblos maintained their own population with only a slight loss, and this loss may have been due to Indians moving to the El Paso area before the 1680 revolt (Bandelier, 1892a, pp. 267, 273).

The final twist to these inconsistent claims of Apache warfare was delivered by Father Ayeta himself, two days before Christmas in 1681. In a seeming turnabout from his earlier statements, he chastised the Spaniards for wishful thinking in expecting the rebellious Pueblos to submit to the Spanish in order to gain protection from "the pitiless warfare that the heathen Apaches would wage upon them." This hadn't happened, wasn't going to happen, and the Apaches in fact had not destroyed any pueblo or even damaged one seriously (Hackett and Shelby, 1970, pp. 306-308). With the Spaniards gone from the scene, the focus of Apache hostilities had been removed.

Is it probable, then, that the Apaches had an active part in causing the loss of a half-dozen or so New Mexico pueblos during the 1670s? Father Ayeta's 1679 remark that the Spaniards and other non-Indians capable of bearing arms scarcely numbered 170, of whom the governor could hardly gather 20 men at any one time, indicates that the Pueblos were largely dependent on their own resources in any case (Hackett, 1937, p. 299). The particular circumstances in which any of these abandonments took place are unknown or confused, nor is it certain when most of them occurred.

PUEBLO ABANDONMENTS
IN THE 1670s

Claims that certain pueblos were abandoned at specific dates, as Las Humanas in 1672 (Schroeder, 1979, pp. 237, 241; Hayes, 1981, p. 2; Brugge, 1984, p. 170), may have started with Bandelier's (1890, p. 131) allegation that Fray Juan Alvarez placed the loss of the six Salinas Pueblos after the 1672 massacre at the Zuni Pueblo of Hawikuh and immediately before the slaughter at Senectú in January 1675. Bandelier evidently misread Father Alvarez' 1705 letter, since the minister was indefinite as to the times of all of these events (Wilson, 1973, p. 25). More recent scholars perhaps misread Father Ayeta's May 10, 1679 petition where he said only that "from the year 1672 until your Excellency adopted measures for aiding that kingdom, six pueblos were depopulated..." (emphasis added). Again the time was indefinite, since Ayeta arrived back with these "adopted measures," namely with New Mexico's first permanent garrison of 43 soldiers, only around November 27, 1677 (Hackett, 1937, p. 297). No specific dates for pueblo abandonments can be confirmed, but alleged dates gain an undeserved credibility by being carried from one secondary source to another and help to confuse what may actually have happened.

The loss of Hawikuh may be the best documented event. The fiscal's opinion attached to Father Ayeta's 1676 petition cited the complete destruction of five settlements (unnamed) and later said that the Apaches killed Padre Fray Pedro de (Avila y) Ayala, ministro en el Pueblo de ajuico, on October 7, 1672 (AGN, PI, Exp. 7). Other writers including Vetancurt and Fray Juan Alvarez described the massacre of all the men, with the women and children being carried off and the church burned (Hodge, 1937, p. 99; Adams and Chávez, 1956, pp. 196-197; Wilson, 1973, p. 23). The best evidence is that these tragedies actually happened during the first half of 1673 (Adams and Chávez, 1956, p. 197), but there may also have been two separate events: one on October 7, 1672, and another in 1673. If the mission was rebuilt prior to 1680, then it was destroyed again, since Hodge (1937, p. 101) found scarcely a piece of wood in the church or convento that had not been reduced to charcoal.
Senectú, a Piro Indian Pueblo, lay along the Rio Grande below Socorro. Travelers along the Camino Real mentioned the ruins of Senectú during the 18th century but the river silts have probably covered the site. Bandelier (1892a, p. 250) summed up much of what is known about the loss of this pueblo. According to Father Ayeta and the fiscal, Solís de Miranda, the missionary at Senectú, Fray Alonso Gil de Ávila, died in his own convento from the effects of five arrow wounds suffered in an Apache attack on January 25, 1675 (AGN, PI, Exp. 7; Hackett, 1937, p. 297). Father Ayeta however included Senectú as one of the four pueblos depopulated in the one and one-half years between his departure from New Mexico for Mexico City sometime in the spring of 1676 and his arrival back with the badly-needed aid ca. November 27, 1677. The murder of Father Gil in 1675, therefore, had no connection with the abandonment of Senectú in 1676 or 1677. Upon Ayeta's return, he immediately set about resettling the place. It was still occupied at the time of the Pueblo Revolt.

Another Franciscan, Father José Arlegui, told of a somewhat different story in his 1737 history of the missionary province of Zacatecas. According to Arlegui, a Father Alonzo Gil...and some Spaniards were besieged in the church at an unnamed pueblo. The priest appeared at the window with a crucifix in an attempt to pacify the natives and was shot in the breast with an arrow. He died shortly afterwards while the other Spaniards in the church were soon aided by some soldiers and all escaped (Hackett, 1912, p. 148). As Hackett noted, the father minister was undoubtedly the same person in all of these passages, and Arlegui therefore described a siege at Senecú. The entire tenor of Arlegui's narrative was of a native revolt and not an Apache attack. Ayeta, for reasons of his own, said that the Apaches were responsible for Father Gil's death, but Ayeta blamed the Apaches as a matter of course. The timing, January 25, 1675, was about six weeks before Governor Juan Durán de Miranda's second term ended. This could mean that Father Gil's demise actually occurred during the abortive Pueblo Revolt which ended with Don Estevan Clemente meeting the hangman (Kessell, 1981).

Father Ayeta listed Las Salinas, Cuarae (Quarai), and Chililí as the other three pueblos depopulated during the year and a half that he was in Mexico, i.e., in 1676-1677. About Quarai and Chililí we are told no more, except that in 1759 a Tano Indian repeated to the governor what he had heard: that those who moved from Quarai had joined the Indians at Tajique (Wilson, 1973, p. 26; Kessell, 1979b). Vetancurt (1961, p. 279) said that Tajique was occupied to the time of the Revolt, but there is no independent evidence for this. Las Salinas was resettled, and this frontier garrison held as late as 1679 although the people there suffered from hunger and repeated attacks by the Apaches and their allies (Hackett, 1937, pp. 297-298). The Indians and the soldiers probably stayed until their supplies ran out, then left for good.

Abó's last days lie in obscurity, as Toulouse (1949, p. 4) has said. This Tompiro Pueblo at the southern end of the Manzano Mountains had a mission as recently as 1672, when the Chapter elections that summer assigned Fray Alonso Gil de Ávila as the guardián there. Ayeta told us all that is known about the Pueblo's last days, including the bizarre circumstances of the murder of the missionary, with Ayeta implying that the Apaches committed these atrocities. The martyr was apparently not Fray Pedro de Ayala, whom Ayeta named, since he perished at Hawikuh. Abó and Las Humanas (Gran Quivira) were among the six pueblos depopulated from the year 1672, but they were not listed with the four sites abandoned between spring 1676 and late fall 1677. This dates their loss to sometime between 1672 and 1676 (Hackett, 1937, pp. 297-298).
Out of eight pueblos abandoned in the decade prior to 1680, all were lost in the five-year span from 1673 to 1677. Only two—Senecti and Las Salinas—are known to have been resettled. The Apaches have been blamed for destroying the missions, but the Apaches' role is poorly documented and the attacks seem mainly to have been directed at the friars and the visible symbols of the mission enterprises. The pueblo abandonments in the Piro and Salinas provinces may have coincided with other events than Apache hostilities.

On December 22, 1681, Governor Antonio de Otermin conducted a series of judicial proceedings. One of his witnesses, the sargento mayor Diego López Sambrano, said a good deal more than he probably should have. He outlined the attempts at Pueblo revolts and the punishments meted out by various governors over the past 40 years. Pedro Naranjo, an 80 year old Keres Indian with the reputation of being a "consummate sourceer," confirmed as much in his own testimony. Naranjo of course had been a conspirator in the earlier efforts, and in his experience these had failed for want of leadership and organization (Hackett and Shelby, 1970, pp. 245-246, 298-301, 308-309). In 1650 the plotters actually conspired with the Apaches to destroy the whole kingdom.

The Piros kindled one of the earlier rebellions during the governorship of Captain don Fernando de Villanueva, who held office from June 1665 to the end of November in 1668. One of the Pueblo crimes then according to Diego López Sambrano was an ambushade "with the enemy Apaches in the Sierra de la Madalena" where they killed five Spaniards, among these the alcalde mayor (Hackett and Shelby, 1970, p. 299; Scholes, 1975, p. 20). Although this alcalde mayor's name was not stated, the jurisdiction would have been that of the Piros. In 1665-1667 Luis López was alcalde mayor of the jurisdictions of La Isleta and the Pueblos of Los Piros (Scholes, 1937, p. 140; Hackett, 1937, p. 276; Chávez, 1973, p. 58). Luis López was probably not removed from office, since he evidently served Governor Villanueva, but he is not heard of after 1667. Presumably he was the murdered official. If so, then the attempted Piro revolt would have taken place in 1667 or early in 1668, right at the time when Fray Juan Bernal claimed that the whole land was at war with the Apaches, that no road was safe, and (in 1668) the Indians were perishing of hunger and lying dead along the roads (Hackett, 1937, pp. 271-272). Diego López Sambrano's testimony provided the clue that Father Bernal's version of events which included nothing about a native revolt, was less than the complete story.

Sometime between this episode and the beginning of Governor don Juan Francisco Treviño's term, that is before March 1675, a general conspiracy headed by Don Esteban Clemente "Indian governor of all the pueblos of Las Salinas...whom the whole kingdom secretly obeyed..." failed on the plot being discovered. Don Estevan was hanged for his trouble (Hackett and Shelby, 1970, pp. 299-300; Kessell, 1981). This period coincides with Father Ayeta's chronicle of misery: famine in 1670; pestilence in 1671; Apache uprisings in 1672—plus the abandonment of Aló and Las Humanas between 1672 and 1676 (Hackett 1937, p. 302). The conclusion here is that at least one native revolt was also involved and that Ayeta knew more than he chose to tell. Undoubtedly both he and Fray Juan Bernal worked with the same purpose in mind, which was to make a convincing case for aid that might save the missionary effort. Full disclosures were not required.

Other types of information can add to our understanding. For example, the paleoclimatic tree-ring chronology for Chupadero Mesa displays negative departures from the standard index for the decades 1660-1669 and
1670-1679, a reflection of lower than normal precipitation and above normal temperatures (Dean and Robinson, 1977; Levine, 1982, Fig. 1). Assertions of drought by Ayeta and Bernal were true enough, although it is difficult to judge the impact. In an area without irrigation potential such as the province of Las Salinas, this moisture deficiency may have been critical in forcing an end to settlement. During the early 20th century homesteaders in this marginal farming country had the same experience (Levine 1982, pp. 122-124).

CONCLUSIONS

The conclusions drawn here are that many present understandings about population trends, group relationships, and settlement abandonments in pre-rebellion New Mexico need revision. Population figures show a substantial decrease in the Pueblo population between ca. 1620-1640, but after that nothing to indicate a major impact due to Apache raiding, drought, famine, or pestilence.

The Spaniards themselves may have started hostilities with the Apaches but the actual extent of damage by Apache raiding is questionable. Spanish emphasis upon constant attacks by hostile Apaches may have served to obscure other less palatable tendencies, namely exploitation of the native Pueblo peoples beyond what they could tolerate—to the point where revolts began. The Apaches were hardly in a position to reply to accusations made against them, while reports charging the Apaches with ruining New Mexico were more likely to elicit support in Mexico City than was news of internal revolts. Native rebellions may have been responsible, as much as Apache warfare, for some of the Pueblo abandonments in the 1670s.

Las Cruces, N.M.

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Our own Taos Pueblo shares a mystery with an area of the Basque country in Spain, theirs concerning the small statue of Nuestra Señora de Aránzazu, and that of Taos concerning a painting of this same Nuestra Señora. This information is being published in the hope that some of the readers may be able to furnish clues leading to the rediscovery of this painting.

In 1469, near the present village of Aránzazu in the northern Basque country of Spain in the Pyrenees Mountains, an eight-year-old sheepherder heard the sound of a lead pack mule's bell in the thorn bushes near his flock. When he went to investigate he fell to his knees in awe, as there in the bushes was a small stone statuette of a seated Virgin Mary holding the Christ child on her left arm and an apple or pomegranate in her right hand. The particularly ugly nude baby Christ child, typical of the period, was holding a smaller apple or world on his left knee, his right hand seemingly broken off.

Rodrigo de Balzategui, the young sheepherder, covered the statue with branches, then ran downhill to the town of Oñate and into the chapel where he told the priest of his discovery. So the Nuestra Señora and her Niño were rescued and protected in that chapel until such time as the Franciscans took over the sect of Nuestra Señora de Aránzazu.

Later, this Nuestra Señora became the patron saint of the mariners of Spain and many miracles at sea were attributed to her. Only an area which is now a National Forest lay between Aránzazu and the Bay of Biscay to the north, and there many of the early explorer ships of Spain were built. No doubt many Basques joined these ships to try their luck in the new country of Mexico—or "Viscaya" as it was named on the old maps—which then included New Mexico.

The Franciscan priests also came and brought the sect of Aránzazu with them, which helps to explain why we find that Governor Captain General de Vargas, making his third expedition to Taos on September 24, 1696, received from the former Sacristan Felipe, of the Taos Pueblo mission church, an oil painting of the Blessed Virgin of Aránzazu, in size approximately 57.75 by 41.25 inches.

This painting is never mentioned again! Where is it today? What has been its history after being packed with corn from Taos Pueblo and returning to Santa Fe with de Vargas? Was it placed in his personal chapel at the east end of the Palace of the Governors? The chapel having been destroyed, was the painting returned to Spain when de Vargas returned to see his family and was it placed in the family chapel there? Or is it in the basement of one of our museums in Santa Fe?

A translation from the military journal of de Vargas states:

On the said day, month and year, at about four in the afternoon, the above mentioned Felipe, the Sacristan, being urged by me, said Govr. and Capt. Genl., with the kindest words, told me he would deliver the Blessed Virgin of Aranzazu, which he has hidden in his house, with some valuable altar ornaments of the Church, missals and books, and so I ordered the lieutenant general of cavalry and the magistrates and captains, Lázaro de Misquía and Diego Montoya, with a party of soldiers to go with him the said Indian, Phelipe, to his house, which is on the other side of the river, in the quarters of the Teguas as he is of that nation, and to bring Our Lady, aforementioned, as was due, and, therefore, I gave them the said detachment and some other persons who
were of the said camp, chosen as excelling in her Devotion, and they brought the said Blessed Image, the Commanders and Officers with the remainder of the said Men-at-Arms going out to receive Her, beyond the Parade Ground, and saluting with repeated shots from the Arquebuseers, and falling upon my knees, I received the said Image, which I took and placed in my tent. It is painted on a canvas about a yard [sic] and three quarters high and a quarter [sic] wide...(Grant, 1984, p. 45).

This translation appears in a first paperback edition of Blanche C. Grant's Taos Indians, reprinted from a Rio Grande Classic of 1925 lent to the publisher by Homer Hastings of Santa Fe, New Mexico. For some reason, the second "vara" (the measurement used in the original) was left out of the translation quoted in Grant's book. It should read "a vara and three quarters high and a vara and a quarter wide."

This is highly important because "vara" is a measurement which at that time was 33 inches, and I wish this to be noted so that the correct size—57.75 by 41.25 inches—may be known to all and, hopefully, the Nuestra Señora painting may be found!

My information on Rodrigo de Balzategui comes from the official book on Aránzazu purchased in the gift shop at the Aránzazu basilica in November of 1983. The book is Historia de la Virgen y del Santuario de Aránzazu by R. P. José-Adrián de Lizerralde, printed in 1950, a year after the historian died.

Lizerralde's book contains illustrations of Nuestra Señora de Aránzazu and the different robes she wore from the 1600s to her coronation by the Catholic Church on June 6, 1886. Her large crown, seen in modern photographs of the sculpture, dates from 1886. The statuette has been threatened by fire three times in 300 years.

Corina Santistevan from Cordillera, two miles west of Ranchos de Taos, and Mrs. Ruth Marie Colville from Del Norte, Colorado, accompanied me in the fall of 1983 to Lisbon, Portugal, where we rented a car and went on to Santiago de Compostela and the famous pilgrimage route backwards towards France to Aránzazu. My two companions went back the next day to talk with Fray José María Celaya who gave them his address and phone number to contact for information: Fray José María Celaya, Franciscano Santuario de Aránzazu, Oñate, Guipúzcoa, España, telephone 78-07-97 or 78-09-51.

All the Basque country in this area is called Viscaya and, as Corina pointed out, the area of New Mexico and Arizona until separated in 1863 was called Viscaya on the old maps (see Carl Wheat's Trans-Mississippi West, 6 volumes of maps through the railroad period, the 1880s).

Similar, too, are many of the family names. The following are some of the Basque names Corina recognized in Lizerralde's book, the "indice onomastico," also found in the Taos area: Abad, Agueda, Aguirre, Baltazar, Bernal, Castillo, Cerain (St. Vrain), Cisneros, Cruz, Diaz, Delgado, Durango, Flores, Gomez, Gonzales (or -ez), Guerra, Hernandez, Hurtado, Jimenez, Juana, Blas Lopez, Larrinaga (Basque painter from California), Martinez de Zoazo (Suazo, seven different Martinezes, Miranda, Mondragon, Ortega, Ortiz, Ponce de Leon (hot springs in Miranda Canyon named by Pascual Martinez), San Esteban (Taos, Santistevan), Juan, Santa Clara, Santa Cruz, Santiago, Vasquez.

I mention the above names in the hope that some who bear them, even today, may remember Nuestra Señora de Aránzazu and be able to give us a clue as to how an oil painting of her measuring 57.75 by 41.25 inches landed in the Taos Pueblo mission church before the revolt of 1680. It was during the revolt that the Sacristan Felipe saved her and many other
objects from the altar before the altar was destroyed and the Franciscan priest killed. The church, itself, was not destroyed but, as General de Vargas said, used as a stable. He requested that the altar be reinstated and he and his Franciscan followers rebaptized the Indians of the pueblos they visited.

All this may be found in the journal of Don Diego de Vargas Zapata y Lujan at the New Mexico Record Center.

It is a shame that Spain is now building a big highway winding its way from the town of Oñate to the Santuario and its 1950 basilica. On the old road, we drove past resting bulldozers and parts of widened road. A big parking lot in front greeted us on the Sunday morning in October, 1983, when we drove up. Mass had already started—there must have been at least a thousand local people there. While the choir sang in Basque, two projections of the songs were thrown on the wall on either side of the altar, one in Spanish and one in the Basque language. This went on for a half hour.

High above the altar in a niche sat Nuestra Señora—so tiny and so high one could only make out a white spot illuminated by a spot light. After Mass, several hundred people walked up steep steps to where one could see her from 10 feet away. The lead mule bell under her feet, attached to the sculptured thorn bush base of her stone seat, was the original one found and is almost as big as the statuette. The bell has been authenticated, as well as the stone from central France from which she was sculpted.

When we left we went no further north nor northwest than Barcelona, which was not so far away, but returned in one day's drive to Madrid, where we turned in the Renault station wagon and proceeded for 10 days on a Spanish bus tour of southern Spain to Ronda, Granada, and Seville—all shown us by local historian tour guides from each city. The weather was perfect and there were no big crowds.

So begins the quest for the oil painting of Nuestra Señora de Aránzazu. Let us hope that it will be rediscovered.

ACKNOWLEDGMENTS

I wish to express my thanks for the correction from the original journal of de Vargas, given to me by the Record Center of New Mexico's director, Dr. Hordes, and researched by Ms. Smith-Gonzales, in April of 1984.

Also, I thank Ruth L. Hatcher, Taos, New Mexico, who first edited this paper.

Ranchos de Taos, N.M.

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PRIMARY SOURCE

New Mexico State Records Center and Archive: SANM II 54a and 54b (journal of events of Don Diego de Vargas, October-December, 1693).
Figure 1.
During the 1850s, pioneers from the villages of Manzano and Torreon, on the eastern slope of the Manzanos near Albuquerque, established several small communities along the Rio Bonito in southcentral New Mexico (Caperton, 1983, pp. 8-10; Figures 1-3). By the early 1860s the largest village in the Bonito Valley was La Placita, which later became Lincoln. In later years Lily Casey Klasner described La Placita as it appeared in 1868:

Scattered along two sides of a crooked stretch of road were twelve or fifteen houses which might be credited with a certain degree of pretension. They were adobe, flat-roofed, angular in shape, and one story in height. In the rear of the houses fronting the street were a number of jacals, that is, houses of a simpler type made of standing up cedar or juniper posts and daubing them together into walls by applying mud. Both sorts of houses were plastered on the outside with mud and whitewashing inside with a gypsum solution called by the Mexicans jaspe.

The population of the town was predominately Mexican, most of them getting their livelihood by farming tracts up and down the valley of the Bonito which flows by the edge of town and furnishes abundant water for irrigation. The business section of La Placita consisted of a few stores. Jose Montano [Montaño] had a store and a saloon, and Jacinto Sanchez a store in which Antonio Sedillo worked. Captain Saturnino Baca, who later became one of the conspicuous citizens, had not moved into town, but was living some miles to the west in the vicinity of the old Antonio Torres place. Mariano Trujillo was the most prominent citizen of the village...Alec Duval ran a saloon which was a branch of the L. G. Murphy & Co.'s store. Pete Bishop, a large bald-headed old man, ran a regular saloon in town; that is, his place was exclusively for the dispensing of alcoholic liquors (Klasner, 1972, pp. 53-55).

In 1869 the Territorial Legislature created Lincoln County, and La Placita was designated the county seat. Its name was changed to Lincoln (Keleher, 1962, p. 33). In 1870 there were 588 people living in the voting precinct of the new county seat (U.S. Census: 1870; Figures 4, 5).

In May of 1876, Mesilla attorney John D. Bail filed an application for patent on a 40 acre tract that contained the future McSween house site as well as a large portion of the town of Lincoln (Bail, 1876). The tract also contained the L.G. Murphy & Co. store—a large mercantile that was built in 1873 (Caperton, 1983, pp. 133-136).

The store was established by ex-Major Lawrence Gustave Murphy of Wexford, Ireland, and ex-Lieutenant Colonel Christian Adolph Emil Fritz of Stuttgart, Germany. They had served together at Fort Sumner and Fort Stanton and operated a sutlers store at the latter post—nine miles upstream from Lincoln (Fulton, 1968, pp. 45-47; Keleher, 1957, pp. 32, 51-52). In the late 1860s, Murphy and Fritz established a store in La Placita (Klasner, 1972, pp. 53-54) and were involved with a lucrative contract beef business with the U.S. Government. Fritz died in 1874, and Murphy took on James J. Dolan as a partner. Dolan was from Laughrea, Ireland, and had served at Fort Stanton (Keleher, 1957, pp. 184-187, 230-231; Figures 6, 7).

Murphy claimed that he had attempted to procure title to the land earlier, but through the "willful
negligence" of his attorney, the transaction was not completed. In an affidavit, Murphy stated that Bail "made said purchase for the purpose of being avenged...and for no other purpose" (Murphy, August 17, 1876). The matter was settled when Bail secured a patent on the tract on November 26, 1876 (Lincoln County Records, Patent Book F, pp. 342-343) and sold it to Murphy on the same day (Lincoln County Records, Contract Book B, pp. 24-26; Deed Book B, pp. 4-5).

Alexander A. McSween (Figure 8) and his wife, Susan (Figure 9), moved to Lincoln in 1875. Alexander was reportedly from Nova Scotia and Susan was from Gettysburg, Pennsylvania. Alexander was a lawyer and performed work for Murphy & Co. (Fulton, 1968, p. 55).

In 1876 McSween met a young Englishman named John H. Tunstall in Santa Fe. Tunstall had spent a three year apprenticeship in a family owned mercantile in British Colombia and was ready for a more challenging career. As a result of that meeting, Tunstall came to Lincoln in the fall of that year. He quickly realized that it was just the place to further his ambitions. He and McSween established some sort of partnership. They purchased a herd of cattle and grazed them on public domain. They also realized the potential of starting an enterprise that would rival Murphy & Co. (Fulton, 1968, pp. 57-58; Keleher, 1957, pp. 54-55; Nolan, 1965, pp. 46-104).

In January of 1877, McSween purchased a portion of the original Bail patent from L.G. Murphy. He also obtained a quit claim deed from Saturnino Baca, who had been residing on the land for several years (Baca, 1879, p. 462; Lincoln County Records, Deed Book B, pp. 6-7, 8-9).

McSween then reportedly converted a structure on the property, which may have been Murphy's old La Placita store, into a two-family dwelling (Nolan, 1965, p. 233).

In the meanwhile, Murphy sold out his interest in the company to J.J. Dolan and John H. Riley (Lincoln County Records, Contract Book B, pp. 50-53, 60-63; Deed book B, pp. 10-11). Fellow Irishman Riley had been a clerk for Murphy & Co. and served at Fort Stanton (Keleher, 1957, p. 56).

By August of 1877 the McSweens and the recently arrived Shield family, consisting of Susan McSween's sister, Elizabeth (Figure 11), and her husband, David (Figure 12), and their five children. David Shield was also an attorney, and he went into partnership with his brother-in-law (Fulton, 1968, p. 233).

Tunstall constructed a new flat roofed adobe about 30-40 yards east of McSween's residence to house his mercantile, living quarters, a law office for McSween, and the new Lincoln County Bank (Bonney, 1879, p. 327-328; Fulton, 1968, p. 60; Hinton, 1956, pp. 193-196; Figure 13).

Tunstall revealed his intentions in a letter to his father: Everything in New Mexico is worked by a "ring". My ring is forming itself...I propose to confine my operations to Lincoln County, but I intend to handle it in such a way as to get half of every dollar that is made in the county by anyone (Tunstall, 1877).

The bank was to be a vehicle for Tunstall's credit system of grain notes. The stationary listed John S. Chisum as president, McSween as vice-president, and Tunstall as cashier. Cattle baron Chisum was Murphy & Co.'s main competitor for army beef contracts (Fulton, 1968, pp. 38-40; Nolan, 1965, pp. 213, 225, 275).

McSween's home was U-shaped with its two wings running north or away from the road (Dudley, 1879b; Figure 14). The adobe house contained at least 10 rooms and the two family quarters were probably separated by an internal wall (Lincoln County Record, Deed Book B, pp. 33-35;
INDEX OF SITES

1. WORTLEY HOTEL
2. WATSON HOUSE
3. FREQUEZ HOUSE
4. TUNSTALL STORE
5. TORREON
6. CONVENTO
7. SAN JUAN CHURCH
8. SHERIFF BRENT HOUSE
9. WPA SCHOOL
10. COURTHOUSE
11. PAGEANT GROUNDS

TOWN OF LINCOLN, NEW MEXICO 1980
Crawford, 1938; Dudley, 1879b). There was a kitchen at the north end of each wing (Boyle, 1879, pp. 627-628; Long 1879, pp. 592-593). The windows had shutters (Eastin, 1879, p. 173), and the building had a flat roof which consisted of boards overlaid with dirt (Chaves, 1879, p. 339). The McSweens lived in the west wing of the house and the Shields in the east wing.

An adobe wall ran along the property north of the house. The wall was connected to the main structure, with walls running north-south from the east wall of the west (McSween) kitchen and the west wall of the east (Shield) kitchen. These connecting walls had doors or gates in them and there was a gate in the center of the north wall. The connecting walls and the north wall formed a "plaza" from the U-shaped portion of the structure. There was a chicken house next to the McSween kitchen, and the wall north of the house formed its north wall (Boyle, 1879, p. 653; Dudley, 1879b; Nash, 1879, p. 679). Outside the north wall, to the north and northwest of the west wing, was a stable and corral. Part of the stable was below the level of the yard on the slope that leads down to the Rio Bonito (Dudley, 1879b; Nash, 1879, p. 655; Pierce, 1879, p. 610). Apparently the adobe wall extended to the east of the Shield residence to a privy. There was an irrigation ditch about 15 feet north of the privy (Long, 1879, pp. 594, 615).

A plank fence ran along the west side of the house (Boyle, 1879, p. 651), and the fence on the east side was described as having two or three boards or poles nailed horizontally to posts. There were two gates in the east fence. The east and west fences were connected to the adobe wall to the north of the house (Boyle, 1879, pp. 649, 651; Dudley, 1879b; Pierce, 1879, p. 649). There was also a low fence in front (south) of the house (Peppin, 1879, pp. 547-548). According to a map of the site, there were six rooms, including kitchen, in the west.
Figure 5. Lincoln, New Mexico, looking northeast about 1886.

a - Murphy-Dolan Store (became the county courthouse in 1880); b - Watson House; c - Tunstall Store. The McSween House site is between b and c. J. R. Riddle Photograph. Photo Archives: Museum of New Mexico.

wing. The east wing had three rooms, and a large room connected the two parts of the dwelling (Dudley, 1879b).

On August 6, 1877, soon after the Shield's arrival, the Sunday peace was shattered by the following incident as described in a contemporary account:

...Frank Freeman, a desperado who had been compelled to leave the country for the cowardly shooting of a soldier about a year ago, arrived from Texas accompanied by others like himself, amongst them a man named Armstrong. Freeman accompanied by a would be desperado, Charles Bowdre, came to the town of Lincoln on Sunday evening and compelled Jose Montaño (sic) to open his store and forced him to give them liquor; they commenced smashing things generally, breaking lamps, bottles, glasses, etc., meeting with an innocent Sergeant from Fort Stanton and there being no other victim at hand, Freeman who had one of his murder fits on, took the Sergeant by the hand in a friendly manner, and with the other hand reached around and placed the muzzle of his pistol at the back of the Sergeant's head and fired: fortunately the wound was not mortal, but Freeman, supposing that he had killed the Sergeant accompanied by Bowdre rushed through the streets of town shooting at every object that met their view. Freeman shouting that his "name" was Frank Freeman, and no twenty men could arrest him, that "he had had his man for all meals in the day and that he intended to kill every man in town that he didn't like," thus they paraded the streets shooting, and yelling like demons. After a while another idea seemed to strike them, coolly re-loading their weapons
they proceeded to the residence of A. A. McSween Attorney at Law, a prominent and good citizen, where John S. Chisum was stopping as a guest. Two ladies and five children were also in the house, they fired a number of shots promiscuously in the house, broke a window open and forced an entrance. The inmates fled for shelter, the ruffians then riddled a sewing machine with bullets, and shouted that "if John S. Chisum or his corpse was not turned over to them they would burn the d-d house down"; about this time a Mexican servant in the employ of Mr. McSween obtained a firearm and discharged it at the ruffians slightly grazing Freeman in the arm whereupon they ingloriously fled (The Mesilla Valley Independent, September 8, 1877).

Freeman was killed by a posse the following week (Ibid.).

A consuming hatred developed between Tunstall-McSween and Dolan-Riley that would affect the entire county. The emotions may have had their roots in the troubled relationship between the English and Irish in the old world. According to a Lincoln resident of the period, "Both parties employed all the outlaws and ruffians they could recruit, who made no distinction when they could get a chance to steal" (Taylor, 1879).

Soon after the Tunstall-McSween-Chisum alliance was formed, a well organized band of rustlers decimated the Chisum herds. They also began to prey upon the Tunstall-McSween cattle (Hinton, 1956, pp. 192-194; Keleher, 1957, pp. 38-53; Nolan, 1965, pp. 225, 240, 245, 251, 275).

McSween refused to turn over the proceeds of a $10,000 insurance policy belonging to Emil Fritz to the heirs, Charles Fritz and Emilie Scholand, who were the deceased Fritz' brother and nephew. Dolan also thought he had a claim to the money, for he was the successor to Murphy & Co. in which Emil Fritz had a half interest (Fulton, 1968, p. 95).

On December 4, 1877, McSween sold the east wing of his home to his sister-in-law, Elizabeth Shield, for one dollar. The deed mentioned that five rooms were conveyed to the Shield and that the two households were divided by an internal wall (Lincoln County Records, Deed Book B, pp. 33-35).

A few days later A. A. McSween was arrested on charges of embezzlement in Las Vegas while on his way to St. Louis on an alleged business trip (Fulton, 1968, pp. 96-98; Nolan, 1965, p. 276).

McSween was arraigned in the District Court in Mesilla and the trial was set for the April term in Lincoln. He could not furnish sureties for the bond and was returned to Lincoln in the custody of a Las Vegas deputy. He was to be turned over to the Lincoln sheriff—William Brady. Brady was also a native of Ireland and had served with the New Mexico Volunteers where he attained the rank of Major (Lavash, 1982; Figure 15). McSween managed to convince the Las Vegas deputy that if he were turned over to Brady his life would be endangered. Thus McSween was placed under house arrest.

On the 18th of February, Brady served a Writ of Attachment on McSween's household goods (Keleher, 1957, p. 62; Nolan, 1965, pp. 268-270). Brady's inventory of the McSween property was as follows:

List of articles inventoried by Wm. Brady Sheriff in the suit of Charles Fritz & Emilie Scholand VS A.A. McSween now in the dwelling house belonging to said A.A. McSween.

Articles in the Parlor
One Parlor Organ
1 Mirror
5 Pictures
1 Sofa
1 Center Table
Figure 6.
Standing, John H. Riley (left). Seated, Christian Adolph Emil Fritz (left) and Lawrence Gustave Murphy (right). Photo Archives: Museum of New Mexico.

Figure 7
James J. Dolan. Photo Archives: Museum of New Mexico.

Figure 8
Alexander A. McSween. Photo Archives: Museum of New Mexico.
Figure 9
Susan McSween. Photo Archives: Museum of New Mexico.

Figure 10
John H. Tunstall. Photo Archives: Museum of New Mexico.

Figure 11
Elizabeth Shield. Photo Archives: Museum of New Mexico.

Figure 12
David Shield. Photo Archives: Museum of New Mexico.
<table>
<thead>
<tr>
<th>Articles in Sitting Room</th>
</tr>
</thead>
<tbody>
<tr>
<td>6 Chairs</td>
</tr>
<tr>
<td>1 Corner Rack</td>
</tr>
<tr>
<td>2 sets of Window Curtains &amp; fixtures</td>
</tr>
<tr>
<td>Carpet on the Floor</td>
</tr>
<tr>
<td>One Book</td>
</tr>
<tr>
<td>Lot of Music</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Articles in East Bed Room</th>
</tr>
</thead>
<tbody>
<tr>
<td>One Bureau</td>
</tr>
<tr>
<td>One Wash Bowl &amp; Pitcher</td>
</tr>
<tr>
<td>Two Mattresses</td>
</tr>
<tr>
<td>One Washstand</td>
</tr>
<tr>
<td>Two Bedsteads</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Articles in Bed Room</th>
</tr>
</thead>
<tbody>
<tr>
<td>One Bureau</td>
</tr>
<tr>
<td>One Table</td>
</tr>
<tr>
<td>One Bedstead</td>
</tr>
<tr>
<td>One Wool Mattress</td>
</tr>
<tr>
<td>One Straw Mattress</td>
</tr>
<tr>
<td>One Washstand</td>
</tr>
<tr>
<td>Carpet on Floor</td>
</tr>
<tr>
<td>One Loose Carpet</td>
</tr>
<tr>
<td>One set window curtains</td>
</tr>
</tbody>
</table>

(Crawford, 1938).

While Brady was conducting the inventory of the McSween household, a posse was dispatched to the Rio Pecos to place an attachment upon the McSween-Tunstall livestock. Tunstall rode out to his cowcamp and rounded up some of his prize horses. While he and a small group of his men were apparently driving them to a place where they could be hidden, they met up with members of the posse. Tunstall was killed (Lavash, 1982). McSween went into hiding.

At noon the next day a missionary party arrived at the McSween place. They had been requested several months before. The party included Taylor F. Ealy, Presbyterian missionary and physician, his wife and two children, and Susan Gates, a teacher (Fulton, 1968, p. 168).

Dr. Ealy's diary clearly revealed the tense situation in town:

Feb. 20, '78 Lincoln, Lincoln County, N.M. This is truly a frontier town-warlike. Soldiers and citizens armed. Great danger of being shot.


While McSween was in hiding he turned his house over to the missionaries who occupied the place on the 28th of April (Ealy, 1955, p. 37).

On March 1, in a letter to his mother, Ealy reported:

Your letter is finding us keeping house. Mr. McSween is a lawyer—whose wife is at present in St. Louis—has kindly allowed us to occupy his house for the present.

At present we have all kinds of kitchen furniture—but of course it is not our own. Mr. McSween has about 70 chickens. We boarded a week with Mr. Shield, his partner in law (Ealy, 1878a).

In a diary entry of March 8, Ealy mentioned that, "McSween is having a wall 12 feet high built of stone and adobe about his house" (Ealy, 1955, p. 38). Apparently only the north wall was completed.

On March 19, Ealy wrote a friend that:

McSween refuses to go to jail. He is persecuted partly because
he is a Presbyterian.

He is now a refuge. I can see no dishonor in it. They are a bunch of Irish cut throats, and you know what their religion is (Ealy, 1878b).

In his memoirs Ealy recalled that "Our little children kept their dolls on the floor under the window. They said 'So [sic] our dolls will not be shot'" (Ealy, n.d.).

According to Francisco Trujillo, who rode with the McSween faction, McSween offered a $500 bounty to each person who killed a member of the Dolan group (Crawford, 1937). In early March three murders were perpetrated by his "Regulators" (Fulton, 1968, p. 147).

Trujillo recalled that several of McSween's men, including William Bonney—alias Billy the Kid (Figure 16)—met McSween at Chisum's ranch, where he asked them to meet him in Lincoln the following Monday. He said, "As soon as I arrive, Brady is going to try and arrest me and you should not let him get away with it. If I am arrested I shall surely be hung and I don't want to die, while if you kill Brady you shall earn a reward" (Trujillo, 1937).

On the 1st of April, Brady met with four deputies and proceeded toward McSween's home to serve a warrant and an additional attachment on his property at the Tunstall Store. When they reached the Tunstall Store they were ambushed and the sheriff and a deputy were murdered. William Bonney was wounded while he was apparently attempting to remove the warrant from Brady's body (Ealy, 1955, p. 66; Lavash, 1982).

During the April court session the grand jury exonerated McSween of the embezzlement charge and indicted William Bonney, John Middleton, Fred Waite, and Henry Brown for the murder of Brady and his deputy; Jesse Evans, George Davis, Miguel Seguro, and John Long were indicted for the killing of Tunstall—with Dolan and Billy Matthews listed as accessories (Fulton,
McSween moved back into his house on the 26th of April, and the missionaries moved into Tunstall's former living quarters (Ealy, 1955, pp. 55-56).

In that same month of April, J. J. Dolan & Co. became insolvent and closed their operation. In June they mortgaged their building, its contents, and their livestock holdings to a Santa Fe bank (Lincoln County Records, Contract Book B, pp. 139-142).

In June, A. A. McSween signed a mortgage for $500 to Isaac Ellis for a piano manufactured by the Daniel F. Beatty Co. of Wollington, New Jersey. The mortgage stated that the piano was "now in the home of McSween... together with all and singular appurtenances thereunto belonging" and that McSween was to keep it insured "against loss by fire and other damage" (Lincoln County Records, Contract Book B, pp. 143-147). Such an expensive musical instrument was the talk of the Territory. A Mesilla newspaper commented.

Col. John Kinney [leader of a gang of rustlers from the Mesilla Valley (Fulton, 1968, pp. 129, 204; Gibson, 1965, p. 126; Keleher, 1957, pp. 139-140)] holds Lincoln. And Lincoln I regret to say presents an appearance as melancholy as an old top when about to join the Sons of Temperance. While the dozen or so graveyards scattered around town adds a little cheerfulness to the place neverless the late arrival of a $900.00 piano for Mrs. McSween and a $500.00 set of Scotch bagpipes for honest Mac; Lincoln is terribly dull - extremely so (The Mesilla News, June 29, 1878).

During the night of Sunday, July 14, some 40 to 50 McSween men began congregating in Lincoln. They took positions in the McSween home, Ellis store, Tunstall store, Montaño store,
and Juan Patron's house (Long, 1879, p. 590).

Inside the McSween place were Susan and Alexander McSween, the Shields, and, as far as is known, Jim French, William Bonney, Ignacio Gonzalez, Jose Chavez, Tom Cullins alias Joe Bowers, Joseph J. Smith, Eugenio Salazar, Tom O'Folliard, Francisco Zamora, Vicente Romero, and Harvey Morris—a young man recently arrived from Kansas, suffering from a lung ailment, who was reading law in McSween's office (Fulton, 1968, p. 249).

The house was fortified by placing adobes in the windows, sand bags behind doors, and rifle ports were drilled through walls (Dudley, 1878b; Nash, 1879, p. 675).

On Monday the 15th, the sheriff's party fired some shots at the McSween home that caused "portions of the roof to fly" (Dudley, 1879a).

After four days of relatively ineffective exchanges of gunfire, the military arrived with cavalry and infantry detachments under the command of Col. N.A.M. Dudley. Dudley entered Lincoln armed with a Gatling gun and mountain howitzer. He intimidated the large force in Montaño's store with his howitzer, and the inmates fled to the hills north of town where they were joined by the men in the Ellis store (Fulton, 1968, pp. 260-261). This was a serious blow for McSween—he lost nearly two-thirds of his forces.

While a directive from the War Department stated that Dudley could not take an active part in the civil affair, his troop's presence instilled courage in the sheriff's men.

The occupants of the McSween place had been surrounded for days. On the 19th, at about 1:00 p.m., Deputy John Long and Octaviano Salas (Salas, 1977), who was a deafmute called "Dummy," attempted to set fire to the Shield's kitchen. The fire went out and they were fired upon from the Tunstall store. They escaped by jumping into the outhouse. They were joined by Buck Powell, who was shot.
at as he was coming up from the river, and all three men remained there until after dark. Long stated that the privy was located outside the fence and dug into the bank. It was open, facing the Rio Bonito. The deputy recalled that, "I could stand up in it. Standing up most of the time. It was not a good place to sit down" (Long, 1879, pp. 594, 608).

The sheriff's men managed to set fire to McSween's kitchen at the northwest corner of the building about 2:00 p.m. The fire burned slowly south along the west wing, across the south side of the house, and up the east wing (Boyle, 1879, pp. 627-628).

The Shield family and Susan McSween were evacuated from the burning building.

After being forced by the fire to the northeast room, some of McSween's men made a dash out the kitchen door toward the gate in the east fence. Tom O'Folliard, William Bonney, Jim French, and possibly others escaped past Tunstall's corral and to the creek (Fulton, 1968, pp. 270-278; Boyle, 1879, p. 630).

Then, according to Deputy Andrew Boyle:

When the first party escaped McSween and two Mexicans got as far as the back house, and Harvey Morris fell as he was going out the gate. McSween and the two men was [sic] back in the corner and stayed about 5 minutes. They then tried to run out the gate a second time, we shot at them and they again ran back, stayed there about 10 minutes more. McSween called out I shall surrender. Robert Beckwith replied I have got a warrant for you. He went then at the back door to Serve the Warrant. McSween replied I shall never surrender, then the fire became Promiscuous and that was the time the Big Killing was made.

When McSween said he would not surrender everyone of them
commenced to shoot. Robert Beckwith fell first with McSween on top of him and two Mexicans right beside them. Two more Mexicans went into the Chicken House, and two fell between the door and the back of the house (Boyle, 1879, pp. 641-642).

McSween, Romero, Gonzales, and Zamora were all killed. Eugenio Salazar was wounded and left for dead (Fulton, 1968, pp. 270-278; Figure 17). During the blaze a keg of gunpowder exploded in the building. Susan McSween stated that it contained five or six pounds of explosive (McSween, 1879, p. 231).

A newspaper reported that: ...McSween had a keg of powder under the parlour floor, touched it off with a fuse expecting to blow up some of the sheriffs posse that he thought were in the room, but none were near the room except a man named Sanders who was blowed heels over head, picked up for dead, but after some work he came to not seriously hurt (The Mesilla News, August 8, 1878).

Rumors persisted that there were other men who were killed and left in the house. On the day after the conflagration, Col. Dudley wrote his superiors that, "Thomas Culliens, and another man unknown, were wounded or killed, and reported as buried in the burned house of McSween since the commencement of firing between the two belligerent parties, it is reported that one man was killed on the 18th inst. and buried in the cellar of McSween's house" (Dudley, 1878a). A couple of days later, the Colonel went on to say, "No attempt has been made to remove the debris in the McSween house, for the purpose of searching for the remains of the three or four bodies of persons reported as being wounded, and unable to escape from the burning building" (Dudley, 1878b). The Colonel went on to praise the sheriff's men:
SITE OF THE McSWEEN HOME

Opening a law office in Lincoln, 1875, Alex A. McSween rebuilt an old adobe into a ten room house. On July 19, 1878, Peppins posse fired it to oust inmates. Leading victim of this strategy was McSween himself. The burned-down house was never rebuilt.

OLD LINCOLN COUNTY HISTORICAL COMMITTEE 1952

Figure 19. Site plan showing Fresquez House (center), with Watson House to left and Tunstall Store to right.

Figure 20. The Fresquez House and McSween Site looking west in the 1950s. Photo Archives: Museum of New Mexico.
Men who have the reckless courage to attack a building in bright mid-day, its walls forming a perfect protection, against any modern musketry to its inmates, pierced as this Castle of McSween's was, with scores of loopholes for rifles on every side and angle, to say nothing of the flat roof, protected by a perfect wall of defense, and for hours hugging the walls, exposed to the fire of not only from the loop holes, but from the roof and adjacent buildings held by McSween's men... (Dudley, 1878b).

A couple of days later, while Susan McSween and her two employees were going through the wreckage of her home hoping to recover something of value, John Kinney and another man came by. While she was picking up what remained of what had been a valuable carpet, McKinney's companion reportedly threatened, "What's the good of trying to save that? By God, we've killed McSween and we'll get you next" (Fulton, 1968, p. 281).

A Mesilla newspaper commented that on the day after the house burned, "Mrs. McSween left early in the morning, after taking what things she wanted, except that $1000 piano" (The Mesilla News, August 8, 1878).

In January of 1879, the burnt house and property were appraised at $200 (Crawford, 1938). In September of that year the Shields sold their interest in the property to Ira E. Leonard. The deed description indicates that there may have been some wall remnants at that time (Lincoln County Records, Deed Book B, pp. 36-37). Susan McSween lost her interest in the property through a court decree. That portion of the lot was purchased at public auction by Charles Fritz in 1882 (Lincoln County Records, Deed Book D, pp. 150-152). Meanwhile, Leonard sold the former Shield portion of the lot to J. J. Dolan (Lincoln County Records, Deed Book F, pp. 403-405). By the spring of 1886, Dolan had acquired the rest of the site (Lincoln County Records, Deed Book G, pp. 570-573).

Dolan sold the former McSween-Shield property to M. L. Gorton, a butcher, in September of 1888 (Lincoln County Records, Deed Book K, pp. 132; County Commissioners Journal of Proceedings, Book A, pp. 464).

In March of 1892, Gorton and his wife, Orella, sold their parcel to William Rosenthal & Co. who were then doing business in the former Tunstall store (Lincoln County Records, Deed Book O, pp. 231). The deed did not mention any improvements to the property. However, when Rosenthal sold the parcel to Cecilia West in 1894, it was mentioned that the lot had on it a shingle roofed house, known as the old Gorton Butcher shop (Lincoln County Records, Deed Book M, pp. 374; Deed Book A-2, pp. 132-134). From 1895 to about 1910, the house was rented to various attorneys who used it as their law offices (Lincoln County Records, Deed Book M, pp. 564; Deed Book T, pp. 263).

West died in 1908, and her husband sold the parcel and house to Henry Lutz two years later (Lincoln County Records, Deed Book A-2, pp. 135).

The following year Lutz sold it to Teofilo Sisneros and Isidro Fresquez. The deed contained a clause giving occupancy of two rooms to Attorney George Barber (Lincoln County Records, Deed Book A-5, pp. 372).

In February of 1912, Sisneros sold his share in the property to Fresquez (Lincoln County Records, Deed Book A-5, p. 373). By that time two rooms of frame construction had been added to the west side of the former Gorton Butcher shop. The entire building was finished with batten and board (Velio, 1979; Salas, 1979; Figure 18).

In 1935 a letter was found in the basement of the Tunstall store that was addressed to McSween from "Billy" that stated, "Behind the door..."
near the basement are a thousand dollars—get them." Fresquez searched the basement of the Tunstall store, which was not added until 1886, and a search was made of his house (Carter, 1983, p. 384; Fresquez, 1937). According to his daughter, Clara, her father "looked behind all the doors and also looked for a basement, even though he had lived in the house for thirty years without knowing of any door leading down. He lifted up part of the floor and discovered there was a basement. He kept looking until he found melted silver pieces and a few shards of pottery" (Fresquez, 1937).

The frame addition to the Fresquez House was removed in the 1940s, leaving the building in the same form as it is today.

Fresquez sold the site to James C. Joyce in 1961 (Lincoln County Records, Deed Book 46, p. 466), who deeded it to the State of New Mexico in that same year (Lincoln County Records, Deed Book 47, p. 535). The McSween House site is now part of Lincoln State Monument.

There has been little in the way of investigation of the site, and there are no surface indications of the house. It is suspected that the house remains are under the Fresquez House and that they may extend west towards the Watson House (Figures 19, 20, 21).

In 1983 a septic tank was located 39 feet north of the northwest corner of the Fresquez House. There were no cultural remains in that area (Taylor, 1983). The following year a trench was excavated along, and six feet west of, the west wall of the Tunstall Store for the installation of a French drain. While there were no architectural features that could be associated with the McSween home, the cultural material extended four feet below the surface (Taylor, 1984).

There has been no investigation of the cellar beneath the Fresquez House, for it would necessitate the removal of flooring that was installed in the 1960s.

In conclusion, the archaeological
excavation of this site could be expected to reveal the remains of the material culture and architecture of the structure that was a focal point in the Lincoln County War.

Santa Fe, N.M.

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Velio, Ephifania Fresquez
1979 Interview by Rafaelita Pryor. Lincoln County Historical Society Collections, Lincoln, N.M.
Benjamin Kite Wetherill homesteaded a farmsite on the east side of the Maneos River below the small settlement of Maneos, Colorado, in 1879. By 1881 his wife, Marion, and their six children, Richard, Alfred, Anna, John, Clayton, and Winslow, had moved there from their former home in Atchinson, Kansas, to help develop the homestead. It became known as the Alamo (Cottonwood) Ranch for the slips of cottonwood trees that were set out and grew profusely along the irrigation ditch that ran in front of the house.

The ranch prospered as the entire family pitched in to clear cobblestones and sagebrush from farmlands near the river, dig ditches to bring water to those fields, cut logs and fashion them into a cabin, and to care for the farm stock, chickens, and garden. In a few years the irrigated lands yielded bumper crops of alfalfa, timothy hay, wheat, oats, and potatoes. Additional fields were cleared, outbuildings were erected, the house was enlarged, and fruit trees and berries were planted. But the farms about Maneos were so productive that farming was not lucrative.

To supplement their income, the Wetherills turned to the cattle business. Their stock was grazed on the limited range of the Alamo Ranch at the foot of the Mesa Verde, as well as along the Maneos Canyon and in the canyons and on the top of the Mesa Verde. The latter was Ute territory, but, by maintaining friendly rapport with the Indians, the Wetherills were allowed to run their cattle there.

Richard, Alfred, and John Wetherill were particularly curious about the abundant antiquities of the region from the first time their land clearing and plowing turned up broken pieces of pottery and stone implements of an unknown people. When ranch chores permitted, they searched out and dug into the numerous rubble covered mounds that dotted their ranch, the remainder of the Maneos Valley, and down into Maneos Canyon and its tributaries. From them they recovered pottery vessels, stone projectile points, knives, axes, and metates and manos, leading them to surmise that a large number of people had lived there some time in the past.

The identity of those responsible for the deserted villages or the time of their occupation were not known; however, in their initial activities the brothers had no reason to question the popular notion that the ruins had been left by the Aztecs. The origin myth of those Indians had them originating "to the north" and, after a series of migrations, settling in the Valley of Mexico, where the Spaniards found them early in the 16th century. Through erroneous interpretation of that legend, the pioneer settlers of the Southwest not only considered their local antiquities as those "northern" outposts of the Aztecs but named some of them Aztec Ruins, Montezuma Castle, and Toltec Ruin. It was not long until scholars corrected that misbelief by demonstrating that the archaeological remains of the Southwest actually were the ancient habitations of the ancestors of the Pueblo Indians. The Wetherills learned this by reading some of the early Annual Reports of the Bureau of Ethnology and the reports of the U.S. Geological and Geographical Survey of the Territories, otherwise known as the Hayden Survey after its director Dr. F. V. Hayden, whose parties traversed Maneos Canyon in 1874 and 1875.

As awareness of the regional ruins grew, so too did a market for their artifacts. A number of the local residents began devoting their spare time to gathering up salable specimens. Spurred on by this interest, the Wetherill brothers, frequently aided by their brother-in-law, Charlie Mason, turned to exploring for and collecting artifacts from the Mesa Verde cliff
dwellings, where discarded articles were especially plentiful and well preserved. Led by Richard, by the summer of 1890 they had found and examined 182 of the major cliff dwellings of the Mesa Verde and had accumulated and sold two large collections from them. They claimed they had "furnished historical societies and individuals with enough material to keep scientists busy for years trying to place the people who lived, fought and died there, or simply drifted away" (Wetherill, 1977, p. 127). They also had named most of the ruins and geographical features of the Mesa Verde and had roughly mapped the areas explored.

The first Wetherill collection, which was obtained through combining efforts with miners Charles McLoyd, Howard Graham, and Levi Patrick, came primarily from Cliff Palace, Spruce Tree House, and Square Tower House. Grubstaked by the Wetherills, the miners did the digging and the brothers packed the recovered items out to the Alamo Ranch. Al Wetherill later wrote that no written or photographic records of the diggings were kept but that no standing walls in the dwellings were destroyed by the miners. Common excavation practice of the time was to use picks and shovels to throw dirt and fallen walls of one room into another that had been previously cleared (Wetherill, 1977, pp. 124-125). The collection, a fine cross-section of Mesa Verde material remains, was purchased by the Denver Historical Society and placed on display in the Colorado State Museum in Denver.

Soon thereafter the Wetherills were again back at the same ruins and several additional more remote ones, enthusiastically gathering a second assortment. It duplicated and expanded upon the previous collection. No field notes were kept (Wetherill, 1977, p. 127).

In his autobiography, Al Wetherill states that after assembling the second collection at their ranch, the items were catalogued before being packed for shipment to Durango, Colorado. There the assemblage was placed on display. The public showed so little interest in paying to see the materials and "being educated," that the collection was shipped on to Pueblo and then Denver. In both places public reaction to the cliff dweller remains continued lukewarm. Just when the venture was about to be declared a financial failure, the art director for the Minneapolis Industrial Exposition, H. Jay Smith, reviewed the exhibit and decided to purchase it for display at the Minneapolis fair. Subsequently in 1893, the collection was shown at the World Columbian Exposition in Chicago and later was acquired for the University of Pennsylvania (Wetherill, 1977, p. 126).

In gathering their first two collections, the Wetherills followed the same practices employed by contemporary "relic hunters" or "ordinary pot-hunters" (Wetherill, 1977, p. 119). It was the only way they knew how to recover the material. However, during those times they developed a proprietary interest in and assumed a personal responsibility for the Mesa Verde ruins (Wetherill, 1977, p. 130). They grew critical of indiscriminate pot-hunting and of those who damaged or demolished archaeological remains in their search for marketable objects. The ruins became "symbols of the past" to the Wetherills (Wetherill, 1977, p. 104), and they strove to excavate them as completely as possible in order to keep their contents from falling into the hands of "the regular army of pothunters and so be scattered to the four winds" (Ibid., p. 125). Also, they adopted the premise that if the artifacts were properly cared for, they would provide information about the people who had made and left them in their now ruined communities.

Although the brothers sold many artifacts piecemeal to individuals, and in fact for a time advertised their offerings in the local newspaper, they
came to recognize the value of keeping collections intact in order to place them in the hands of museums where they would be studied by scientists. Richard is said to have written both the Smithsonian Institution and Harvard's Peabody Museum requesting that they either sponsor him and his brothers or send their own scientist to work with them. Neither institution responded affirmatively to the request, though the Smithsonian did say it would be glad to accept the donation of a collection from Mesa Verde (McNitt, 1966, pp. 35-36).

The summer of 1891 marked the turning point in the archaeological activities of the Wetherill family. Then, for the first time, they worked with a scientist from whom they learned excavation procedures, recording techniques, and archaeological objectives commensurate with the professional code which prevailed at the time. Their mentor was Gustaf Nordenskiöld, a young Swedish scientist who came to Mancos to spend a week touring some of the Mesa Verde ruins with the Wetherills. However, during the tour he became so enthralled with the cliff dwellings that he remained there for four months of what proved to be the first intensive scientific research in the Mesa Verde.

Gustaf Nordenskiöld was the son of a distinguished Swedish family. His father, Baron Nils Adolf Erik Nordenskiöld, was an internationally recognized geologist, best known for his research in arctic regions. He saw to it that his son was trained in the physical and natural sciences and was raised in a scientific environment. Young Nordenskiöld's Bachelor of Arts degree from the University of Uppsala, with concentration in geology, botany, and chemistry, was completed in only three years of studies. Immediately thereafter, following in his famous father's footsteps, he accompanied an expedition to Spitsbergen. The hardships of arctic exploration proved too rigorous for Gustaf's somewhat frail physique, and he came down with tuberculosis before the expedition ended. He was sent to a clinic in Germany for treatment and, as his condition improved, he resolved to travel around the world to further regain his health in warmer climes.

After journeying for three months in southern Europe, Gustaf sailed from France for the United States, arriving in New York in the spring of 1891. Traveling across the country, he routinely sought out places of interest, including universities, libraries, and museums, and conferred with local scientists. Upon visiting the Colorado State Museum, he viewed the collection of archaeological materials which had been obtained from the Wetherills two years earlier. He also conversed with Alice Eastwood, a botanist whom the Wetherills had guided to some of the Mesa Verde cliff dwellings. The artifacts and the talk with Eastwood so aroused his interest that he decided to change his travel plans in order to detour into southwestern Colorado to personally visit the Mesa Verde. Thus, armed with a letter of introduction to the Wetherills, he went by train to Durango and then on the second of July by buggy to the Alamo Ranch in Mancos.

The Wetherills had become accustomed to guiding an ever increasing number of visitors to the nearby ruins and readily agreed to show some of them to Nordenskiöld. For a week they rode horseback and hiked over the Mesa Verde, examining numerous cliff ruins, and doing some limited digging in several of them. Although not an archaeologist, Gustaf quickly recognized the scientific importance of the remains and, learning that no one had seriously studied them, decided to stay there long enough to do so. He also proposed to acquire a representative collection of specimens for shipment to Sweden. Engaging the Wetherill brothers as guides, outfitters, laborers, and confidants, he conducted explorations and excavations until the end of October.
Extensive digging was undertaken in Mug House, Long House, Kodak House, Step House, and Spring House, all on the mesa that Nordenskiöld was to name Wetherill Mesa after his respected colleagues. Other major cliff dwellings, such as Cliff Palace, Balcony House, Square Tower House, Spruce Tree House, and Painted Kiva House, were investigated and described, and a few artifacts were collected from some of them (Arrhenius, n.d.; Nordenskiöld, 1893).

When the Swedish scholar initiated his archaeological studies, he was as well, if not better, qualified for the undertaking as were other individuals then engaged in similar pursuits. Archaeology as a discipline was in its infancy, and most of those working in it had been trained in other fields. Geology, Gustaf's area of specialization, especially provided both the technical and theoretical structures within which a descriptive archaeological program of the 1890s could be adequately accomplished.

Utilizing procedures from his previous investigations, Nordenskiöld established a scientific order to his work that provided him the information necessary for the preparation of his monumental work upon Mesa Verde. He photographed and drew maps of each ruin, numbered the rooms, located all specimens found within a given room and noted their association with other objects. All artifacts were carefully catalogued, described, measured, photographed or sketched, and typed according to function or the material from which they were manufactured. Excavations were accomplished a layer at a time in order to record the stratigraphic relationships of features and objects encountered. Every bit of physical evidence that his diggers brought to light was collected, including such things as human excrement, wood ash from firepits, sweepings from room floors, and unworked twigs and bark. Analytical studies included such diverse practices as chemical and microscopic examination of thin sections of potsherds and relating the cultural attainments of the cliff dwellers to the environmental setting in which they took place (Nordenskiöld, 1893).

Periodically during the excavations, specimens that had accumulated were boxed, taken to the railroad station in Durango, and dispatched to Sweden by way of the Swedish consul in New York. Local residents, bent upon protecting the Mesa Verde cliff dwellings and preserving their contents, tried to stop Nordenskiöld from shipping his collection abroad. To their dismay, a court hearing determined that there was no law to prohibit it.

Upon completion of his Mesa Verde research, Nordenskiöld and Al Wetherill made a two month journey by horseback to the Grand Canyon and back. Then early in 1892 he bade farewell to the Wetherill family and returned to Stockholm. Two years later he published his comprehensive report, The Cliff Dwellers of the Mesa Verde, Southwestern Colorado: Their Pottery and Implements. Originally printed in Swedish in 1893, it was translated into English almost immediately. Gustaf maintained a lively correspondence with the Wetherills and was especially interested in the finds Richard had made in southeastern Utah after his departure. He asked for, and received, a series of skulls from them that Richard demonstrated had belonged to people older than the cliff dwellers. These were turned over to Professor Gustaf Retzius, noted anatomist and anthropologist of the Medical College of Stockholm, who verified their anthropometric characteristics (Arrehenius, n.d.).

During the six months of close association and shared experiences, there evolved not only a real sense of mutual admiration and respect between this educated son of a nobleman and the unsophisticated self-appointed custodians of the cliff dwellings of the Mesa Verde, but in the process the Wetherills learned the fundamentals of archaeology. Gustaf did not inten-
tionally pose as an instructor to his cow­
yboy associates, but the example of his 
painstaking field methods was not 
lost upon them. They adopted his tech­
iques and, demonstrating a sincerity of 
purpose, realized that the proper 
understanding of the cliff dwellers 
could only be obtained by following 
Nordenskiöld's investigative methods. 
Al Wetherill signified his approval of 
proper excavation procedures when he 
referred to the collection they helped 
Nordenskiöld assemble as, "the first 
scientific and only famous one made 
by us at the Mesa Verde. We were 
all proud of that collection" (Wetherill, 
1977, p. 127). Richard Wetherill did 
likewise in a letter to Gustaf after 
he returned to Sweden stating, "As it 
now stands you have the only 
collection that has been taken out 
properly" (Letter, Richard Wetherill to 
Gustaf Nordenskiöld, April 127, 1892). 

Al Wetherill wrote, "We had 
started out as just ordinary pothunters, 
but as work progressed...we developed 
quite a bit of scientific knowledge by 
careful work and comparisons" (Weth­
erill, 1977, p. 119). Although Norden­ 
skiöld is not credited for his change 
in technique, it was undoubtedly he 
who first taught them, by example, to 
work precisely and "scientifically." 
Richard Wetherill specifically ac­
nowledged the fact that the Wether­ 
ils had learned photographic methods 
from Gustaf when he wrote of his 1894 
field work in southeastern Utah, "I 
have charge of the outfit. Al and 
John do the photographic work and 
are doing well at it thanks to you" 
(Letter, Richard Wetherill to Gustaf 
Nordenskiöld, February 6, 1894). 

A contemporary of the Wether­ 
ils, McNeil Camp, related in a news­ 
paper interview quoted in Al Wether­ 
ill's autobiography that after news 
spread of the Wetherills' discovery of 
the major Mesa Verde cliff dwellings, 
newspaper reporters and archae­ 
ologists flocked to see them. "The 
archaeologists and explorers...lodged 
at the Alamo, the roomy, ever-
hospitable hacienda of the Wetherill 
clan ...the correspondents and scholars 
were pretty dependent upon the Weth­
erills. In turn, the Wetherill boys, 
keen of intellect as they were, re­ 
cieved as it were a free university 
education in archaeology and kindred 
subjects, from the world's best minds 
of the time. Already the best guides 
in the Southwest, they quickly became 
the Southwest's best-informed amateur 
archaeologists, as well" (Wetherill, 
was largely responsible. 

The Wetherills put their Norden­
skiöld training to use first in gath­
ering a comprehensive collection to be 
included in the state of Colorado's 
hibit at the Chicago Worlds Fair. 
Their education in archaeological mat­
ters was even more evident in the 
winter of 1893-1894 when Richard 
Wetherill led a collecting expedition 
to the canyons of southeastern Utah 
under the joint sponsorship of Talbot 
and Frederick Hyde, sons of a well­
to-do New York family, and the Ameri­
can Museum of Natural History. To 
document the finds, Richard designed 
a work sheet for standardizing record 
keeping based upon the Nordenskiöld 
example. It contained spaces for en­
tries under the following headings: 1, 
number of house or ruin; 2, number of 
article; 3, name of article; 4, number 
of room; 5, number of section; 6, 
deepth; 7, number of floors, if any; 8, 
remarks. In a letter to Talbot Hyde 
prior to the field work, he outlined 
the field methods he would use. 
"Every article to be numbered with 
India ink and fine pen or with tube 
paints white, red, or black. Plan of 
all houses and sections to be made on 
paper or book to be ruled both ways. 
Drawing of article to be made on paper 
with numbers and name. Photograph 
each house before touched, then each 
room or section and every important 
article found. I think you will find 
this will meet all the requirements of 
the most scientific but if you have 
any suggestions whatever I will act 
upon them. This whole subject...is in
its infancy and the work we do must stand the most rigid inspection, and we do not want to do it in such a manner that anyone in the future can pick flaws in it" (McNitt, 1966, p. 62).

By digging stratigraphically and using his years of experience in field work and a finely tuned sense of observation, Richard was able to distinguish two distinct occupations in many of the Utah caves. The upper one, associated with cliff houses, was similar to that with which he was familiar on the Mesa Verde. At lower levels, beneath the cliff dweller structures, were deposits and burials left by earlier occupants of the caves, who did not possess pottery but instead used a profusion of basketry. Furthermore, their burials demonstrated that they had elongated skulls, while the cliff dwellers uniformly had round-shaped skulls. The earlier people and their distinctive culture were named Basket Maker. The name is still used to distinguish the earlier stages of Anasazi culture; the cliff dwellers came to be associated with the more recent, or Pueblo, periods of the Anasazi continuum.

Of the Wetherill brothers, it was Richard who devoted most of the rest of his life to archaeological activities. He continued his association with the Hydes to work in the archaeologically rich Chaco Canyon of northwestern New Mexico. From 1896 through 1900, the Hyde Exploring Expedition concentrated its excavations there on the classic site of Pueblo Bonito. Richard had hoped to be director of this project, but the American Museum named George Pepper, a brilliant young Harvard student who had never been to the Southwest, as the expedition's field director. However, the 38 year old, work-hardened Wetherill assumed his second-in-command duties and actually organized and outfitted the expedition, ran the camp, bossed the Navajo laborers, and planned and executed much of the extensive field work. His past training and experiences admirably qualified him for his many contributions to the success of the endeavor. At its close, Richard remained in Chaco, running a ranch and operating a trading post. He was murdered there by a Navajo Indian on June 22, 1910.

Evaluations and criticisms of the Wetherills' archaeological efforts, particularly those related to the Mesa Verde, have been voiced and written from the 1880s until the present. For three generations the brothers, their brother-in-law, and their descendants have conducted a rebuttal of remarks which they have considered derogatory, unjustified, or untrue. A long feud has festered between the Wetherills and the U.S. Department of the Interior and the National Park Service. The Wetherill clan has claimed that the interpretive literature and programs of Mesa Verde National Park frequently are not correct in reporting on the family's activities and in assessing its accomplishments. The Park Service has countered that its interpretations are based upon competent historical research (Wetherill, 1977, pp. 279-309). An almost totally ignored facet of the Wetherill story is the role that Gustaf Nordenskiöld played in changing the brothers from pothunters to members of a small cadre of field workers who were doing archaeology by scientifically accepted procedures.

An objective appraisal of the two sides of the Wetherill controversy might be:

1. They were "vandals." Not true. Their early digging was a far cry from modern excavation procedures, but their activities in the cliff dwellings are stated not to have knowingly damaged any standing walls. Moreover, the Wetherills were critical of those who did harm the ruins. They assumed a protective attitude toward the ruins of the Mesa Verde.

2. They were "pothunters." True, for their pre-Nordenskiöld experiences until 1891. As did many of their friends and neighbors, they dug up and sold individual specimens and entire
collections. No records were kept inasmuch as the purpose of digging was to quickly recover as many artifacts as possible. There were no laws or mores to prevent such digging.

3. They excavated "carefully and kept detailed notes." True, for their post-Nordenskiöld excavations. Their four months' association with Nordenskiöld taught them certain scientific techniques and an understanding of how their use contributed insight into past peoples and cultures. After that, the Wetherills’ excavations were similar to those used by individuals recognized as archaeologists at the turn of the century.

Prescott, Arizona

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Figure 1. Al Hayes with Gretchen Chapin (on right; later Gretchen Hayes) and Carolyn Osborne at the 1940 UNM field school.
MEMORIES AND LESSONS FROM AL HAYES:
THE CHACOAN CHAPTER

JOHN D. SCHELBERG, PETER J. MCKENNA, AND THOMAS C. WINDES

With the retirement of Al Hayes in 1975 from the National Park Service's Chaco Center, a chapter in the history and archaeology of Chaco Canyon had come to a finish. Al began and ended his archaeological career at Chaco, withdrawing from the scene, as many would contend, just as a new era in archaeology was dawning. Possibly one of his greatest legacies to his friends and successors was his unfailing wry wit and common sense in the face of all manner of trendy enthusiasms, innovations, and frustrations which arose during the course of his archaeological pursuits. Al provided many of us with a reasonable bridge between the past and the future; his pragmatic approaches to survey and excavation still serve as models of efficient field methods. Rather than a recitation of facts, or a polemical diatribe with Al as the historical focus, we thought it more appropriate to offer some reflections about Al Hayes during his tenure during the early phases of survey and excavation with the Chaco Project, as a tribute to some of what he brought to his work and how he flavored our appreciation of archaeology.

By the time Al confronted the last group of known-it-alls and greenhorns he was to ramrod through the Chaco Project, he had a vast store of experience and stories from his life in the Southwest. After graduating from the University of New Mexico, he spent the summer driving a Fred Harvey tourist bus from Santa Fe to points throughout northern New Mexico. During this time he practiced the fine art of "telling lies to tourists," a skill he employed as recently as 1983 when, as the archaeologist, he spent a good portion of the summer on Southwestern raft excursions again "...telling lies to tourists." One can but wonder to what extent this talent was extended also to summer field crews. The early survey-related stories which unfolded ranged from near snake bites in Mexico to transforming the low evening cook fire into a blazing "whiteman's fire" for partying during crisp autumnal nights in western Canada. By baking the ground and then parking the Model T in such a way that the blanketed engine was over the hot spot, they slept peacefully knowing that it would start in the morning. Another favorite story involved an extended rafting trip down the Yukon when several months passed without seeing hide-nor-hair of another person.

The Chaco field school years of Model T Fords, hogan dorms, and virtual isolation may have been a little less rigorous than Wetherill's earlier pack horses and chuck wagons, but certainly must have made the trailer accommodations and eight-cylinder transport of the early 1970s seem like the garish Ritz. During Al's camp-boy days in Chaco Canyon for the University of New Mexico field school, he and Robert Lister traveled to the railhead at Thoreau once a week to pick up supplies. The Model T pickup was always loaded beyond capacity, and one afternoon a frame member snapped. While they contemplated the situation a Navajo family approached in a horse-drawn wagon. After a brief, mutually unintelligible conversation, one of the sons unhitched a horse and rode off. He returned with fresh cut juniper splints and the truck was emptied and repositioned so that several people could push from below while others lashed the juniper poles across the break. Soon the truck was reloaded and both parties went their respective ways. The juniper-frame repair lasted for the remainder of the summer and the truck for several more years.

One of the teaser stories Al occasionally started, but never finished, involved his famous "Love Rock"—a huge, sheltering boulder about 250 m. west of Casa Rinconada
and a little past the old field school hogans—where couples would meet to "drink a little wine and read some poetry. Especially in the rain." Other greatly anticipated social events were the squaw dances where all present decked out in as much Navajo silver and finery as they had available. Al met his future wife Gretchen during one of the later field schools (Figure 1). While Gretchen also studied archaeology, her interest centered around Navajo folklore and ethnohistory of the Chaco area. Among her projects, one was the documentation of the Legend of the Gambler. He reputedly lured people to the Anasazi town of Pueblo Alto to gamble with their fortunes (Chapin, 1940). Years later, Al would be involved with the planning which prepared the Chaco Project for four seasons of field work at this ruin. Other Navajo folklore stories concerned Threatening Rock behind Pueblo Bonito. Gretchen interviewed one knowledgeable Navajo woman who, initially, refused to expand upon the generally known tale, but after repeated inquiries she opened up and provided additional material. Threatening Rock fell later that year (1941). Gretchen never believed in a causal relationship between the inquiries and the collapse, but she did wonder what the Navajo woman thought. Gretchen made a point of visiting the canyon each summer and was a delight to talk with. At the close of each field season the Hayes hosted a party at their Corrales home featuring an evening of spicy Mexican food and conversation. Gretchen's occasionally pointed comments clearly indicated that her quiet nature should not be misunderstood.

There are far too many of the earlier stories to relate here, but their telling and savoring lent to the flavor of work with Al. We all knew that he had headed the survey (Hayes, 1964) and teamed with his mentor Al Lancaster for work on the Badger House Community (Hayes and Lancaster, 1975). The early Chaco Project crews inherited this blend of experiences. On the high mesas of Chaco, Al would occasionally break a gazing reverie across the heat-shimmered, juniper-dotted vistas with the rhetorical quip, "My God! Can you imagine? We get to do this and they pay us for it?" A chestnut, but one could appreciate the differing experiences that went into that statement.

There were some tasks, however, which caused Al to drain the bitter cup. He disliked bureaucracies and bureaucrats almost as much as he enjoyed field work. Among his least favorite people were Administrative Officers—especially when he had to cope with the first round of field supplies acquired by the system. In order to "save money," the least-cost packs, trowels, and plastic tape measures all came from government catalogs—"the backyard gardener's variety," Al noted as another single-stitched canvas pack disintegrated. Halfway through the summer and in no uncertain terms, Al arranged for "real" equipment and sent one of the crew in to assist. Unfortunately, history has not recorded what he said after discovering that a request for shovels netted a carton of surplus army entrenching tools covered with 14 layers of paint; it is known that he took the stairs two at a time.

In the winter, Al would read the survey forms and excavation notes and offer suggestions and points for clarification. He definitely preferred grammatically correct statements which were clear, to the point, and generally devoid of "million-dollar-words." This penchant resulted in a "Memo to Budding Authors" which featured ripe examples for illustration. Lists of personal grammatical transgressions were entered as evidence in discussions with the accused. One surveyor (Schelberg) returned from
school for Christmas and stopped by the office to pay respects. "Have a seat," Al gestured with a cigar to the empty chair. "You know, you're one of the worst spellers I have ever had to put up with. After all that book learning it seems to me that more words would have stayed with you." He paused for a moment while shuffling through his crowded desk. "Just wait until I find the list...." Schelberg has never misspelled storage since.

Despite the impression that may have been created above, Al's role in the office certainly was not that of the gruff curmudgeon (Figure 2). He was actively concerned with report preparation for his major projects, analyses of the survey collections, keeping tabs on the progress of his crew chief's reports, and in the writing of timely topical reports which might otherwise have gone begging. His goal of five printable pages a day resulted in reports (in the order they were written) on the survey of Chaco Canyon (Hayes, 1981), a predictive study on the significance of Chaco's "shrines" as a possible communication system (Hayes and Windes, 1974), a descriptive report on his excavations at Shabik'eshchee (Hayes, 1975), and a report on a cache of exquisitely preserved Anasazi gardening tools found during the Chaco survey (Hayes, 1976). When others, less adept at the writing game, despaired of matching the five-page-pace, he was forced to admit it had only taken 20 years to reach that pace. When Al left he had an enviable record of completed reports for all projects undertaken.

Compared to the field, working in any government office is more often than not a dull and tedious matter, and the Chaco Center was no exception. Al instituted Friday luncheon outings where everyone in the office gathered, usually at "The Club" on Broadway and Lomas, to feast on matchless carne adovada burritos and cold beer. All the office tension of the week would dissipate in a pleasant glow of camaraderie as Hayes would...
salt his beer, drag out the omnipresent stogie, and join the varied conversations.

The field, however, was his real element. The field work conducted by the Chaco Project began in earnest in May of 1972. Robert Lister was the project director and Al was the field director, a position he retained through the summer of 1974. He then retired from the field and spent the next months, as mentioned, preparing reports—a decision guided by conscience rather than preference. Al thoroughly enjoyed the field and remained in the canyon for each of the three field sessions he directed. Field work provided a partial escape from the bureaucratic maze of paperwork and proprieties which he regarded with contempt.

The first week of survey consisted of orientation to the area, the forms to be used, the variety of sites, and finally the completion of the usual battery of forms required for government jobs. At one point he held up a handful of forms and dryly noted that they all had to be filled out in event of an accident. "A word to the wise," he jokingly cautioned, "if any of you do anything serious enough to necessitate their completion, you're fired." Luckily, no one ever got hurt during his years in charge.

As it turned out, forms were the least of the worries. The first challenge quickly materialized in a chronic antagonism of the Park staff toward the surveyors, and Al had the decidedly unpleasant task of running interference between the crews and the upper-level management of the Monument. That archaeologists were unwelcome to Park personnel was made painfully clear when the first subject of the Chief Ranger's "Welcoming" speech was an itemized list of taboos, foremost of which was that the campground and maintenance areas were completely off-limits to all archaeologists. The second point was that in event of a water shortage, Park-staff lawns took precedence over

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Figure 3. Al Hayes with his 1974 field crews and other researchers. Bottom row: Peter McKenna, Kellie Masterson, Marcia Truell, Richard Loose, Edmund Kelley and Al Hayes. Middle row: Tom Windes, John Schelberg, Vernon Morgan, James Kee, John Wero, Gene Begay, Bruce Yazzie, Ken Augustine, and Ben Norberto. Top row: Junior Lopez, Jimmy Lopez, Chee Beyale, Randy Morrison, and David Brugge.
workers' showers. Since most of what we did was an offense of some sort, Al's powers of diplomacy were tested that summer. Fortunately, the Park staff migrated and the summer of 1973 saw a more relaxed and interested group at the Monument.

The work of the first summer was primarily an inventory survey while subsequent seasons consisted of a mixture of survey and excavation. Although approximately 450 sites were previously recorded (Pierson, 1949) a systematic survey had never been undertaken and some of the existing documentation was deemed inadequate. By the end of the third summer of survey, over 2000 sites had been recorded (Hayes, 1981).

Al's approach to the project consisted of quiet but firm diplomacy mixed with a good deal of interest and humor. He managed to spend a portion of each week out with the surveyors and, later, the excavation crews (Figure 3). He always supported his crew chiefs and never publicly interfered with their field decisions; usually suggestions and criticisms were privately relayed. While his routine varied, it was not uncommon for his silhouette to appear on the horizon sometime during the morning, seeking a crew to join in their search for sites (Figure 4). Often Al would find a site before finding a crew and, if the site were particularly interesting, would plant cryptic little messages on yellow paper and await the results. If he got no response in a few days, inquiry would be made and occasionally a humbled crew would return to the field to search for the noted site. At the time, these missives did not seem to be as poetic as those of the Wetherills and, unfortunately, none have survived. His penchant for notes extended to weekly comments in the Monument's visitors log. They varied from a routine series of "You've seen one ruin you've seen 'em all," "Don't pave the roads," and "This is a Park isn't it? I thought Parks were supposed to have trees," to comments alluding to recent events, such as "Next time why not burn all of the weeds?" The latter followed an attempt by some of the maintenance workers to dispose of a large pile of tumbleweeds near Wijiji the easy way. Naturally, the fire roared out of control searching a swath around Wijiji.

Al's field deportment was always above reproach and he toiled with vigor and excellence in the discipline as he had been trained. In the field he was always neatly dressed for the occasion. The crews tended to be under-dressed and noontime shade was a paramount consideration, but with Al present, lunch occurred wherever the group happened to be. When the boss was already digging in, and the shade was farther away than the break was long, sunburn sacrifices were made. "Shorts!" he once snorted, the contempt barely contained. "Shorts! Well hell. When I was a kid we had to wear shorts and we couldn't wait until we were old enough to wear long pants. I suppose you tore up a perfectly good pair in order to make those." Of course, Scheiberg, Windes, and Neller, all of the same crew, continued to wear shorts in the field despite the spiny plants and Al's equally pointed remarks.

When the director of one of the several documentaries filmed during the course of the project suggested that he would be more comfortable (and presumably look more authentic) if he removed his hat, pulled out his shirt tail, and put on gloves, Al suggested that perhaps this was an opportunity to educate the public, since he had been doing archaeology looking more or less as he did at the moment for longer than the director had been alive. Indiana Jones would have scored no points with Al Hayes. To quiet the grumbling protestations and appease cinemagraphic sensibilities, he finally pushed back his hat so that "your face can be seen."

During the first summer a certain tension existed between several of the younger proponents of "New
which was recorded as having a clay lining in an attempt to obtain samples for archaeomagnetic dating. Unfortunately, the structures had been inadequately backfilled, and any suitable burned clay had long since weathered away. Rock hard alluvium from Frank H. H. Roberts' 1927 backdirt piles (Roberts, 1929) made this an arduous short-term project. Undaunted by his crews' labors, Al also directed that another pit house be excavated at Shabik'eshchee (Hayes, 1975) to obtain not only the archaeomagnetism samples but to secure wood for dendrochronological dating. As Shabik'eshchee's dating was considerably later than other Basketmaker sites, it presented a problem of interpretation and Al sought to confirm the dates through additional samples, not knowing that the same summer the tree-ring lab had just revised Bannister's dates for the site (Robinson et al., 1974). Al also thought it worthwhile to reexcavate several of the large ovens associated with the Pueblo Bonito Foundation Complex in the hope that a clue to their function might be found. Specifically, Al felt the ovens may have served as crematories (a possible reason for Chaco's infrequent burials), and he was searching for fragments of charred human bone embedded in the mortar or in gaps between the masonry. Unfortunately, Neil Judd's excavators were thorough and neither the original contents remained nor were any interstitial human fragments found.

Al made several memorable public forays while at Chaco, three of which stand out more vividly than most. His summary of the field work presented at the 1974 Pecos Conference at Mesa Verde prompted a long convoluted question concerning a possible differential distribution of jack rabbits and cottontails. After a reflective pause his, "Well hell, they're all just bunnies to me," brought down the house. After the laughter subsided he responded more fully to the question. Giving "site"-seeing tours and Archaeology" and the generally traditional archaeological orientation of the project organizers. Al was decidedly in the latter category but it became clear that he had read and considered much of the literature concerning New Archaeology. Rather than dismissing it out-of-hand, as many of his contemporaries had done, he discussed the merits and trivialities of the subject. While remaining generally unconvinced, he appreciated the differing point of view and was willing to listen and defend his own. Perhaps the single most important point he tried to get across was to be sure to consider any situation, problem, or site from the perspective of common sense. Much of what Al held to be common sense has been implanted in the archaeological realm of "mid-range theory" (Ebert, Hitchcock, and Taylor, 1974; Binford, 1976, pp. 7-8), and "behavioral archaeology" (Schiffer, 1976) although his views were never so formalized. Over the years his contention has frequently rung true that blindly clinging to overly complex or simplistic models may be misleading, and that a judicious dose of common sense should be swallowed before embarking on any project. Common sense, then, was his guiding principle whether it was in answering a question about prehistoric lifeways or an excavator's question about superimposed walls and floors.

Al's willingness to wrangle with and try out innovations extended to new technical toys, albeit with the usual wit. Many of these experiments involved the nascent field of archaeologically applied remote sensing, the progress of which Al kept filed under the label of E.S.P. Cameras were suspended from Whittlesey bipods and drifted clicking over sites in hydrogen-filled balloons. Several days were spent with proton magnetometers and portable radar units probing for burn anomalies and unseen mysteries beneath the surface. Partway through the summer of 1973 Al sent the survey crew to Shabik'eshchee Village to reexcavate every pit house hearth
answering tourist-type questions occupied more time than Al preferred, but, by comparison with other impositions (especially during the summer of 1972), such tasks were deemed not "so bad," if not without peril. A visit from the President of the University of New Mexico preoccupied Al so much with seeking memorable areas to show off that he failed to see a rattlesnake lurking in the brush. He swears that it struck at him and it was only after his feet stopped moving that he was able to regain his breath and realize that his sunrise jogging had additional benefits. Finally, at an evening talk in Chaco, a California couple asked a long question concerning the Anasazi's oneness with each other, nature, the earth, the cosmos, and several other groovy abstractions. His answer that most of the Anasazi were just simple dirt farmers trying to make ends meet revealed again a down-to-earth perception of past life and behavior.

It is believed that Al was inadvertently responsible for naming one of the smaller Chacoan towns, The Rabbit Ruin, through misdirection (Windes, 1977, pp. 71-73). When Jim Judge joined the Chaco Center staff in 1974 as Al's successor as the field director, Al oversaw the transition by guiding Jim through the canyon, showing him the ruins, informing him on the Project's progress and research aims, sites to be dug in order to gather information to address those aims, and the quirks and personalities of the field crews. Al must have appeared a bottomless well of information and knowledge about Chaco. Jim must have been mildly surprised as, each time they passed a large house mound just north of Pueblo Alto and he asked its name, Al would always point in the opposite direction and say "Did you see that rabbit?" And thus, "The Rabbit Ruin," as the site came to be known, was named.

Final recollections involve the field social events. The first summer closed with a traditional cabrito or goat roast. Al and several assistants
methodically dispatched and prepared the goats while the city boys took up an intense interest in astronomy. At the close of his field work in 1974 another large cabrito roast was held, attended by some 70 friends, associates, and hangers-on. It was actually a surprise, not an easy feat given the confines of Chaco. We later found out that during the preparation for goat dispatching, Al was fretting that the city boys were about to render a quantity of meat unfit for consumption, "cause they don't know what they are doing." Which was true. Once everything was ready, Al was asked to take over and he did—much to everyone's relief. Ironically, it never occurred to him why the event was taking place. At the end of all the hoopla and boasting, Al was presented with a "Silver Marshalltown" trowel. Dave Barde prepared the engraved, traditional Navajo coin-silver trowel, and Pete McKenna carved the handle. The celebration rolled merrily into the wee hours.

This festschrift to a partially retired Al Hayes has the distinct advantage of letting him know that others have taken note of his participation in and contributions to the world of archaeology. A happy disadvantage is that he remains in a position to disagree with that which is attributed to him. As with all recollections, the specifics have blurred and some may quibble with expansive points, but our image of Al is firm. An archaeologist and man of integrity, humor, and intelligence, he set us an example of energy, curiosity, productivity, humanity, and common sense. As Al remembers Al Lancaster, so we remember Al Hayes.

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