Proceedings of the ANASAZI SYMPOSIUM 1981

Compiled and Edited by Jack E. Smith

Mesa Verde National Park, Colorado
Mesa Verde Museum Association, Inc.
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A lot of people worked very hard to make the Symposium a success. I will not try to name them all, but a few who played critical roles should be singled out. Certainly, Adrienne Anderson and Bob Heyder, whose original idea this was, deserve special credit. Gil Wenger, Chief Park Archaeologist and Chief of the Interpretive Division, did a wonderful job in helping to organize the Symposium, and he worked very hard behind the scenes making certain that our equipment was provided and working properly—no simple task as we moved about and played tag with the rains. John Ogier, head of the Mesa Verde Company, made certain that everyone got fed, and Mel Livingston, also of the Mesa Verde Company, always had the coffee and cookies there when we needed them despite our straying from the time schedule. The design work in the announcements, programs and other printed materials was the work of Joyce A. Attebery, Park Library Technician. Joyce also did a variety of chores during the Symposium from selling tickets to the dinner to directing traffic in the mud. Seasonal Ranger Gail Waggoner sat through the entire Symposium operating the tape recorders, and never fell asleep once. Our session chairpersons: Joe Ben Wheat, Elizabeth Morris, Susan Collins, George Gumerman, Adrienne Anderson, and Jim Judge, were outstanding; they kept the sessions rolling, the participants within reasonable time constraints, and frequently added their own contributions to their respective sessions. Without these people there would have been no Symposium. Anna Sofaer brought her slides and films of the solstice marking site in Chaco Canyon and provided a special stimulating evening for us all, and a special thank you must go to her. Finally, a most important thank you must go to everyone who came, to those who presented papers and to those who listened, commented and questioned. The turnout was magnificent, especially considering the distances travelled and the myriad of commitments, academic and otherwise, which everyone had in addition to this. I can speak for all of us at Mesa Verde National Park in extending sincerest thanks to everyone who did such a great job in making this a most memorable event, well worth the effort it took to put it together.

In the following pages is the Symposium pretty much as it happened. Some of the papers were revised by their authors after their presentation (and I suspect a few of them were written afterwards), but here they are essentially as they were presented. The papers are organized by sessions in the order in which they were presented, followed in each case by the comments, questions and answers which followed. There are some variations in format from paper to paper just as there are in writing style. We have not attempted to standardize the papers. The reader will thus find "pithouses," "pit houses" and "pit-houses" and "great kivas," "Great Kivas" and other variations. We felt it important that each participant have his way with his paper and that we maintain as much as we could of the individuality of each. If this bothers some readers we are sorry, but the choice was ours and this was how we chose to do it.

The Symposium elicited a lot of ideas on a lot of subjects. Some were new and some were not so new; there were agreements, and disagreements, a lot of serious discussion, and even a little humor. All in all, it really was a "convivial meeting." We hope this feeling comes through to the reader.
Although the 1981 Anasazi Symposium was held in Mesa Verde National Park, we tried not to focus it on the Anasazi of the Mesa Verde but rather on the Anasazi in general. We likewise wished to dedicate the Symposium to a person who significantly contributed not simply to the knowledge of the Mesa Verde Anasazi, but to knowledge of the Anasazi in general. With these ideas in mind, it seemed appropriate to single out a pioneer of Southwestern archeology in the Anasazi area, Jesse Logan Nusbaum.

Admittedly, Mr. Nusbaum’s many productive years as Superintendent of Mesa Verde National Park may have biased our choice a little. However, his contributions as first director of the Laboratory of Anthropology in Santa Fe and his pioneering work in what was once called “salvage archeology” (and now has evolved into what we sometimes refer to as “contract archeology” or “mitigation archeology”) have indirectly and directly fostered much of the research opportunities of present day archeologists in the Anasazi area. Thus, we felt that to dedicate this symposium to Jesse Nusbaum was an appropriate gesture of recognition. Mr. Nusbaum materially aided the cause of archeology as part of the American public’s concern for the preservation of an important part of our heritage—not only of the Anasazi but of all the prehistoric people who lived within what are now our national boundaries.
Although there had been a Hohokam Conference in Tempe, Arizona in 1973 and a Mogollon Conference in Las Cruces, New Mexico in 1980, it had been a long time since there had been a formalized gathering of archaeologists specializing in Anasazi studies. Given the tripartite division of Hohokam, Mogollon and Anasazi as the traditions of the prehistoric southwestern United States, it was time for some such meeting on the Anasazi.

Robert C. Heyder, Superintendent of Mesa Verde National Park, presented me with the idea of hosting an Anasazi Symposium in the spring of 1980. I don’t remember who decided that it should be a “symposium” rather than a “conference,” but it was an excellent choice of words, for the objective was to be, as Webster defined it, “a convivial meeting” in which ideas could be exchanged in an informal atmosphere.

We all agreed that holding such a symposium was a good idea and that Mesa Verde National Park would be a good place to hold it. Usually there is not a more pleasant place in the fall. The weather then is typically mild and sunny, the insects have disappeared, and the park is settled down to the quiet which follows the summer visitor season. The outdoor amphitheater on Chapin Mesa, overlooking the wide expanse of Navajo Canyon, offered a pleasant outdoor setting which would be conducive to the informal atmosphere which we wished to create. So we selected as dates for the Symposium the first three days in October, after the summer rains and before the snows of late fall. Altogether an ideal time and place—or so we thought!

I don’t know how many years it must have been since it last rained on the Mesa Verde during the first three days of October, but on this particular occasion rain it did. The Symposium began on schedule on October 1, after intermittent showers the night before. Throughout the day dark clouds rolled across the sky and we seemed always on the verge of a deluge, but we did make it through the first day in the outdoor amphitheater, cold but dry. That was important, I think, for in that setting the proper atmosphere for the Symposium was established and even though the deluge did hit during the night, we were able to maintain the atmosphere established on that first day through the next two days when we were crowded into our old ramshackle Civilian Conservation Corps recreation hall (built in 1934), the only building large enough to provide us with shelter from the storm.

Probably everyone who attended the Symposium will remember first the rain and the mud. But I believe they will also remember what a good time it was, how much sharing there was of a diversity of ideas about the Anasazi, and what a lot of stimulation we all came away with. Afterwards, as I read the papers again and spent many hours listening to and transcribing the comments, questions, and discussions which followed the papers and which we recorded on tape, I once again caught the feeling of what a really nice time it was. In this compilation of the Proceedings we have tried to catch as much of that feeling as we could. Thus, the discussions were transcribed pretty much verbatim and the editing was held to an absolute minimum. At times a word or a phrase was not clear on the tape; sometimes someone coughed at just the wrong moment or the speaker mumbled just a little too much, so there are a few guesses and a few omissions, but what is printed here is pretty much what was said and how it was said. I apologize if I have misquoted anyone or distorted their statements where I had to do a bit of guessing.

In organizing the Symposium we borrowed ideas from both the Hohokam and the Mogollon Conferences. We particularly wished to keep everyone together and offered only single sessions—a lesson learned at the Mogollon Conference in Las Cruces where Pat Beckett really showed us how a conference ought to be done. For the format of these Proceedings we borrowed some ideas from the beautifully designed volume of the Hohokam Proceedings. The rest was pretty much our own. The themes of the six sessions were hammered out by Adrienne Anderson, Bob Heyder, Gil Wenger, and myself. We kept the themes broad and general to provide the widest possible range of topics while still keeping them within manageable bounds. We were fortunate in having a response to our call for papers which closely fitted our allotted time, and we had to reject very few. Even more fortunate was the fact that nearly everyone showed up or sent his/her prepared paper for someone else to read. Only one person backed out and only one failed to provide us with his paper for publication. For that many archaeologists, it is a pretty good record. I will admit, however, that we did do a bit of badgering and extending of deadlines to get all the papers that are included herein.

Jack E. Smith,
Chief, Research and Cultural Resource Management
Mesa Verde National Park
WELCOME

On behalf of Mesa Verde National Park...

It is indeed with great pleasure on behalf of the park staff and myself that I welcome you today to Mesa Verde National Park and the Anasazi Symposium. Without the assistance of the Mesa Verde Museum Association this symposium would still be in the idea stage, so we are deeply grateful for their support in putting this most important event together.

This symposium was conceived as part of the seventy-fifth anniversary year celebration of Mesa Verde National Park. We felt that this park, as an institution of national and international significance would be the most fitting place to hold such a gathering to reassess what we mean by “Anasazi.”

We look at research as one of the primary and paramount missions of this institution, for through a research program we can provide improved maintenance and resource protection, and most of all an up-to-date interpretive program. This symposium will provide that type of insight and data that will be of enormous value to both the science of southwest archaeology and to this park.

I do hope that you have an excellent symposium and that the ideas developed and exchanged here will be of value to your science. I have no doubt that many new avenues for future research possibilities will be opened up during the next three days. The day we think we have learned it all will be a sad one, so I am quite sure, in my mind, that you will develop more new questions than all the old ones you will have solved.

If I may be of assistance to you during the next three days, please let me know.

Robert C. Heyder, Superintendent,
Mesa Verde National Park, Colorado
WELCOME

On behalf of Rocky Mountain Region and the National Park Service. . .

It is truly a pleasure for me to represent the Rocky Mountain Regional Director, Lorraine Mintzmyer, and to welcome you here to Mesa Verde National Park for this symposium on the Anasazi.

I know I speak for Bob Heyder and his entire staff when I say we are honored by your presence.

I recognize at least a few of the folks in this audience, and I recognize still more by name or by reputation. I know that this audience embodies much of the body of knowledge in the world today about the subject of this symposium.

Given that expertise, and given your intimate knowledge of Mesa Verde, it would be pretentious for me to offer any but the most passing comments about either subject.

While overall cultural coordination responsibilities within the Region fall in my Directorate, it has been said—and they will remain nameless, that I know enough about the Anasazi to be considered dangerous. However, let me join you, you who have made the Anasazi a lifelong academic pursuit, in a complete and utter fascination and appreciation of this lore.

Allow me further to offer a few general comments that reflect some personal views that I hope may be appropriate to the occasion.

We speak of the National Parks as being many things. They are national treasures, places of recreation and inspiration, showcases of our nation’s history, living laboratories, and fragile islands where the natural order of things continually goes on.

But in the face of recitations such as that, it occurs to me that we think too seldom and too modestly about the role of the parks as great repositories of scientific challenge and discovery.

At any given time, for example, there are literally hundreds of individual research projects in progress in various areas of the National Park System. Most of them are conducted by individuals or groups representing colleges and universities, but certainly other organizations and scientific institutions are involved as well.

And the subjects. . .well, as you very well know, they embrace an incredibly wide range of interests in the cultural and natural sciences.

You distinguished members of this audience know also so very well that the National Park Service contributes only in very modest fashion to these research endeavors.

We have the archeological centers at Chaco Canyon, at Tucson and at Lincoln, cooperative studies units at the Universities of Arizona, Hawaii, Washington, Idaho, Oregon and several others. We have a water resources lab at Colorado State University and an Air Quality Office in Denver. Field stations are located at Great Smokies and Everglades, and research centers at Glacier, Grand Teton, and here at Mesa Verde.

But practically speaking, the opportunities and the potential for greatly expanded research in the parks simply overwhelm the efforts that we are able to invest in this activity, and for reasons with which you are familiar—and these never seem to change, most notably, funding and personnel ceilings.

We are enormously pleased, and I hope that you are also, with the work that has been accomplished at Mesa Verde in the development of the research center here. Bob Heyder and members of his staff have done a splendid job on a virtual shoestring, and I appreciate this opportunity to commend them for their work before their friends and professional peers here in this audience. They are presently involved in reorganizing the collections and in reassessing their cultural resource management program, with an expanded role for research.

Another step that is intended here is to establish a research library to properly protect and to make available to researchers the myriad of materials that have been, and continue to be accumulated through the park’s vigorous cultural and natural resource management and research program.

But as much as anything else, all of us in the National Park Service want to create and maintain as hospitable a climate as possible for your own research endeavors and those of others like yourselves.

We value our association with you. We value your work. And we value the produce of that work. For, like the physical resources of the park themselves, that which emerges from your research represents an enduring legacy for all generations to come.

Again, it is an honor for me to join Superintendent Heyder in welcoming you to Mesa Verde National Park.

I know that your symposium will be most productive. With that, let me wish you good luck and thank you all.

Richard A. Strait
Associate Regional Director
Planning and Resource Preservation
National Park Service
Rocky Mountain Regional Office
Denver, Colorado
INTRODUCTION OF KEYNOTE SPEAKER

It is my honor to introduce our keynote speaker this morning. Before I do that, however, I want to read you something.

In the little brochure we sent out last spring it says, "Mesa Verde National Park, Colorado is a delightful and very colorful place to visit in the fall with its clear weather and many hours of sunshine." When we wrote that I didn't expect to be standing here today wearing a winter parka!

I want now to introduce Dr. Joe Ben Wheat, whom most of you know already either through personal contact or by reputation. Joe Ben has been in the field of Anasazi archeology for a good long time, and he certainly has as much of a grasp of the overall picture of the Anasazi as anyone I know. It is, incidentally, his own fault that he is the keynote speaker this morning. A year ago, when we first started planning this symposium, I went out to Joe Ben's camp at Yellowjacket, and we sat and talked about the idea of the symposium. I was fishing for ideas about general topics and so forth, and he began to let me have some of his pet theories. Right then the wheels began turning in my mind and I said, "Well, Joe, I think maybe you would be a good one to start it off." So, he sort of put his foot in his mouth that time. I am very glad that he did, however, because I can't think of anyone who would be more appropriate to launch this general review of the phenomenon we call "Anasazi" than Dr. Joe Ben Wheat.

Jack E. Smith
Chief, Division of Research & Cultural Resource Management, Mesa Verde National Park
For a number of years, now, I have been interested in the development of certain ideas about Southwestern archeology, notably the Chaco Center ideas, and to begin, I would like to go back some distance in time and sketch some of the significant things that happened in the development of the Anasazi concept, which is our concern today. To do that, I think we must go back to A.V. Kidder, who, even though he wasn’t the first to do Southwestern archeology, was, in a very real sense, one of the most productive archeologists, both in terms of his own personal work and in terms of the work he engendered. When Kidder first became interested in archeology, in the early 1900’s, he was told to select some area other than the Southwest because the Southwest was, in fact, a squeezed lemon; there was nothing left to learn there; we already knew all about the Southwest. This followed the work of such people as Nordenskiold, Prudden, Fewkes, Pepper, the Wetherills, Cummings, Hewett, and other pioneers who had their own ideas about the development of the prehistoric Southwest and what it was.

So, it was with much help from Kidder, who thoroughly ignored the squeezed-lemon advice offered him, that we began to develop what has come to be the field of Southwestern archeology and the concept of the Anasazi.

Terminology in the early 1900’s was quite individualistic. Each archeologist who worked there had his own ideas of what was to be found, his own ideas of sequence, and his own ideas on terminology to represent each of these—the pithouse period, the small-pueblo period, and so on. And so, in part, to reconcile these different ideas and terminologies, Kidder convened the first Pecos Conference in 1926. It was the direct result of that conference and the one that followed in 1927, that we began to get some agreement on a comprehensive terminology for the Southwest. It is interesting that it was a non-Southwesternist who offered us our Pecos chronology or sequence—the sequence of Basketmaker I, II, III, and Pueblo I, II, III, IV, and V. That individual was T.T. Waterman. Kroeber and Waterman were visitors to the first Pecos Conference, and Kroeber once told me that Waterman, bothered by all of the confusion, took a long walk one day and came back the next morning to offer what we now think of as the Kidder, or the Pecos, chronology. I was very curious when he told me that for I had never heard it from anyone else. And so, I asked Kidder about it one day, and he said yes, that was exactly what had happened. So, for the record, we ought to give credit for our first comprehensive terminology—to T.T. Waterman.

From that conference began the kind of synthesis that led ultimately to what we now call the concept of the Anasazi and its branches. Kidder’s 1924 book was our first synthesis of Southwestern archeology, and it is still a very useful book. He first reviewed the various areas of the Southwest and the various cultures or culture complexes that occupied them. He thought that the cultures that centered around the drainages of the San Juan River, the Chaco, Kayenta, and Mesa Verde, were all related—they were all Pueblo—but while they all had certain kinds of things in common, they also had a great number of differences. It seems to me that through the years, we have come to emphasize more and more of the differences and perhaps less and less of the com-
monalities of these three subculture areas that Kidder described.

At any rate, this group ultimately became known as the “Anasazi,” which is the Athapascan name for “the ancient ones.” The ancient inhabitants of the Gila and Salt rivers had been called the “Hohokam,” which was Pima for “the ancient ones,” and so it was thought that Anasazi was a good counterpart name for the San Juan peoples.

Today we have what we generally think of as three branches of the Anasazi. We need to know a lot more about those than we know at the present time. All of us have been concerned with one area or another of the San Juan. We have people working in the Kayenta area, we have people who are working in the Mesa Verde area, we have people who are working in the Chaco area. The intensity and extent to which this work has been carried on has varied considerably over the years, and also the work has varied depending on the kinds of sites that have been excavated. The distinctiveness of the three branches is, I think, a matter of some importance, and I think it is one that we are here, at least in part, today to address. How distinctive are they? When did they split up, if they did? In other words, were they all basically the same originally and then developed their own thing through time? Or is there some commonality still existing among them? How distinctive is Kayenta? How distinctive are Chaco and Mesa Verde? Are we dealing with what we can think of as the prehistoric Eastern Pueblo vs. prehistoric Western Pueblo?

There are a number of things that bear on this problem, and one of those is, of course, our old and trusted friend, the pottery. If you look at the pottery from all three areas—Kayenta, Chaco, and Mesa Verde—from Basketmaker III through Pueblo I and on into very early Pueblo II, the potteries are essentially the same. There are local clays, local temper, local paints, but the designs are the same, the way it’s made is the same, the shapes are the same. And so, certainly up through that period of time, the pottery seems to be very much alike. Sometimes in early Pueblo II there began certain changes, and the course taken by the potters in the Kayenta area seems to have varied increasingly from those in the Chaco and the Mesa Verde. When one looks at the Pueblo II pottery of the Chaco Canyon and the Pueblo II pottery in the Mesa Verde area, it is essentially the same. The various kinds of Pueblo II pottery that we lump under one type, Mancos, in the Mesa Verde seem to be very much the same pottery that we lump under several type-names for the Hosta Butte and Bonito phases in the Chaco Canyon. Perhaps that means that we are dealing with more closely related groups in these two areas, at least ceramically, than we are in the Kayenta area where the pottery does differ significantly during that time.

By Pueblo III times, the pottery in the Kayenta area is altogether different. The series of polychromes and the black-on-whites are certainly very distinctive in terms of their decoration as well as their paints and paste and so on. However, when you look at what happened in the Mesa Verde and in the Chaco Canyon, you find a succession of the very similar Pueblo II pottery types—Mancos, Gallup, Escavada, and so on, followed, essentially, by McElmo and Mesa Verde black-on-white in that order, throughout both the Mesa Verde and the Chaco. And so, again, I think that we need to look at those continuities to see whether we are dealing, in fact, with developments in place or whether we are dealing with migrations of people. After the Chaco had been abandoned by its original inhabitants, did the Mesa Verde people give up and go down to the Chaco and reoccupy all those houses?

Essentially, what I’m asking, I think, is whether we are dealing, in fact, with time periods, in that particular aspect, rather than with culturally distinctive areas? How different are Chaco and Mesa Verde in terms of the pottery? It seems clear that during Pueblo II, the Chaco style of pottery prevails throughout both Chaco and Mesa Verde, just as it is certain that the Mesa Verde style of pottery prevails during Pueblo III in the Chaco area, as it does in the Mesa Verde area. With this in mind, then, I would like to suggest that we are dealing not so much with two culture areas, in that respect, as we are dealing with time periods—a Pueblo II that is essentially Chacoan and a Pueblo III that is essentially Mesa Verdean, notwithstanding certain developments that are in addition to those particular things.

Now, there are still many elements that we don’t know. Architecture is one of the major problems, I think, in dealing with this. Kidder pointed out, long ago, that the architecture, at least in terms of construction, was quite different in the three areas of the Anasazi—that of the Kayenta was said to be very sloppy, the houses were not very well constructed, while that of the Mesa Verde was sort of intermediate in terms of the way the houses were designed and put together, while, finally, the apex of Southwestern architecture was in the Chaco. Well, this is certainly true. There is no doubt that such places as Pueblo Bonito and other Bonito Phase houses are outstanding examples of architecture. What we do need to know, however, is to what extent this is a function of the materials available to the builders and how much of it is a function of developments in a cultural sense. In other words, to what extent does geography influence the nature of the building? In overall terms, is the fine core veneer masonry of Chaco Canyon a superior development to that of the finely finished Pueblo III pecked wall stone masonry of the Mesa Verde? These are questions that I think we need to address. We need to know a lot more about the geographic variations of architecture and their causes. One of the things that strikes me about the Southwest is that we know a great deal more about certain types of buildings in the Chaco than we do in the Mesa Verde area. We know more about certain types of buildings in Kayenta and Mesa Verde than we do in the Chaco. For one thing, I think it can be argued that both the
Mesa Verde National Park per se and the national monuments in the Kayenta area are, in one sense, aberrant. That is, the cliff dwelling phase. Remember that at one time the cliff dwellings were considered to be a separate time period altogether. Were the cliff dwellers really Pueblos? Well, that has been more or less answered now, but we still have to look at these things, I think, as part of the geographical possibility concept. Certainly you do not build cliff dwellings where there are no big caves to house them. And so, to the extent that we have big caves, we have architecture that is adapted to them, and that is true both in the Mesa Verde and in the Kayenta. We do not have big caves in Chaco, therefore we have no big cliff dwellings. There are some questions, then, as to how much we should rely on these various architectural features as cultural indicators.

One of the things that is clear about the Chaco is the difference between the Hosta Butte, that is, the ordinary Pueblo II, and the Bonito Phase. It seems clear that we have certain major structures in the Bonito Phase in the Chaco where essentially the same pottery and the same other ordinary things were used as in the Hosta Butte Phase. We do not yet know whether or not this is also true in the Mesa Verde area. The reason we don't know is that no one, yet, has ever tackled a major site in the Mesa Verde outside of the somewhat aberrant cliff dwellings. Because of this lack, we don't know what the big houses in the Mesa Verde are like.

A great point is made with regard to the Chaco outliers theory. This has to do with the planned construction, large size, and the large number of rooms in Bonito Phase houses. It is true that there are some very large buildings with numerous rooms, incorporating kivas in the room blocks. Well, that’s also true in the Mesa Verde area. At Yellow Jacket, for example, there are three major houses of three or more stories on the main ruin, which will have something close to three to four hundred rooms, with the kivas incorporated into the room blocks. Those are fairly sizeable houses. They fall, certainly, within the average of the Bonito Phase houses in the Chaco. So far, we don’t know whether they were planned, and we don’t know whether they were built all at one time. The reason we don’t know is that nobody has ever had the money or the inclination or the time to tackle such a house. And until we do, we won’t know. In any case, it is difficult to conceive of such a house merely as an aggregation of small “unit” houses built a few rooms at a time as needed. One of the interesting things about the Chaco is that because of the dryness of its climate when the houses collapsed, they didn’t collapse all that far. Thus, it is easier to see some of the major architectural features at Chaco than it is at Mesa Verde where the houses tended to collapse into more amorphous piles of stone.

There are some other architectural features that are supposed to be distinctive of the Chaco. One is the so-called Chaco type of regular kiva with the subfloor vent. Just how prevalent are they in the Chaco? We know that many of the Pueblo II houses have regular four- to six-pilaster kivas with vent shafts that go out under the southern recess—and some of them do have southern recesses. Not all of the kivas in the Mesa Verde have southern recesses, either. We do know that in certain places in the Mesa Verde area we have some so-called Chaco-type kivas. Are they Chaco out-liers or are they part of a common tradition that developed over the entire area? I submit that we do not yet know. There are Chaco-type kivas, for example, down in the Mancos Canyon in sites that show no other sign of Chaco influence. And so, I think we have to look at these, not comparing them, necessarily, back to Chaco, but comparing them to the nextdoor neighbors on the next ridge over, in the Mesa Verde.

Another one of the major features of Chaco, noted by Kidder, was a number of Great Kivas. How many Great Kivas were there? In 1960, Gordon Vivian could say that Chaco Canyon, with nine Great Kivas, was the center of density of the Great Kiva in Pueblo III times. He noted only one, rather aberrant, Great Kiva for the Mesa Verde proper, and then there came the Chacoan out-liers of Aztec, Lowry, and one or two others. There was one at Goodman Point and one at Jemez Springs.

Let’s go back a little bit. Prudden, in 1903, made a survey, mostly through the northern part of the Mesa Verde area, and he noted a great many large, circular structures, some of which showed masonry walls. He didn’t know what they were, and said so. Fewkes came along a few years later and noted a number of the same features, which he termed reservoirs. So, we had a considerable number of reservoirs scattered over the northern part of the Southwest, the Mesa Verde area. Fewkes recorded that one of these had four large pilasters out in the middle of the depression—a rather interesting feature for a reservoir. However, it was not until the Bureau of Land Management surveys were made that we began to realize that there were, in fact, a number of Great Kivas in the Mesa Verde area. That survey turned up 26 Great Kivas on BLM land alone. When you add to that the Great Kiva at Yellow Jacket, another on Mesa Verde, and several others scattered around the area on private land, it becomes quite clear that the Great Kiva is not centered in Chaco Canyon. If you were to go simply by the numbers, then you must consider that their center is in the Mesa Verde area.

So, perhaps we should think of the Great Kivas in the Chaco Canyon as Mesa Verde out-liers. I am being a little facetious here, you may notice, but in reality, what I’m pleading for is not a down-playing of the Chaco Center theory as much as for careful consideration of the implications of some of these commonalities rather than pulling them more or less out of context.

So, in terms of Great Kivas, and tri-walled struc-
tures that occur with some of the planned houses, it is clear that there are more of them in the Mesa Verde than in the Chaco. Perhaps these belong to that indigenous population common to both areas. One of the things that I think we must recognize is that there is something of a dichotomy between the Hosta Butte Phase and the Bonito Phase in the Chaco Canyon. There is the common, everyday, garden variety of Pueblo, where most people lived; and there are some extremely large buildings, very fancy buildings, that had to do with something beyond ordinary, everyday commerce, and these are, in the Chaco, called the Bonito Phase. Do we have anything comparable to them in the Mesa Verde? One of the things that Pruidden did point out in his survey was that there was a number of structures, such as Goodman Point, Bug Point, and others, that were as large as 900 feet along the base. We've never dug one of these things. We don't know what they are. Do these major structures correspond to the Bonito Phase houses in the Chaco Canyon? Are we, in fact, dealing with a dichotomy?

Now, we are accustomed to think of these, in the Mesa Verde, at least, as beginning in early Pueblo III, a little bit later than they are supposed to have begun in Chaco, if the Chaco dates are correct. But, we don't know the true nature of these very large Mesa Verde houses because no one has ever dug one. So far as I am aware, no one has ever even tested one. We know, in the Yellow Jacket area, that on the Yellow Jacket ruin, itself, there is a Great Kiva. There are also three large houses at least three stories high. In one place, in a pot-hunter's hole, it was possible, several years ago, to see clear down to two stories, well above the base, so there may even be more. And, as I pointed out, the kivas in these three houses are embedded in the room blocks. These do not suggest random agglutinations of rooms. Many smaller houses are arranged in rows along the crest of the site. Most of these appear to be planned structures. In the majority there are two rows of rooms at the back, many of them two stories high, with kivas in a plaza enclosed by a row of rooms or a wall across the front. Several of the houses have attached towers. These would appear to be planned structures, but they seem to be, more or less, the standard type of Pueblo II house for that particular site. Are they, in fact, the kind of planned structure that we conceive when we're talking about the Bonito Phase houses?

At Yellow Jacket, the Great Kiva seems to be the center—the ceremonial or commercial or whatever—center of the series of villages that lie around there. There are some fifteen or twenty of these that have anywhere from ten to thirty rooms in each one of them. Furthermore, this association of a Great Kiva center with satellite villages seems to be the case not only at Yellow Jacket but in most of the areas where Great Kivas have been found. Each seems to be the focal point for a group of villages. The same, I would say, is true of the Chaco development, and whether they stand apart or not is something that we need to know.

At any rate, there is the possibility that we have the same kind of dichotomy in the Mesa Verde area that we perceive in the Chaco. And it may be for the same reasons, that is, that those impulses that came up out of Mexico, whether they came via the pochteca or whether they came via gradual creep from one village to another, or whether they came through Hohokam and Mogollon farther south, reached both the Chaco and the Mesa Verde. In any case, wherever it came from, we may be dealing with artificial borders when we think of it as essentially a Chaco phenomenon. It may be that when it came north of the San Juan it took somewhat different expressions in the Mesa Verde area, in part because of the kinds of stone available for masonry in that area.

And so, I think that we need to look at a number of aspects here that have not been looked at. We need to plan for some major work in at least one of these large ruins in the Mesa Verde area outside Mesa Verde Park. We need to know what those large structures were like. And we need to know in what respects they correspond to the Chaco phenomenon, whether they are, in fact, different, or whether they are the same thing.

We need to know a great deal more, too, about regional geographies. Obviously, there is some significance for the development of the Anasazi branches in the kinds of areas that they occupied, not only in the natural foodstuffs available in each area, but also in terms of raw materials. I have suggested that the availability of different kinds of stone in the different areas may have shaped the kind of architecture that was possible. The lack of thin, laminar sandstone in the Mesa Verde must surely be considered when comparing the houses there with those of the Chaco and Kayenta.

What about the pottery? The kinds of clays that were available and the kinds of paints that were available, whether mineral or carbon or a combination of those, must surely have affected the appearance of the end products. We must define the relationship that the geographic environments of each of these culture areas had on the development of its own peculiar regional style.

Finally, when we are talking about Anasazi, I think it is clear that in the Kayenta, at least from early Pueblo II on, the course of development was quite divergent from that that we see both in Mesa Verde and in Chaco. One of the questions that I think we need to address is, how similar, or how different, are Chaco and Mesa Verde proper. Are we, in fact, dealing with simply slight regional variations due to geographic areas, or are we dealing with major differences, as the Chaco out-lier theory would imply? When we talk about Chaco out-liers, I think we need to look at what was going on, as I said before, on the next ridge. We need to compare more widely than back to Chaco. We need to compare to those other
sites within the Mesa Verde area proper. We might even like to look at some of those in the Kayenta, to see whether there are major sites there that offer the same kind of contrasts that we see, both within the Mesa Verde and in the Chaco Canyon, between the huge buildings, sometimes called Pueblo III, sometimes called Bonito Phase, and between the ordinary houses of those peoples.
ANASAZI: TOWARD A REDEFINITION
AND CLARIFICATION

Thursday, October 1, 1981
Morning Session
Chair: Joe Ben Wheat
At some point, early on in the history of Southwestern archaeology, a popular phrase, attributed by Dr. A.V. Kidder to Roland Dixon, a mentor and professor (J.S. Dean, personal communication), was that the archaeology of the prehistoric Puebloan people was a “squeezed orange,” that all possible data had been wrung out of it, and it was time to move on. After all, Fewkes had done his study of Puebloan migration myths (among many other projects in the Verde Valley, Canyon de Chelly, Tusayan and Cibola; Mesa Verde had been thoroughly explored and the major cliff ruins mapped and potted; the Basket Makers had been studied; and major sites like Pueblo Bonito and Pecos Pueblo had been excavated. Relative to other areas of the New World, we knew it all, and there were no more worthwhile questions to be asked.

There are important implications of this informal model. Most importantly, the model of a squeezed orange is inward-looking: it implies a finite data set and a bounded, known and mutually held spectrum of theoretical interests. As juice is wrung out of the orange, it is technically possible to know everything about that bounded universe. This model has as its philosophical basis a precept of Boasian historical particularism: gather enough data, and eventually they will speak to you.

This simple-minded model has, of course, fallen out of favor with the modern professional archaeological community, but no comparable visual analogy has risen to take its place. On the other hand, the idea of knowledge as finite and bounded has remained quite popular to the lay bureaucracy and private industry. In an effort to combat this prevailing epistemology, I would suggest replacing it with an equally simple alternative; in keeping with the vegetal analogy, I prefer to call it the Apple Pie Theory of Knowledge.

Apple Pie versus Squeezed Orange

I was first introduced to this concept several years ago by an artist and Renaissance man from Tucson (Bill Klesert, personal communication). Originally known as the circle theory of knowledge, it dovetails quite well with current feelings in science about the nature of data and knowledge. Today, most archaeologists work on the premise of a conceptually infinite, unbounded range of theoretical and methodological possibilities. While we must all be aware of the finite nature of cultural resources, the potential information garnered from these resources is infinite. To accompany this new realization, I would suggest that instead of an orange, with its finite, bounded dimensions, we look upon the Anasazi data base as an ever-expanding apple pie (Figure 1). If we view the pie...
Understanding the Anasazi... from above what we know of the Anasazi is inside the pie and what we don’t know is outside, in an unbounded, infinite space. This is already a significant improvement over the squeezed orange, since it is outward-looking and effectively accounts for the unbounded nature and potential of scientific data.

The apple pie model has one more significant improvement over the old model, however, and that is the crust: the interface between what we do know (the filling) and what we do not know (the universe) can be seen as what we realize we do not know. As our universe increases, the pie grows larger, taking up more and more space, but the interface/crust, what we are aware of not understanding, also gets bigger. In other words, the Apple Pie Theory of Knowledge suggests that as our understanding of the Anasazi grows, so must our realization of just how much we have to learn. The greater our knowledge, the greater is our comprehension of our fundamental ignorance. The usefulness of this simple model in archaeology can be discussed in terms of our theoretical and methodological approaches to the study and continual redefinition of the Anasazi, as well as in the general process of CRM. But first, I will briefly show how some other scientific fields illustrate this model.

Examples from the Sciences

One of the most innovative sub-fields in computer science is the study of artificial intelligence. AI studies, as they are called, consistently involve extrapolation into unknown areas, regions outside the pie, and the result is often a concomitant expansion of the pie around these points. The creation of artificial intelligence through the comparision of AI to human intelligence (Simon 1981) or through the study of the essence of analogies (Hofstadter 1979) are examples of this trend. In terms of the model presented here, it is significant to note that experts in AI agree that they are essentially no closer to developing or even defining AI and its criteria than they ever were. As one put it, AI is defined as what computers cannot yet do: as new computer capabilities are invented, suddenly such abilities are not considered really definitive of true intelligence after all (Hofstadter 1979).

The interface between the known and unknown in geology has usually corresponded to time, and secondarily to explanatory mechanisms. Since the days of Bishop Usher, geologic time has been steadily rolled back, and the chief result of this new understanding has been a profusion of new questions arising from a continual need to resolve such a geometric or exponential expansion. A major current geological mechanism is plate tectonics, or continental drift (Hurley 1968; Ben-Avraham 1981). While many in geology consider this model essentially proven (Hurley 1968), it is clearly a controversial idea, one which has created its own generation of problems to be solved, including the causes of shifts in the magnetic poles and the existence of oceanic plateaus and ridges (Ben-Avraham 1981). As the study of the earth’s crust proceeds, the pie crust in our model is expanding and shifting as well, exposing many new problems to take the place of older, defunct or resolved ones.

Astronomy offers a striking visual realization of the apple pie model, in a three-dimensional form. Ever since Edwin Hubble employed the red shift to calibrate the size of the galaxy and known universe, our perceptions of the size and age of the cosmos have expanded dramatically (Sandage 1956). But along with this new dimensional framework came a host of complex problems, once considered essentially solved in the old Biblical universe. Problems such as the origin of the universe; whether it is expanding, contracting, or both; the evolution of stars; and the existence of anomalies such as quasars, pulsars, black holes, radio stars, etc., are all a direct consequence of the epistemological expansion of the universe in space and time (Gamow 1956).

A more recent and closer to home example from astronomy, the Voyager missions, also amply demonstrates our model. The rationale for these missions, at least to the public (who, we recall, operate under the squeezed orange model), was to “solve”
The Anasazi as Apple Pie

In this section, I will discuss the nature of our understanding of the Anasazi from the point of view of our model; that there is no end in sight, no ultimate point at which we will have squeezed all the information we can out of the Anasazi data base. Examples of this inevitable trend will deal with theory building and methodology, but the validity of the model can also be seen on a more general level. Years ago, major topics of interest included culture history and typologies, and little thought was given to anything beyond that (e.g. Roberts 1929). More recently, as these topics have become old hat, higher level, theoretical concerns have become pre-eminent; but these are of concern in addition to the more mundane issues of chronology and classification, not instead of them. Any adequate study of the Anasazi today must begin with time-space systematics and then use these as the basis for additional work. In other words, the "new archaeology," done correctly, has increased our range of questions, problems and issues, not simply shifted it. The study of Anasazi social organization is a good example of a relatively new line of inquiry which has increased our concern with old problems, rather than eliminating it. As studies of social groupings within sites have been examined (Longacre 1970; Hill 1970), it has become of vital importance to re-examine and re-test our old assumptions about such well-worn ideas as the use of ceramic designs as temporal indices (S. Plog 1978), or the effects of site depositions processes on artifact locations (Schiffer 1976). Prior to these studies, such cross-examinations were unnecessary; now, old artifacts and assumptions are being scrutinized for new data.

The causes of abandonment have also engendered more controversy now than they ever did in the past. As a theoretical issue 50 years ago, causes could not have been easier to find: the Great Drought, famine, disease and raiding Athapaskans. Of course the longer we studied these catastrophic agents the less convincing they became, and our accumulation of information on the subject has only led us down the path of complexity and confusion. While it now appears that no single event "caused" the downfall of the Anasazi, we are, if anything, further away in our own minds from finding an adequate explanatory, theoretical model for it (Klesert 1981). The fact that an entire session of this symposium is dedicated to this "squeezed out" issue seems to suggest that, once again, the inevitable result of the acquisition of knowledge is an awareness of what still must be learned.

Studies of subsistence and settlement have also resulted in a flood of new questions. The Anasazi, once described as "peaceful farmers" were in fact, we now realize, capitalizing on a bewildering range of foodstuffs in addition to cultigens; and to make things more complicated, combinations changed through time and across space. The realization of this fact immediately led to a re-examination of such diverse (and supposedly resolved) realms as the causes of abandonment (as just mentioned), the effects of agriculture on social organization (Klesert 1979), and the seasonal nature of so-called "permanent habitation" and "special-use" sites (Powell 1980). These in turn have forced a reassessment of population figures and levels of socio-economic organization, which in their turn have directly affected still other domains. Clearly, new understandings of given topics tend to lead to a greater understanding of our own ignorance, rather than to any particular feeling of omniscience.

Methodologies have taken the same route as theories. A direct result of the awareness of the importance of smaller, seasonal sites, just mentioned, was a re-examination of sampling procedures, away from intuitive samples concentrating on the goodies, towards statistical samples concentrating on "representativeness." At this point, such a seemingly simple shift in emphasis has opened several large-scale cans of worms, including how to interpret small 'non-sites" (Thomas 1975; Powell and Klesert 1980), how to choose the most precise and/or efficient statistical sample (Plog et al. 1978), and how to justify stratifications along natural or cultural lines.

Input from the "hard" sciences has consistently improved the precision of Anasazi archaeology, especially in terms of new techniques for accurately dating deposits. This, naturally enough, has often meant unpacking old artifacts and samples to recover new data. In many instances, however, the needed data were never collected, since at the time they were considered worthless (that is, outside the closed universe of the particular orange being squeezed at the time). A particularly colorful example of this phenomenon was the Wetherills' habit at Pueblo Bonito of using old roof beams for firewood in their trading post!

The new emphasis on CRM in the archaeology of the Anasazi (and elsewhere) has, in the same sense as the new emphasis on theory, resulted in an additional layer of concerns being placed on the archaeologist. While many consider CRM to be something less than "pure" archaeology, I feel that it involves much in addition to the old, straightforward science of archaeology. Once again, we find ourselves pushing out the crust of our pie, enveloping many new aspects of knowledge, and having to come to grips with their
complications and implications. Business administration, law economics, and public relations must now be added to geology, botany, soil science, paleontology and orienteering as basic archaeological requirements.

In terms of public relations, the model being presented here may be of value. As mentioned earlier, a pervasive attitude shared by the public is the "squeezed orange" perception of science. This is expressed by clients as: "You folks have already done lots of surveys in Colorado; why do you need to do any more?" This generally leads to an exchange in which the participants are totally at cross purposes to each other. A recognition of the conceptual basis for this question being asked, however, might go a long way towards resolving this conflict. While many people may never be willing to adopt such a stance, they will at least be aware of its existence.

Conclusions: The Structure of Inquiry

To conclude, the apple pie model presented here can be discussed in terms of a quote attributed to the Roman philosopher, Horace (65-8 B.C.), who said: "To know all things is not permitted." Note that Horace feels that omniscience is not simply impossible to attain, it is an unsuitable goal to pursue. This outlook closely parallels the apple pie model in that while it is extremely useful to continually force the piecrust to further and greater dimensions, it is always implicitly recognized that the unknown region outside is infinite. As an infinite space, it is, technically speaking, essentially unknowable; the vast majority of this infinite region will, by its very nature, never come under the domain of the pie. To phrase this another way, the search for absolute truths, or definitive statements which become absolute doctrine, is a patently impossible and unworthy goal for the study of the Anasazi.

In addition, it is also a part of the model that as one delves more and more deeply into any given subject, contrary to popular expectations, one only becomes increasingly aware of the amount of problems and confusions which expand proportionally with the newly gained knowledge. The issues at hand are being taken care of as we expand the dimensions of our apple pie, but they invariably are replaced by innumerable, and until then unsuspected, additional issues and concerns.

We can see that this model is a significant improvement over the former squeezed orange model, which viewed the study of the Anasazi as an inward-looking, finite process with an eventual and known endpoint. The Apple Pie Theory of Knowledge, on the other hand views scientific study of the Anasazi as open-ended, outward-looking, and essentially infinite in potential, with no hard and fast set of goals beyond which we cannot extend ourselves. Such is the nature of scientific exploration, and such is the nature of the study and continual redefinition of the Anasazi.

Questions and Comments

Question: What computer data base for southwest archeology is available?
Answer: For locational information, the SARG computer banks at Arizona State University are pretty complete.

Joe Ben Wheat: The Southwest is one of the very few areas of the world where we do know enough that we can actually do a variety of other kinds of anthropological research within the archeological data base, and that is certainly not true of many areas of the world. Not only do we need to go forward and expand it, we ought to use it, as we are using it in many cases now, for other kinds of anthropological studies than just the gathering of new data.
THE FALLACY OF ANALOGY IN ANASAZI CULTURE HISTORY

Theodore R. Frisbie

For those of us who have restricted our primary interests to the Anasazi cultural continuum there are a number of features which we have long taken for granted. Foremost among these is the fact that there are living pueblo peoples who are the direct descendants of those of the archaeological record. One cannot fully appreciate how incredibly fortunate we are until one has worked outside of the Southwest in areas where this is not the case. For example, midwesternists with whom I share an academic affiliation are in awe of our ethnographic data and the way in which it may be used to support archaeological inferences. With the exception of a relatively meager midwestern ethnohistoric record there are virtually no data relative to archaeological inquiry.

Our data base for analogy derives from a great variety of source materials, although those from which most investigators draw are the classic ethnographic present descriptions (ca. 1875-1930). These are particularly suitable since they reflect a more pristine ethnographic record than that of recent times. However, recent work in ethnoarchaeology clearly shows that much can be done to provide precise information geared toward explication of the archaeological record. This approach has a long history in the Southwest. As a matter of fact, Fewkes (1900:579) must be credited with coining the term “ethno-archaeology,” in its hyphenated form, to describe his work with Hopi clan migrations. Since his time ethnoarchaeology has come of age with a number of southwesternists, and more particularly Pueblo specialists, among its ranks (Frisbie n.d.). The ongoing Walpi project is, in my opinion, the most noteworthy example to date; however, frequently there are data from the various pueblo land and water claims studies which relate to traditional land use and tenure patterns that are of great importance.

Although one may emphasize the positive aspects of our interpretive endeavors, there are also a number of negative ones. It is to these that the present paper is addressed. The paper was inspired in part by my personal reactions to a number of archeological studies which I feel have been cramped and overly controlled by both the confines of the literature and/or recently uttered words of living representatives of a culture which has undergone significant changes in all areas of anthropological concern. Frequently archaeologists do not distinguish between “real vs. ideal” culture, and some are content to incorporate “memory culture” (behavior which may not have been practiced for decades or even centuries) into their explications. Further, because culture is a dynamic phenomenon one must beware of pitfalls awaiting us; some significant changes are rapidly incorporated and quickly become verbalized as the norm through time, even when such patterns may not have had any (or very little) bearing on the past. As archaeologists become more sophisticated in their approaches to the past it becomes increasingly important that models,
hypotheses, theoretical viewpoints, and premises are grounded in fact rather than fictionalized half-truths. Perhaps what I am really saying is that because we are looking at Anasazi culture through systems theory and processual approaches which transcend an emphasis on "things," we are, for the first time, ready to identify causality through factors which escape the confines of normative thought. This is difficult to accomplish because traditionalism is deeply imbedded in our mental templates. To suggest that what we take for granted, based on ethnography, is not congruent with the facts is a bitter pill but one which I believe we must swallow.

My perception of the Anasazi continuum is analogous to a layer cake; however, because of the number of layers and their richness, what I am really considering is a torte with considerable marbling within each layer. At the outset it is important to understand that for a very long time the entire Greater Southwest was characterized by a common culture ("Desert Culture") characterized by a hunting and gathering (i.e. foraging) adaptive strategy. The duration of this culture is incredible—ca. 8000 B.C. to 1850 A.D. (Great Basin) in virtually unchanged form. Without question a shift to a horticultural strategy and gradually pushed into the northern frontier area, including the southwestern U.S. Slowly culture reached this point first, and therefore have been dubbed "The Mother Culture." They were first in a long series of rises and falls: Maya (to the south), Teotihuacan, Toltec, Aztec, as well as a number of others within Mexico. In a northward movement of traits and complexes of traits, the Southwest received numerous stimuli as time progressed. While there may well have been some instances of direct contact, these await conclusive identification. Nevertheless, the Mesoamerican hearth provided the major stimuli for cultural change until a new power intervened, namely, the Spanish. Until that time, as well as for some time after, there were thousands of years during which interactions of various types took place between the Southwest and Mesoamerica (Kelley and Kelley 1975, Riley 1974, 1975a, b, 1976, 1978). These interactions are reflected primarily through archaeology and brief mentions in ethnohistorical documentation. The remaining groups, Athabaskans, Spanish, Mexicans, and Americans, continue to be part of the living cultures of the Southwest, although the Spanish element is somewhat tenuous unless you live in secluded enclaves of northern New Mexico. By deriving interpretations from work with the living and from written reports, we are over four centuries away from Mesoamerican connections as they last occurred with regularity, and over a millennium from the beginnings of the Chaco Phenomenon (See Fig. 1). Present patterns, although beautifully integrated and suggestive of earlier systems, may not reflect that which was essential prior to Spanish contact. Even at contact we are told all of the Pueblos were egalitarian, autonomous units. This I strongly doubt. I believe there were vestiges of a higher level of sociocultural integration at the time of the Spanish Entrada at Zuni and elsewhere (Frisbie 1980:65 n.1).

Based on recent synthesizes, Adams (1981:324) suggests there may have been "a more powerful central authority in the fifteenth century" at Hopi because of the population concentration at that time. He also suggests that the kachina cult may have acted as a mechanism for redistribution of food and that "central authority and redistribution of food are indicative of some social stratification in Hopi society and the development of incipient chiefdoms."

For the Rio Grande Pueblos, Snow (1981:355) states that:

"...society is based, in part, on a status and role hierarchy among community members whose activities, responsibilities, and interactions are intricately balanced between alternating agricultural and non-agricultural phases of the annual subsistence and ritual cycles. Access to successive status roles is provided by formal rituals of passage through life and into death."

The greatest complexity within the system was extant during the Pueblo IV period following amalgamation into large communities. The system was greatly disrupted following the Spanish Entrada and subjugation. Historic documentation lacks indications of even incipient chiefdom organization.

In assessing the above I concur with Wilcox (1981) that analogy based on ethnography and historic documentation must be reassessed with an alternative theoretical viewpoint to present an accurate portrayal of Pueblo society for the Protohistoric Period. Wilcox (1981:382) notes that "the modern conditions described as Pueblo 'autonomy' may be the outcome of a relatively recent historical process. Further, reasons independent of analogies drawn from the ethnographic present are required to show that the protohistoric Pueblos too were autonomous in some sense." He then cites the few examples from ethnohistoric documentation wherein, contrary to general opinion, there are indications of the regional clustering of villages with a council in control for the Tiguex Province, as well as the suggested addition of a paramount chief for the Tompiro and Zuni (Ibid.:384-385). Further, based on ethnohistoric sources, alliances between the clusters are identified by Wilcox (Ibid.:386). These are, indeed, important data and, along with other similar indications, need thorough reanalysis before more than suggestive comment can be presented.

Ellis (1981) provides comment on the previously mentioned papers by Adams, Snow, and Wilcox. She supplies a wealth of information either in support of or to the detriment of these studies. Citing specific examples from the 19th and 20th centuries she demonstrates precisely how the present system operates to
ANASAZI EXTERNAL INFLUENCES

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<td>CHACO PHENOMENON</td>
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<tr>
<td>1120 AD</td>
<td>P III</td>
<td>Mesoamerica</td>
<td></td>
<td>Beans</td>
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<td>970 AD</td>
<td>P II</td>
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<td></td>
<td>Ceramics</td>
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<tr>
<td>700 AD</td>
<td>BM I</td>
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<td>Improved maize/squash</td>
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<tr>
<td>400 AD</td>
<td>BM III</td>
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<td>Primitive maize</td>
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<tr>
<td>200 BC</td>
<td>BM II</td>
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<td>3500 BC</td>
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<td>8000 BC</td>
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Figure 1.

DESSERT CULTURE OF GREATER SOUTHWEST
[Basal—Hunting-Gathering (Foraging) Adaptive Strategy]
The Fallacy of Analogy . . .

account for past practices and procedures. In essence, she does not feel there are sufficient data to imply the existence of stratified social systems in the past; rather, organization was based on “the concept of ‘noncoercive egalitarian society’” (Ibid.:414). Ellis is in total agreement with Ferguson (1981). For Zuni, Ferguson states that there are no historical documents which indicate that Zuni featured anything more than a “‘tribal society governed by a religious council of elders’” (Ibid.:342). There is no indication of the concentration of power in one political office.” He notes, however, that there were “undoubtedly differences in wealth, authority, and power between individuals and between the six villages, all indications that Zuni was basically a non-coercive egalitarian society.” The amalgamation of the six villages into one at Halona following the Pueblo Revolt was probably possible because religion provided a focal point for integration. Nevertheless it allowed “great political autonomy for individual people and villages in many matters while at the same time providing a means to organize communal work groups when necessary.” (Ibid.)

Although Ellis’ arguments are cogent (in part following Ferguson’s reasoning), one may question their applicability to the period in question. The analogous model as presented assumes that all of the perturbations during the past four centuries (and more) have been taken in stride and have resulted in an essentially unchanged total social situation. I would strongly disagree. I believe that in order to arrive at an accurate appraisal of the protohistoric system (social and otherwise) one must recognize what preceded rather than succeeded the period. Without question the highest level of attainment within Anasazi culture occurred during the Chaco Phenomenon (ca. 970-1120 A.D.). Whether one wishes to accept it or not, all recent researchers indicate a level of sociocultural integration synonymous with the chiefdom or ranked society, and have described it as such (Frisbie 1972; 1978:210-216, 1980; Grebinger 1973; Judge 1979; Altschul 1978; Cordell 1979a:101-103, 1979b:149-150; Tainter and Gillio 1980: 114 and others). What this implies is that the Pueblo peoples have undergone a significant culture loss beginning with the fall of the Chaco Phenomenon. It is to be expected that aspects of cultural survival should be encountered, and it is to these organizational principles that our research should relate.

Although it might well be possible to provide additional data for hierarchial ordering within other pueblos at the time of the Spanish Entrada, I shall restrict my comments to Zuni with which I am most familiar. It is my contention that the complexity of organization at Zuni is a direct result of the amalgamation of the six villages. The original religio-political structure was retained and became more highly integrated. The organization appears to have been sustained during periods of population decline as well as other vissitudes (see Simmons 1979:185). It remained intact well into the American period. Serious factional difficulties arose in 1943 which resulted in the highest ranked personage, the Sun Priest, (Pekwinne or “Spokesman of the House Chiefs”), not performing the duties of his office. He ultimately moved to Gallup (Leighton and Adair 1966:58-59, 143) and Tedlock (1979:507) notes the office became vacant in 1952. This, the highest religious office at Zuni, unquestionably figured importantly at the time of contact. Based on the most recent syntheses [Ladd (1979), Eggan and Pandey (1979) and Tedlock (1979)] it would appear that the Zuni system operated so that the Pekwinne did not deal directly with people either within or outside the pueblo. His function was religious and any actions he might suggest would normally be considered in council with the Rain Priests and War Priest (Elder Brother Bow Priest). Decisions would then be carried out by the Elder Brother Bow Priest, assisted by Younger Brother Bow Priest as well as other members of the society as necessary.

In a previous publication (Frisbie 1980:65 n.1), I identified the site of amalgamation for Zuni, Halona, as the probable location of central authority because it is considered to be “Middle Place.” More recent research, however, has shown my suggestion to be in error since it would appear that Matsaki originally held that honor. According to tradition, Stevenson (1904:64) cites this as the center of priestly power at an earlier period. The personage, Pekwinne, resided herein and observed the sun, utilizing a specially-erected stone shrine bearing a petroglyph of the sun. Stevenson (1904:148) states:

“The pekwin observes Yallanna (Great Mountain), a mesa northwest of Zuni, from the shrine at Matsakia (see Fig. 3) for a number of evenings prior to the summer solstice. The sun strikes a certain point of this mesa at sunset for five consecutive days. The Zunis say that the Sun Father rests five times in succession over this mesa in his daily journeys over the world. At other times he does not halt twice in the same place.”

I suspect this individual was the elderly male Coronado saw in 1540:

“who said he was their lord, came with a mantle of many pieces . . . [with whom I argued as long as he stayed with me]. He said that he would come to see me with the rest of the chiefs of the country, three days later, in order to arrange the relations which should exist between us. He did so . . .” (Winship 1904:179).

Similarly, Marcos de Niza’s statement made prior to the Spanish Entrada at Zuni adds further credence to the idea of a paramount chief or leader:
Here I met a man, native of Cibola, who said that he fled from the person whom the ruler had appointed in Cibola, because the lord of these seven cities lives and has his seat in one of them... and in the others he has placed persons who govern in his name (Hammond and Rey 1940:72).

Both of these instances clearly indicate a hierarchical arrangement to integrate and control the functions of the villages. Although one cannot with certainty state which "lord" the Spanish Entrada actually met, almost certainly the War Priests would have had first contact; later, after considerable urging, Coronado met either the head Rain Priest or the Pekwinne himself. Since one might expect the largest village to be the central locus of authority, Matsaki appears the likely location at Spanish contact. Pueblo Bonito in Chaco Canyon has produced lavishly outfitted burials indicating a probably similar situation there. Parenthetically, if excavated, I would expect lavish funeral offerings to occur at Matsaki. To date excavations at Zuni sites have not produced indications of high status burials other than what Hodge (1920:148) termed deer chiefs at Hawikuh, based on his worker's statements. Although Ladd (1979:489) suggests the location for the center of activity to have been either Hawikuh or Kechipauan, I do not believe this contention can be supported. Further, Schroeder (1979:252), based on early sources, notes that at contact Matsaki was the largest of the six pueblos, followed by Hawikuh. The other four were of considerably smaller size—40-100 hearths (houses) compared to 200 for Hawikuh and 300 for Matsaki (see also Eggan and Pandey 1979:481).

Of particular interest in a discussion of hierarchical ordering at Zuni are the Ashiwnani or Rain Priests. There are 12 of these and as a group they wield a great deal of religious and political power. Four of them, however, are of higher rank because of their association with the cardinal directions; among these, the Rain Priest of the North ranks highest. As a group the four highest ranking Rain Priests, along with the War Priest (associated with the nadir and assisted by the Younger Brother Bow Priest), and the Pekwinne (associated with the zenith), made up the highest level of the hierarchy (Leighton and Adair 1966:48). These six personages, collectively known as the Daylight Priests, could be assisted in council by the remaining six Rain Priests, also known as Night Priests [see Tedlock (1979:507) for an enumeration of them]. That this group has been extant for at least since the time of the Spanish Entrada seems highly likely. I suggest that the twelve Rain Priests were originally divided two per village prior to the amalgamation at Halona. Further, I suspect that the Rain Priest of the North resided at Halona, since it was the northernmost pueblo, and that each of the other three cardinal designates resided in the directionally appropriate village: east—Kyakima, west—Hawikuh or Kwakina, and south—Kechipauan.

I base this assumption on the fact that the sacred bundles associated with these priests are still retained in directionally associated houses within the old village at present (Tedlock 1979:506-507). A further lead is provided by the term "Kakwemossi" which translates "House Chief" (i.e., "Village Chief") and is used to designate each of the 4 directionally oriented Rain Priests. As would be expected Rain Priest of the North is the House Chief proper (i.e., head priest).

Operationally, there are two to five ranked male assistants for each of the twelve Rain Priests or Ashiwnani. Usually the highest ranking assistant became the successor when the position was vacated, normally through death. In contrast, the War Priests functioned to assist its head designate. The Pekwinne and Rain Priest of the North must either be members of the Dogwood Clan (picciwke) or "a child of the clan" through their fathers' membership. While some of the other priests and their priesthoods are associated with specific clans, this is because the clan is associated with the storage of their sacred bundles in the house of one of its members (Kroeber 1917:166-167).

Although the Dogwood Clan is of greatest importance, inheritance of priestly office in this and other clans is not automatic within a lineage. Those selected to serve must manifest the proper temperament and personality for duties they are to perform. While serving, priests (particularly the Pekwinne) are provided with materials essential for life by the people whom they serve. Since there are fourteen individuals, minimally, within the priestly category, considerable goods and service are required. The operation of the system at present in this and other pueblos cannot be compared by analogy to that of the past. Because the agricultural base has shifted to a more westernized cash-based economy and because of other acculturative forces, the people are no longer willing and often not able to provide what was formerly required. Therefore, as Ellis (1981:424 and elsewhere) has noted, there is reluctance or absolute refusal to serve. Those who do are frequently the poorest members of their pueblos.

The final priest of the highest ranked group at Zuni, War Priest, is associated with affairs of the world, essentially dealing with outsiders. This might involve warfare, trade or other activities. Since Hawikuh was without question the focal point for outside contact with Zunis, War Priest's residence at Hawikuh seems logical. Riley (1975a) has termed the location the nexus of trade since it relates to a vast network including contacts with the eastern pueblos as well as Mesoamerica. Hawikuh is also most frequently mentioned within the oral tradition for trade relationships with Navajos and Apaches prior to the time of amalgamation (Benedict 1935:170-175,
207-210 and others). Further, Hawikuh was the focal point for the Spanish Entrada and later contact by other Spanish expeditions. The trails or roads led to Hawikuh and it was here that the Zuni were best prepared to meet outsiders.

Viewed in this way, Zuni organization for both religious and political control was harmoniously arranged within their established world view, something which was and continues to be of considerable importance to the maintenance of the system (see Riley 1963). As indicated above, there are provisions for one of the six major figures to have occupied each of the six villages of the historic period. The remaining six Rain Priests (i.e., the Night Priests) could have been equally divided as well, to provide an ideal balance of power within the Zuni region; this may or may not have been the case. The identification of active shrines at some of the sites might help clarify the situation.

For Zuni, as previously noted, factionalism resulted in the cessation of duties of the Pekwinne in 1943, and the position lapsed in 1952. Obviously, then, in the past the system had to have operated so that the honor and prestige of serving carried with it sufficient power through rank and privilege to maintain the priesthood. In general terms, however, it would appear that the system was perpetuated not by applying force, but rather by giving a designated portion of goods and services, some of which could be returned, in part through a redistributive process such as feasting or assistance to those in need. Clearly what happened within the pueblo system was that the priestly authority became unable to extract its due. That these individuals are frequently poor today does not reflect with any degree of accuracy what might have been expected in the past. The few true status burials of record, which begin during the Chaco Phenomenon and are followed by “The Burial of the Magician” at Ridge Ruin (McGregor 1942) and Burial 140 at Grasshopper Ruin (Clark 1967; Griffin 1967), provide, in my opinion, a much more accurate portrayal. Because of the variety and wealth implied by the grave furnishings, it has been suggested that these individuals might have been involved in a trading network, possibly involving a pochteca-like organization (Frisbie 1978 and elsewhere; Reyman 1978 and elsewhere; Kelley and Kelley 1975; and others).

Although Ellis (1981:426) cannot accept status positions resulting in considerable individual wealth based on present analogous evidence, one need not, indeed should not, seek to make the comparison with the living pueblos. The system is radically different and I wholeheartedly concur with Kelley (1981:436) where he states:

[Ferguson 1981] recognizes the key position of Zuni as a trading center, with interchange relationships extending into northwestern Mexico, but he does not further develop the implications of these data. He describes this wide-spread trade relationship as one involving primarily luxury items and commodities, but significantly, concludes that: ‘It is unlikely that the economic leaders in trade at Zuni were the same men who governed the tribe as religious leader.’ If so the situation at Zuni differs greatly from that encountered so frequently in Mesoamerica—and in the Old World—where trade in luxury items was largely carried out between elite members of two societies, who were usually also part of the religious hierarchy. In Mesoamerica, in fact, it is believed that such trade soon resulted in the development of an elite religious group in an evolving stratified society.

Is Zuni then such an exception or do we have to take into account associated historical factors? Perhaps, at Zuni as well as elsewhere in the Southwest, our conceptualization of long distance trade has been warped by developments in northwestern Mesoamerica during the late prehistoric period. There, lucrative luxury trade with the Southwest appears to have been largely usurped after A.D. 1250-1350 by the developing Tarascan ‘Empire,’ in this Tarascan trade entrepreneurial activity (even here under the aegis of royal families) appears to have replaced earlier long distance trade out of central Mesoamerica which was a component of institutionalized ‘state’ controlled external commercial relations. In its own way the Tarascan state seems to have been as aberrant in Mesoamerica as Zuni appears in the Southwest.”

Kelley (Ibid.:439) further states:

. . . I see the Southwest incorporated early in the Mesoamerican world structure as an exploitable periphery but lacking strategic value.

Following this hypothesis, the Southwest was largely dropped from the Mesoamerican World System when access to its exotic goods was interrupted by internal events in central and western Mexico. The culture, especially the economy of the Southwest following about A.D. 1350-1400, is to be regarded therefore as the slowly re-integrating ruin of a formerly highly organized economic system formerly managed from without the area.

I believe the Spanish disrupted a system which was in the process of rebuilding in more restricted geographic areas following the Great Drought of AD 1276-1299 as well as lesser periods of environmental and other forms of stress. At the time of contact, most pueblo locations offered maximum potential for survival through large tracts of arable land and other resources. Further, areas selected frequently were associated with a variety of ecozones maximizing availability of resources. The Spanish were met with formidable organized resistance at their first encounter at Hawikuh (see Ferguson 1981:344); however, once the pueblo had fallen, the stage was set for the continued secretive, internal religio-political actions. In part these reflected the system as it had ex-
Latin American connections (Ibid.:42, 58). Simmons revision and is not analogous to its original positive point. The quality of evil, associated with the bat in like one, acted like one, or simply received this Adventures in Zuni" during the late 19th century, European evil connotation and no longer included as those with power (curing/religious practitioners) could larities between Spanish and Pueblo witchcraft. It is present day Pueblo culture has undergone drastic change in the emergence from the underworld. Although archaeologically figured in a few petroglyphs (Schaffsma 1981) and on ceramic vessels, with possible similar associations to the above, the bat was relegated to its mythic association. 

One of the specific changes of the system related to the practice of witchcraft. Parsons (1927; 1939:1065-1068) was the first to note the many similarities between Spanish and Pueblo witchcraft. It is now generally accepted that pre-Entrada Pueblo witchcraft reflected the widespread native system wherein those with power (curing/religious practitioners) could use it in either good or bad ways. Leighton and Adair (1966:146 n. 11) suggest for Zuni that, “During the period of Spanish rule, when the religion of the people was endangered, group anxiety must have reached a high pitch and provided an ideal psychological climate for the borrowing of European witchery.” In her excellent overview of pueblo witchcraft, Ellis (1970) cites Parsons for specific parallels and adds some practices which may derive from prehistoric Latin American connections (Ibid.:42, 58). Simmons (1974) provides the most recent overview for the Southwest giving extensive coverage to the Pueblos.

An instance of documentable culture loss relating to witchcraft may be shown in the example of the bat which figures in the Emergence Myths of the Navajos as well as in the ceremonialism of Mexican Indians (Frisbie 1981a, b). Herein bat and butterfly assist with the emergence from the underworld. Although archaeologically figured in a few petroglyphs (Schaffsma 1981) and on ceramic vessels, with possible similar associations to the above, the bat was relegated to its European evil connotation and no longer included as “helper.” Cushing (1967:44-45) in his “My Adventures in Zuni” during the late 19th century, reports on his intervention in (temporarily) saving an elderly male at Zuni from death during his witchcraft trial; the latter was called “Bat.” Whether he looked like one, acted like one, or simply received this nickname because of his behavior remains a moot point. The quality of evil, associated with the bat in present day Pueblo culture has undergone drastic revision and is not analogous to its original positive mythic association.

Among other aspects related to witchcraft is the likelihood that one who acquires more than his/her neighbor has done so through evil means. This may lead to accusations of witchcraft, a practice which I feel reflects an historic addition, although not in every case. I have elsewhere indicated that acquisition of shell and turquoise was and continues to be, in actuality, sanctioned by the gods (Frisbie 1975a:132-133). This follows what I expect to have been the old Puebloan system. I further believe these two materials formerly functioned as fixed media of exchange (i.e., money in specific finished forms (Frisbie 1975, n.d.2, see also Riley 1975:156).

I believe it is instructive to look at what we construe to be wealth within the Pueblos. A clear distinction must be made between personal wealth and that which is held in trust, as well as that which must be considered transitory wealth. An individual’s personal property consists of clothing, jewelry, perhaps some ceremonial objects, and a variety of miscellaneous items some of which might be associated with vocation or avocation. In the latter instance tools of the trade, raw materials and finished products (i.e. weavings, pottery, baskets, etc.) are to be included. In actuality, however, much of the property associated with a household is generally “family” owned and related to the maintenance of the group (including foodstuffs).

With the exception of jewelry, what we normally consider to be “luxury goods” are most frequently clan or religious society property or “equipment.” While these may be used by individuals, ownership remains always within the group. Such objects are sacred and requisite for the proper functioning of the group.

Individual membership in a society or other group also entails the loaning of personal materials (jewelry and other items) to ornament and properly dress a dancer or kachina on ceremonial occasions. This I have seen on several occasions at Zuni when the individual was not himself or herself directly involved with the performance. For those who raise or keep turkeys, an eagle, or ducks, the feathers are in large part “presented” to the organization(s) to which the individual belongs. Those left over may be considered “personal” and may be used for trading; however, needs of the organization come first and if these are not met when there should be sufficient plumage, difficulties can arise (Frisbie 1966).

Property held in trust involves the storage of foodstuffs. In traditional fashion, the village chief ( cacique or “mother of the people”) was provided for, and in addition held (extra) supplies for those who were in need when unforeseen problems arose. It is possible that in the past such holdings were considered a form of wealth if the individual could expend some of these materials for personal gain; however, such has not been the case in the recent period. Within ranked society or chiefdom-like organizations such materials become an integral part of a redistributational system which serves to maintain and enhance the position of...
the status-holding individual.
There appears to be some question with respect to
devices of office, insignia, staffs of office, and so
forth. As is well known, the Spanish and Lincoln
canes of office are held by civil officers within the
pueblos for the duration of their terms and then pre-

tered to the incumbent as he takes office. The
archaeological record contains what appear to be similar
items interred with the individuals, particularly staffs
(see, for example, McGregor 1943). Thus, if these objects
are properly interpreted, as I think they are, at
least some were considered personal property and in-
terred with the deceased.

Finally, there is property which I have termed
“transitory” items which an individual holds for
trading purposes. Although such goods might be
interpreted as “personal” since they are usually
owned by the trader, their use involves exchange for
something else. This, in turn, might become personal
property or be held for future transactions. In view of
the fact that the trader is always interested in
“coming out ahead” in order to make a living, those
who are highly successful should show significant
gains in the quality and quantity of goods involved
(see Frisbie 1975:129; Adair 1944:123-125). I believe
the successful trader of the recent period would prefer
to put goods in the “transitory” category, whereas at
an earlier time such goods would have been treated as
“wealth.”

Another area where analogy is not useful at first
appearance relates to exclusive ownership of
resources. Ellis (1981:417) states:

*I definitely do not agree with Schroeder, quoted by
Snow, that historic Pueblo tribes did not claim
exclusive ownership of resources on lands adjacent to
their homes until recent land claims raised the issue.
Elders explain that a Pueblo tribe owned the land it
used for farming and also for other purposes and
shrines marked the natural feathers (sic features)
considered to be boundary points (Ellis 1966). . .
Resources within the tribally marked land belonged to
the tribe, but those outside such areas were considered
native god-given and for general public use.

Among the god-given group she includes salt from
Zuni Salt Lake. On this point I disagree. The lake
clearly falls within the traditional land use and is
evidenced by recent research (see Ferguson 1981,
map 2.) Historically, salt has been considered a “free
resource.” Cushing (1896:354), however, states:

*When found by the Spaniards the Zuni-Cibolans
were still carrying on extensive trade in this salt,
which for practical as well as assumed mythic reasons
they permitted no others to gather, and which they
guarded so jealously that their wars with the Keresan
and other tribes to the south-southeastward of their
country were caused—as many of their later wars with
the Navajo have been caused—by slight encroach-

ments on the exclusive right to the products of the
lake to which Zunis laid claim.”

It is my contention that Zuni Salt Lake provided
one of the valued commodities within the Chacoan
Interaction Sphere and that its very name implies a
long and close connection to Zuni. I have been told
that according to legend there was a wide, straight
road leading to the lake from Zuni, made by Old Salt
Woman as she moved from Blackrock to her new
location at the lake. The Zuni not only state that the
location is the home of Malokatsiki, Old Salt
Woman, but that:

Her residence is adjacent to the home of the Zuni
War Gods, who reside within a volcanic cinder cone
located at the edge of the lake. This association is
unique, and I believe, represents a clue to an earlier
time when the Zunis might well have claimed exclusive
ownership of this important resource. By placing their
most sacred figures of power and authority, the War
Gods—personages both feared and revered—at this
location, ultimate control and authority over the
resources was assured. I suspect the ‘free to all’
attitude arose when the Zunis were subject to hostile
nomadic pressures they could not effectively control.
Most recently (1973), Zuni secured the lease for Zuni
Salt Lake, once again bringing the resource under its
jurisdiction (Frisbie in press).”

The reference to “hostile nomadic pressures” in the
above quote refers to the Athabaskan speaking
Navajo and Apache who are considered to be the
traditional enemies of the Pueblan peoples, and vice
versa. Even the tribal terms used for each other not
infrequently equate with “enemy” or some other
aspect of raiding and warfare. In this instance
anthropologists have been somewhat led astray with the
negativism verbalized and the periods about which
we have considerable data—particularly the 19th cen-
tury when hostile action by the Navajo led to their
incarceration at Fort Sumner. The literature, however,
is replete with examples of positive interaction as well
as negative ones. Stereotypically, the characterization
which most frequently appears in the literature
perpetuates Native American verbalizations without
taking account of readily available sources with clear
indications to the contrary, for example Parsons
and numerous others. It has now been suggested by
Wilcox (1981:214) that the Puebloan people accepted
the Athabaskans “as hinterland residents . . . because
they helped to counteract the economic decline of the
large sedentary centers.” It was not until considerably
later that problems arose following the introduction of
the horse by the Spanish; however, as indicated
above, interaction of a friendly nature also continued.

There remain a few other areas of interest which,
like the preceding, are frequently misrepresented or
misunderstood in the work of current researchers.
Part of the problem rests in the fact that there are few archaeologists who do ethnology; rather, they frequently rely on the classic descriptions of the ethnographic present. The danger resulting from such interpretations is that they do not take account of more recent literature in which misnomers have been corrected. Even though there is nothing new about inter-pueblo warfare during the early historic period, with the destruction of Awatovi (Brew 1979:522), the Hopi-Zuni Wars (Crampton 1977:48) and the emphasis on War Societies (Ellis 1951) as examples, it is not uncommon to hear about the peace-loving nature of the Pueblos. Within the Pueblos, too, the harmonious nature of life— a la Benedict (1934) — continues to creep into the literature in spite of the numerous papers citing factional disputes at Taos (Fenton 1957 and Siegel 1952), Santa Clara (Dozier 1966), Isleta (French 1948), San Ildefonso (Whitman 1940), Oraibi (Titiev 1944 and others), Zuni (Adair and Vogt 1949; Eggan and Pandey 1979:478-479), and elsewhere. Additionally, many tales/legends/myths cite problems for earlier times akin to factionalism. The important point is what we might be able to learn about such activities from the archaeological record. Such problems have led to the social disintegration of pueblos (or aspects thereof) as well as to the actual destruction of sites, as at Awatovi. At San Ildefonso the economic success of ceramic production caused factionalism to occur. The north and south groups split apart in 1930, and as a result the Black-on-black pottery makers headed by Maria and her family were part of the north group. The consequences are intriguing for archaeological inquiry! Certainly, prior to the reservation system the easiest solution would have been for one of the factions to move elsewhere.

We currently seek ecological cause and effect relationships; these are undoubtedly important, but they are not the only type of stress. I believe we should be more cognizant of social factors which may or may not relate directly to the environment, and utilize a data base drawn from ethnohistory as well as from the modern pueblos. We know, for example, the outcome of a devastating flood at Santo Domingo wherein rebuilding in 1880 was a large scale (pueblo-wide) operation headed by the war captain (Ellis 1981:413). Such cooperative ventures are instructive and show the effect of stressful situations on the social system. One might expect the building of the initial village (at any site involving a number of families) to have been similarly handled. Movement to new locations, predicated on a number of factors, was not infrequent during the prehistoric as well as ethno-historic period; see for example Ferguson's (1981:337-339) excellent summation of Zuni. Researchers have suggested that these movements relate to probable environmental stress, as expectable of good archaeologists using the recent tools of our trade. But what of the social factors? Have these been sought? Fractionalism, as noted above, is also a possibility, if it could be archaeologically identified through material remains.

Another possibility, radically different but possibly analogous, relates to something I was told by chance in the early 1960's by a San Felipe Pueblo late-middle aged male. Two children (boy and girl) were herding sheep near the pueblo when a thunderstorm developed. They took shelter under a tree which was struck by lightning and they were both killed. His comment was, "If this had happened in the old days we would have moved the pueblo."

In another instance, Zia residents have verbalized to outsiders the reason for their move from the old village site to the present one as a result of problems with water; however, the truth of the matter relates to the fact that the cacique was bitten by a rattlesnake.

The identification of social factors or features within the archaeological record has not been neglected, particularly since the development of the "new archaeology." Analyses of ceramics using multivariate techniques with the assistance of computers or otherwise have been presented. These normally are predicated on the western pueblo model of matrilineal/matrilocal residence patterns with the clan structure (see Longacre, 1973:202-204 and Martin and Plog, 1973:268-269 for summaries). Although highly innovative, such studies have had their detractors. Unquestionably, discretion is urged regarding to residence patterns when coupled with all of the factors within households that can cause a myriad of variation. The "ideal" operation of the system may appear quite different from the "real" situation. As Kroeber (1917:92) noted: "I should estimate that from five to ten per cent of Zuni women always flew in the face of propriety to live with their husbands rather than lose them." Additionally, there continue to be at least a few endogamous marriages within clans at Zuni (Leighton and Adair 1966:39), as well as at Hopi (Luckert 1981). Marriage, as is expected, continues to be controlled through clan exogamy with strong preference for marriages within villages. As in the past, there continue to be a few marriages to "outsiders" with perhaps a slight increase in the recent period.

With respect to ceramics and other aspects of material culture, the true hope rests with the ethnarchaeologists and those who carefully glean the literature for perceptions which help clarify the archaeological record. While studies indicate that there may have been ceramic "industries" operant within the Puebloan southwest at various locations in the past (Warren 1969 and elsewhere; Carlson 1970), there would not appear to be modern analogous situations. This, however, may be debated if one considers a potter's output to meet the demand of the tourist market. For example, how many pieces were produced by Maria or Nampeyo and their families during their productive years. At Zuni, I have witnessed traditional firings wherein over thirty pieces were fired at once. The vessels, with considerable range in size and shape, represented a few weeks' work. One might ask how many potters would be required to produce
The Fallacy of Analogy . . .

Assuming that ceramic specialization has been adequately demonstrated archaeologically, is it possible to document other similar situations? Specialization in turquoise and other items has been suggested for the Chaco Phenomenon, but I am unaware of other examples. Throughout the literature actual distribution of goods consistently indicates trade or barter, with frequent mention of "trading partner" arrangements, for the pueblos. Such arrangements existed between Pueblo peoples and other groups such as the Navajos and Apaches. Stevenson's (1904:378-379) inclusion of "auctioneering" at Zuni in 1896 provides the only possible alternative. She states that this system of trading is said by the Zuni to have existed long before the invasion of the Spaniards and is practiced in all the pueblos." It was an all-day affair, conducted by an old man in the Great Plaza as needed. Foodstuffs were the primary items offered, with the auctioneer specifying their good qualities and what was hoped for in exchange. I believe this system, as well as periodic markets associated with fiesta-like ceremonial occasions at specific times and places, were, in fact, operant in the past. It is possible to find a number of individuals with pickups (or station wagons) with pottery, jewelry and other items for sale or trade at Saint's Day fiestas (cf. Kluckhohn and Wyman 1940:16). Although we prefer not to refer to such activities as "markets," I do not believe these activities have been viewed in proper perspective. Rather than a simplistic analogous interpretation, the extent and frequency of such activities which do not entail "trading partners" appear to be somewhat akin to markets, at least in incipient form. If one were to gather the variously scattered vehicles into one location, I suspect more attention would be paid than has been the case to date. Thus, since early recorders were not often concerned with issues of interest to some of us today, there are many areas of culture about which we have meager knowledge, if any at all. And of more recent times, again, much of the information useful to our inquiries remains to be collected.

Within the southwestern United States the Anasazi or Pueblo peoples have without question been the most thoroughly studied of all the native populations. An incredible literature exists, one which has grown to astronomical proportions, and yet there remains a vast amount to be done. Although it is not possible for us to ever reach the proverbial "sucked orange" stage which Kidder was warned against prior to his entering the field, we have ascended to new plateaus of understanding. This understanding has come about through new methods and techniques for handling data. Surprisingly, one of the most useful areas, analogy, has been more taken-for-granted than properly utilized. I do not mean to imply analogy has been neglected; our inferences are soundly grounded in ethnographic fact and our material remains can generally be identified with precision through analogy—down to such items as tchamahias, kiva bells, and a host of others. However, with few exceptions (pottery) we lack sufficient detail to discuss full cultural integration of these artifacts in processual terms. This is a requirement of ethnoarchaeology and behavioral archaeology (Schiffer 1976) for which very little has been accomplished to date. Archaeologists have discovered that the types of data required for their inquiries are generally not available; they must gather these themselves. Parenthetically, unlike the Navajo (Kluckhohn, Hill and Kluckhohn 1971) and numerous other groups, we even lack a comprehensive study of material culture for the pueblos.

The purpose of this paper has not been to discuss the potential which analogy holds for future interpretations; rather it has been to show that many of our perceptions based on analogies with the living Pueblos are incongruent or inconsistent with the archaeological record. One must understand the positive as well as negative aspects before proceeding to undo that which has been done. Logico-deductive reasoning may not appear just and sound otherwise. Certainly there are those who cannot or will not alter one iota of their current thought processes regardless of the data presented. To me, our greatest conceptual difficulty continues to be the understanding of Mesoamerican-Southwestern relationships, the specific mechanisms involved therein, and coupled with this, the rise of the Chaco Phenomenon above an egalitarian socio-cultural level of integration. To infer that the Pueblos have remained static in essential form and structure since adopting large communal dwellings in the late tenth and eleventh centuries is no longer tenable, in my opinion. Until we can squarely face what transpired at Chaco Canyon and its satellites, we shall have little chance to pursue the dynamics manifest in the archaeological, ethnographic and recent periods. Although my tentative reconstruction of the ethnographic period, with primary emphasis on Zuni, may seem untenable to some, I believe it will stand the test of time and be congruent with new data as they are presented.
Questions and Comments

Question: How does witchcraft tie into the subject of this paper?

Ted Frisbie: Witchcraft today accounts for a lot of things, e.g. why people cannot accumulate wealth. In the past this had no bearing; then it was perfectly o.k. to acquire wealth. Traders' homes today are filled with wealth but it isn't considered "their" wealth. It is transitory, they own it but it is to move. In the past, traders dealing primarily in luxury goods held the stuff. It was a form of wealth and it was the kind of stuff that was within the religious structure—luxury exotic items. Very different from today.

Question: Are you saying that if they made the wealth items themselves it wasn't being acquired by material means?

Frisbie: No. They didn't necessarily make it themselves. They could have. Whatever you trade to get it—you could be trading foodstuffs or anything else you might make—to have turquoise and shell is considered to be like a gift of the gods, if you can get it and hold onto it. It is something that is very much shared; it is loaned to whomever needs it, for example to Katchina dancers, they get loaded down with the stuff. It is something you can show, display, without having to reflect back on any possible causes of witchcraft. In the past, with witchcraft, it was very likely that if you had someone who did not do what he was supposed to do, if he didn't make it rain when he was supposed to, he could have been accused of witchcraft. It gets into the factional thing. You could kill him or you could simply split—a factional split. Probably many times in the past there were villages that did this because things were not going quite right. We know now that for some Zuni villages there were very short occupations. This could be environmental stress, but it could be that other things were involved. Environment and stress go together. So does the accusation that you aren't doing your religion right; therefore you are no good, let's get rid of you or let's split up and go elsewhere.
This paper asks questions and proposes some themes in the history of Southwestern archaeology, but does not provide any answers. I would like to examine some of our analytic concepts; in particular, our conceptual organization of prehistoric cultural variability through space, an endeavor sometimes referred to as “spatial systematics.” This expression was used recently by Breternitz and Kane (1979) in Report No. 1 from the Dolores Project.

It is dangerous to discuss taxonomy. One hazard is to lapse into pedantry, to present a discourse on semantic details. An alternative danger is to disturb our colleagues by questioning their categories and calling to account the concepts which underlie the organization of data into manageable units.

I will probably fall into both traps, but trust that this symposium is an appropriate situation to raise questions in light of an introspective look at the history of our profession.

The archaeological record is so complex that it is necessary to use categories to describe cultural variation through time and across space. We are aware of the long-standing debate on whether types (or other archaeological categories) are discovered or invented by the archaeologist. Whether we choose the path of computer-assisted schizophrenia or the dogmatist approach that categories are real because we say so, we can probably agree that categories can be useful. Abstractions with given names can help us to think and communicate with one another.

An eloquent discussion of “The Use and Abuse of Taxonomy” was provided by J.O. Brew in his (1946) monograph on the Archaeology of Alkali Ridge, Southeastern Utah, with a Review of the Prehistory of the Mesa Verde Division of the San Juan and Some Observations on Archaeological Systematics. Brew (1946:46) argues that:

... classificatory systems are merely tools, tools of analysis, just as shovels, trowels, and whisk brooms are tools of excavation.

We would not insist that a shovel is a better tool than a broom; they are used for different purposes. Tools are task-specific. They should be traded freely as the job changes.

Incidentally, Brew did not approve of the word “Anasazi” to denote the cultural continuum of Basket Maker through Pueblo. He questions the use of a Navajo word to describe Pueblo people to an English-speaking audience. Brew’s objections are stated in no uncertain terms (1946.ix):

I should like to raise the question as to whether we are really in need of a new word. The modern phase of the culture we are studying is known anthropologically as Pueblo; the people appear in the census lists as Pueblo; they speak of themselves as Pueblo; they are known to their neighbors as Pueblo; and they will continue to be Pueblo whatever we, as a very small archaeological minority, choose to do about it.”

“Anasazi” was not a new word when Brew wrote in the 1940’s; on the contrary, it had been presented to the profession and the lay public as early as 1893,
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when Richard Wetherill described his collections at the Chicago Fair (McNitt 1966). But the word is noticeably absent from the earlier syntheses of Southwestern prehistory. It does not appear in Kidder’s (1924) *Introduction to the Study of Southwestern Archaeology*; it is not used in the published account of the first Pecos Conference (Kidder 1927); nor does “Anasazi” appear in Robert’s initial revision of the Pecos classification, published in 1935 in *American Anthropologist*.

“Anasazi” was introduced to the literature in Robert’s 1937 *American Antiquity* article (1937:5):

>The Anasazi, better known by the names of the subdivisions Basket Maker and Pueblo, is considered a cultural unit with several stages in its development. Its remains are found in the regions of the San Juan River, the Rio Grande, Upper Gila and Salt rivers, the Little Colorado River, most of Utah and a portion of eastern Nevada.

This is a huge area, encompassing all of the Colorado plateau in the Southwest. But even before Roberts reintroduced the word “Anasazi,” the “desert domain” south of the Mogollon Rim was defined as the realm of the Hohokam. The Mogollon cultural pattern, characteristic of the Mogollon Mountains, was defined in the same article (Roberts 1937) that defined the Anasazi cultural unit.

The triad of major Southwestern traditions—Anasazi, Hohokam, Mogollon—is reaffirmed in many textbooks. Let us assume that all is well with the Hohokam and the Mogollon. Researchers have continued to chip away at the territory of the Anasazi.

Thirty years ago, Erik Reed (1950) defined a set of “Western Pueblo” culture traits which may be derived from the Mogollon tradition, but which persist into historic times among the ethnographic western Pueblos. These “Western” traits include red or orange or yellow pottery, 3/4–grooved stone axes, rectangular ceremonial rooms, vertical occipital cranial deformation, and extended inhumation. Carlson (1970) and others working in the upper Little Colorado drainage have continued to define a regional pattern quite different from the Anasazi of the San Juan.

Wendorf and Reed (1955) have developed a series of periods or phases which are more appropriate to the Rio Grande situation than the Roberts classification. Most notable is the substitution of the word “Classic” for “Regressive” to denote the late P IV interval. (Roberts had considered inserting a “Renaissance” subperiod, but rejected the idea.) From the perspective of Santa Fe, Pueblo life flourished after A.D. 1300, and did not decline until several centuries thereafter.

If we remove all of the exceptions from the vast Basket Maker–Pueblo Area as defined by Roberts (and repeated by Wormington in her 1947 definition of the Anasazi area), we are left with the San Juan drainage. The Anasazi area equals the San Juan River system. Neatly, we have also thereby removed Pueblo IV and V from the Pecos system as it applies to the Anasazi, since, by definition, the San Juan was abandoned by the onset of what Roberts called the “Regressive Stage.” If we take a very strict view of spatial systematics, there can be no such thing as Regressive or Historic Pueblo in the Anasazi cultural tradition, since the Anasazi region was abandoned.

Surely, those who formulated the Pecos classification were aware of this conceptual dilemma—Kidder, after all, worked at a Renaissance pueblo on the edge of the Plains. But in 1927, in an astonishing display of consensus, Southwestern scholars chose to emphasize cultural continuity rather than regional diversity.

Since the turn of this century, the “district” concept has been employed to deal with cultural variability through space. A district is smaller than a region, but larger than a site.

As a unit of spatial taxonomy, the district concept has been used as an important basic unit by some authors, but the word is curiously absent from the writings of others. Kidder uses a series of districts—La Plata–Animas, Mesa Verde–Mancos, Montezuma Valley–McElmo, Montezuma Creek–Grand Gulch, Kayenta, Canyon de Chelley–Chinlee, Chaco, and Northern Peripheral—to organize his bibliography for the San Juan chapter of his (1924) *Introduction*, but the word is not used to organize the text. It appears rarely, if at all, in the 1940’s syntheses authored by McGregor (1941) and Wormington (1947). Brew (1946) does not define districts in his Alkali Ridge monograph.

Recently, some archaeology students and cultural resource management professionals have revived the concept. Gillespie (1976) presents a map of San Juan Anasazi districts in his thesis, *Culture Change at the Ute Canyon Site: A Study of the Pithouse–Kiva Transition in the Mesa Verde Region*. This arrangement has been cited in a mitigation plan for proposed developments in the Animas and La Plata drainages (Nickens 1978) and also is cited in a survey report for a major pipeline (Woodward–Clyde 1980). A similar map and a set of districts is presented in the Dolores Project research design (Breternitz and Kane 1979).

How did the word come into usage in the archaeological literature? Why “district” and not “precinct” or “province”? Why did “district” decline from favor, to reappear in the mid-1970’s?

The earliest formalized Southwestern district list of which I am aware is found in Edgar Hewett’s (1908) doctoral dissertation, published in French. The title is *Les Communautes Anciennes dans le Desert American, Recherches Archeologiques sur la distribution et l’organisation sociale des anciennes populations au sud ouest des Etats-Unis et au nord du Mexique*; that is, *Ancient Communities in the Desert American, Research Archaeological on the Distribution and the Social Organization of the Ancient Populations in the Southwestern and Northern Mexico*. It is not in the 1900’s or 1910’s texts, nor is it in the 1920’s articles. It appears rarely in the 1930’s literature, but more often in the 1940’s and 1950’s.
American Desert, Archaeological Research on the Distribution and Social Organization of Ancient Populations in the Southwest United States and Northern Mexico.

In content, the title is somewhat reminiscent of several anthropology theses from the University of Colorado in the 1970's. Hewett earned a degree in sociology from the University of Geneva—hence his focus on "ancient communities" and "social organization." This monograph is not, and most likely never was, readily available to an English-speaking audience, but a typewritten English manuscript version is filed in the Laboratory of Anthropology library in Santa Fe.

Hewett was a teacher, a trainer of archaeologists, and unless we can find evidence to the contrary, I suspect that the word "district" entered our archaeological oral tradition through his influence.

Hewett's spatial systematics are truly remarkable. His map (Figure 1) shows 5 regions—Rio Grande, San Juan, Colorado Chiquito, Gila, and Chichuahua. It is striking that Hewett separated the "Colorado Chiquito" and the Rio Grande regions from the San Juan region, just as Reed and Wendorf would withdraw these drainages from the Anasazi region some fifty years later.

Each of Hewett's regions is subdivided into from 6 to 14 districts, giving a total of 39 districts for the American Southwest and Chihuahua. The districts are drainage (or tributary) specific, and the text emphasizes the apparently impassable desert between the tributaries. Districts are defined as self-contained communities, or societies. Hewett emphasizes the boundaries between districts and sees little or no integration between them. This view predates the Pecos Conference; it reflects a splitter's outlook.

Edgar Lee Hewett is an under-appreciated figure in American archaeology. He is often dismissed as a mere teacher, a trainer of distinguished students, but not someone with significant publications or original thoughts. Willey and Sabloff (1974:60) do credit him with a "precocious" interest in chronology, but this adjective is used in contrast to the "pioneer" designation afforded the next generation of scholars.

An anecdote (Hewett 1943) supports the contention that a pioneering spirit was required to survive Hewett's field school. After a single day of supervision, he simply left three very green young archaeologists—Kidder was one of them—with the assignment of surveying the McElmo country. He told them he would be back in six weeks to collect their report! Hewett believed in training his students the hard way, on the ground.

Hewett is sometimes remembered as the first director of the School of American Archaeology, now the School of American Research. In this capacity, he assumed a key role in the popularization of archaeology, as one who mobilized public sentiment and private financial support for archaeological research and antiquities preservation. He was successful in raising funds to purchase major sites. Pecos, for example, was purchased by the School of American Archaeology and was later donated to the Federal government for protection and curation.

Antiquities management—or cultural resource management (CRM)—is not a new profession. Hewett authored the 1906 Antiquities Act. The 1904 Smithsonian Report includes a chapter entitled "A General View of the Archaeology of the Pueblo Region," by E.L. Hewett. As anthropology, this piece is somewhat disappointing, but as a cultural resource management document, it has a very contemporary tone. Included is an index of known sites, listed by drainage; the word "district" is used casually, but is not defined here. Hewett includes ownership status in his list of sites. Specific in his planning, he indicates the official who was responsible for management of the listed ruins. He has eight ownership/management categories. These are (1) sites on national reservations or parks—there was only one at this time, Casa Grande, but he states that the Secretary of the Interior is responsible for that ruin; (2) those on Forest reserves, and the head of the Bureau of Forestry under the Secretary of Agriculture is responsible for those ruins; (3) those on Indian reservations, the Commissioner of Indian Affairs under the Secretary of the Interior; (4) those on military reservations, the Secretary of War; (5) those on unappropriated public lands, the Commissioner of the General Land Office under the Secretary of the Interior; (6) those on lands withdrawn from entry for special purposes; (7) those on State lands; and (8) those on private lands.

Hewett trained not only professional archaeologists as archaeologists, but also what we might call "paraprofessionals." He advocated that forest rangers learn the basics of recognizing antiquities so that they could pursue their responsibilities. He also suggested, although this never came to light until the Moss-Bennett bill a few years ago, that as the government parted with public lands archaeological surveys should be done.

The archaeology of the turn of the century is highly unappreciated. I have come to resent our high-handed designation of this period as the "museum stage" during which people went out and merely ripped off the biggest and the best. Yes, Hewett supported museums. In the Antiquities Act of 1906, he states rather strongly that research on publicly managed sites should be pursued by suitable universities and museums, and that the recovered antiquities should be retained in public depositories for the benefit of the public.

It strikes me as odd that our spatial systematics have almost gone full circle. From Hewett's early discussion of districts, we went to a rather general designation of three big branches, and over the years we've found problems with generalities and have gone back to specifics. Why are we doing this? I don't know, but I suspect that, as J.O. Brew suggested, we
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choose our taxonomies for the jobs we want to do. Perhaps we should think a bit more clearly about our goals for doing archaeological research, whether they are scientific or CRM, or some combination thereof. I would also second Dr. Wheat’s comments this morning that perhaps we ought to look beyond specific locations and think again about generalities, even though the 1927 generalities may no longer work.
Questions and Comments

**Curt Schaafsma:** Dittert, Hester and Eddy, in their survey of the Navajo Reservoir, discussed what the term, district, meant—a combination of cultural reality and archeological accident, an area of cultural commonality as well as a series of perceptions of a series of archeologists. It is a hybrid term, as I understood it and I recently used it myself when I talked about San Mateo and Grants. There was no implication that this was analogous to a phase, which refers to the subject matter of archeology, but it was a hybrid thing that recognized both an area of cultural commonality and a place where people have been doing a lot of work and therefore we know something about that region.

**Susan Collins:** Thank you for bringing that up, and I’m sorry I overlooked it. Of course that concept was important; and, as you have stated, even there, there is a bit of fuzziness as to whether we are talking about a place or a project or a culture.

**Joe Ben Wheat:** One of the problems we do face, not just in Southwest archeology but in archeology in general, is some kind of a suitable model of the habitation areas—is it a valley, or is it a district, or what is it? And how do cultural ideas, cultural things, move through these various districts or areas or villages, how do they get where they get, how are they organized? I was struck several years ago, while looking at Julian Steward’s book on Africa, by the organization of the districts of Africa and of the various tribes, some of whom are closely related linguistically and some of whom are not. His map, which is one of the most detailed maps I’ve ever seen and one of the most difficult to use I’ve ever seen, nevertheless shows how the villages over a very wide part of the continent are organized and the relationships between them. In that particular case they are organized into groups. I think it might repay someone who is trying to develop a model for our own archeology in the Southwest to study that particular set of maps and the implications of the village structure and the regional organization.
THE EVOLUTION OF REGIONAL DIVERSITY
IN THE PREHISTORIC SOUTHWEST:
ANASAZI PUEBLO AND THE HOHOKAM

David E. Doyel

Introduction

We are gathered here in a timely and organized fashion to continue the process of analysis and to ask certain basic questions about the prehistory of the northern portion of the American Southwest. Who were the Anasazi? Who, when, and where did they develop? What environmental and historical factors shaped the evolution of Anasazi lifeway patterns? Were there external forces that influenced the direction of Anasazi cultural development? And so on.

If this paper has anything to contribute to a symposium focusing specifically upon the Anasazi, it is in the realm of promoting and encouraging the use of a broadly based comparative approach to the study of Anasazi prehistory. I intend to do this by discussing three general topics that must be components of a synthetic framework if we are to produce meaningful comparative statements on the evolution of prehistoric Southwestern societies. These three topics include the identification of historical contacts and interaction spheres, the elucidation of local adaptive patterns and the study of regional diversity, and the need for a generalizing approach within the framework of controlled comparison. A specific example will be discussed that attempts, on a general level, to identify several key characteristics that influenced the evolution of the Chaco Anasazi and the Hohokam cultural traditions, as seen within a comparative framework.

Historical Contacts, Interaction Spheres, and Regional Systems

Forty-five years ago, Alfred V. Kidder (1937:vii) commented that the San Juan Basin hearth-area concept of the origins of Anasazi culture “was fostering a squirrel cage type of investigation only terminated by Gladwin’s bold reinterpretations of Gila Basin archaeology.” More directly, Gladwin stated that the “tide of civilization” in the prehistoric Southwest flowed from south to north, from a hearth area in the Arizona desert. Except from the contemporary “everything came from Mesoamerica” proponents, one rarely hears statements like these today. All of us are too busy studying our local systems and solving methodological problems, but the fact remains that many basic questions were never answered satisfactorily. That the elementary economic basis of Southwestern society, including foodstuffs, pottery
making, and the like, came ultimately from Mesoamerica is not disputed today (Woodbury and Zubrow 1979). These and other elements, which combined to create a sedentary agricultural lifeway, are usually seen as an overlay on top of a late Archaic base already tending towards formative-level patterns, with the possible exception of the Pioneer Period Hohokam (Haury 1976). Today, some archaeologists argue that a fully sedentary, agricultural, pottery-producing society flourished in the Arizona desert several centuries prior to such developments on the Colorado Plateau, an argument not unworthy of consideration when tracing the development of Pueblo Anasazi culture.

Within the general Southwestern province, evidence of processes other than the diffusion of basic elements is clearly visible throughout the prehistoric ceramic horizon. The presence of similar ceramic design styles from northern Mexico to southern Utah on the early ceramic horizon still begs explanation and may indicate historical contacts on a broad scale (Schroeder 1963, 1981). The Hohokam are considered to have been active traders (Schroeder 1966; Doyel 1979), and certainly some of the marine shell that ended up on the Colorado Plateau came through the Hohokam province. It seems highly likely that cotton was introduced onto the Plateau from the Hohokam. Some western Anasazi pottery types (i.e., Walnut and Dogoszhi black-on-white) may even reflect design styles reminiscent of Hohokam textile designs.

The list of parallels and trait similarities evidencing contact could be expanded ad nauseam, and Al Schroeder could stand up and talk all day about it, given the chance. We can also assume that different elements—shell, cotton, art forms, design styles, etc.—arrived on the Plateau through various mechanisms (Pilles 1979). Shell and other products could have arrived through down-the-line trade, and the evidence does show some selectivity on the part of the Anasazi. Cotton production, ceramic design styles, and the similarity with Hohokam textiles may argue for closer contacts. Other traits, such as the ball court-cremation associations found at the Stove Canyon Site, located near Point of Pines along the southern edge of the Plateau province (Neely 1974), could have been introduced as functional complexes from outside sources. Still other items, such as the parrots of Mesoamerican origin found in Chaco Canyon, may have been imported directly by individuals who knew how to keep these birds alive during long trips.

I am not advocating a diffusionist perspective for the origins of Anasazi culture. We must recognize, however, that these historical connections must be considered in our reconstructions, and that Anasazi connections with the south had considerable time depth. The basic point here is that we can no longer permit ourselves to perceive the Anasazi, or any other cultural province, as a closed system.

Interaction spheres and the development of regional systems are best documented for the later phases postdating A.D. 1000. We should be able, on some level, to distinguish among sporadic or transient contact, continued or persistent interaction, and systemic integration, somewhat paralleling the concepts of historical contacts, interaction spheres, and regional systems. Fred Plog (1979:127) has suggested that regional styles reflect symbols of unity that bound together people with different subsistence strategies. While this is an interesting theoretical position that has not been defined precisely, other equally interesting positions can be developed. Regional styles zones could reflect, through time, the shifting boundaries of interaction spheres or alliance systems, formed for various socio-economic and political reasons; the Chacoan late Bonito to Mesa Verde transition in the San Juan Basin is one example.

Highly integrated, truly regional systems appear to have a limited distribution in the Southwest, with the central or core-area Hohokam and the Chaco Anasazi representing the two most illustrative examples (Doyel 1979; Judge 1979; Marshall and Doyel 1981). There undoubtedly existed numerous levels of interaction and exchange within and between such regional systems. Perhaps there were different spheres of exchange, as defined by Fried (1967), wherein elite group-to-elite group exchanges occurred and finally filtered downward through local networks. The point to be made here is that variation in complexity appears to have characterized much of the Southwest at any point in time, and that all of these levels of interaction should be considered within a synthetic framework and addressed from the perspective of their prior probabilities of having existed.

Adaptive Patterns and Regional Diversity

Fred Plog (1979) has identified 12 variations of western Anasazi culture, although some would argue against including the Mesa Verde and Zuni areas within the western Anasazi province. A number of variations can also be identified among the eastern Anasazi, including the Chaco, Rio Grande, Largo-Gallina, and perhaps other expressions (Cordell 1979). Likewise, at least six regional variations have been recognized for the Mogollon. This sorting-out process has barely begun, however, within the Hohokam province (Doyel 1979; Doyel and Plog 1979). One recent trend in research has been the identification and explanation of this generally recognized diversity in lifeway patterns and material expressions (Cordell and Plog 1979), although the typologies used today still emphasize material culture expressions as opposed to socio-cultural attributes.

Diversity exists within a variety of material products, architectural and settlement patterns, and subsistence systems. It has been argued that during most of the sequence the western Anasazi lived in homestead-sized settlements and did not reflect the
strong trend towards nucleated settlement systems discernable in the eastern Anasazi province from late Basketmaker times through the Pueblo periods (Gumerman 1972; Plog 1979)

At this point, we should ask how much of this diversity is attributable to ecological factors, in light of the environmental mosaic found throughout the Colorado Plateau. Plog (1977;111) has suggested that agriculture was “riskier” among the western Anasazi because of lower and less predictable precipitation. Regardless, recent research certainly has begun to identify considerable diversity in farming strategies within the Anasazi area. Sand-dune farming systems (Doyel 1982), dry farming (Judge and others 1981), gridded gardens and diversion systems (Vivian 1974), and terraces and canals (Plog and Garrett 1974) have all been identified through field research. Undoubtedly, the inhabitants were aware of, adapted to, and exploited this diversity. Such adaptations to local diversity must have had concomitant effects on settlement patterns and socio-economic organization, as well as implications for carrying capacity and demographic profiles.

The shift to a primary dependence upon agriculture also appears to have occurred at different times throughout the Southwestern area. It is likely that both the Hohokam and Mogollon had made this transition several centuries prior to a similar shift on the Plateau. Furthermore, the shift to an agriculturally based economy may have been effected earlier among the eastern Anasazi, although more research is certainly needed here. Plog (1979) has suggested that agriculture may not have been emphasized among some of the western Anasazi until after A.D. 900. Contrary to most other opinions, Plog (1980) has also suggested that agriculture may not have been relied upon in the Hohokam area until quite late; there seems, however, to be little support for this position (Haury 1976).

As archaeologists interested in understanding the meaning of this diversity in evolutionary terms, it is incumbent upon us to loosen up our typically provincial perspective and to employ a worldwide perspective and data base when developing testable models to account for the transition from hunting and gathering to food production. Ethnographic research has already demonstrated some close connections between subsistence systems and social-group formation. Netting (1965, 1968) has shown a close statistical correlation between family/work group size and the type of agriculture practiced among subsistence agriculturalists in Africa. Sahlins (1957) has also shown how family/work group organization in Polynesia is structured by the distribution of natural resources. Testable models certainly can be developed to account for the regional diversity characteristic of the prehistoric Southwest. Can we now identify different forms of social and economic organization that appear to have evolved as a result of different adaptive pressures?

Chaco Anasazi and Hohokam Adaptation: A General Comparison

Certainly on a broad level of contrast, we can begin to perceive different adaptive pressures present among the major Southwestern traditions. Introductory Southwestern archaeology teaches us that the Anasazi, the Mogollon, and the Hohokam were adapted to the Colorado Plateau, the forested mountain zone, and the desert areas, respectively. On a more specific level of comparison, however, we can also identify environmental factors that could have strongly influenced the direction of evolution and adaptation. As a specific example, I will present a first-approximation comparison of the structure of the Chaco Anasazi and the Hohokam regional systems and will discuss some similarities and differences in the nature of these systems.

Without dwelling on specifics, we can state that the Chaco system evolved in a high-elevation, semi-arid, steppe-grassland environment where food sources, water, and arable land are unevenly distributed. That this environment was relatively productive vis-a-vis neighboring areas is demonstrated by the existence of settled villages from at least the Basketmaker III horizon up to the Pueblo III Period.

By Bonito Phase times, a large area had been incorporated within a regional system that included numerous large pueblos within Chaco Canyon and some 50 so-called “outliers” located at various distances from the canyon (Marshall and others 1979). While the precise nature of the Chacoan adaptation remains moot (Cordell 1979), we do know that the system has at least several centuries of time depth, with several major phase changes apparent (Judge and others 1981). Two points can, however, be made with some certainty: (1) the settlements incorporated within the system appear to have been established to take every advantage of the micro-environmental diversity present in the region, and (2) these settlements interacted on a regular basis, as is demonstrated by the incredible system of roadways that connect the settlements as well as by the presence of a variety of imported products at many of the sites (Figure 1). Sites are located along the San Juan River, thus affording access to a riverine environment; in the Chuska Basin, where the sites (with the exception of some late ones) usually are located where areas of high agricultural potential might best be utilized; and along the Chuska slope and in other areas, where best advantage might be taken of the forest and other available resources.

I am not the first to state that this system was designed to exploit this micro-environmental diversity; indeed, the system appears to represent an attempt to
organize this diversity on a regional scale (Judge 1979, Powers 1981). For the sake of descriptive comparison, I will refer to the Chaco network as a dispersed system. This system is characterized by pockets of dense population and extensive land use located in environmentally dissimilar situations and separated by large areas of badlands and poor soil zones known as settlement voids (Marshall and Doyel 1981).

This dispersed system stands in marked contrast to the Hohokam example. Generally bounded by the major river systems, the desert area is characterized environmentally by high resource redundancy. The rivers represent ribbons of moisture that have along their courses well-stratified biotic communities, including mesquite bosques, saguaro/paloverde communities, and extensive creosote flats. Major settlements occur along these drainages at almost predictable distances (Wilcox 1979), generally on the first or second terrace above the floodplains. It is well known that the Hohokam had developed a highly sophisticated water control network focusing upon canal irrigation derived from the rivers (Haury 1976). Although it is also known that the Hohokam did divert water, via canals, considerable distances from the river sources (Haury 1976), the location of the water sources clearly had a strong influence on the location of major settlements. Also, the soils located along the floodplains and terraces were suitable for agriculture except in those areas where surface gravels or highly compacted or saline soil zones occurred (Doyel 1977b). Thus, unlike the Chaco system, the Hohokam system included usable water sources, arable soils, and communities of native vegetation in close proximity along the rivers, creating what I will refer to as linear systems (Figure 2).

Even on a general level of analysis, it can be postulated that the dispersed versus the linear nature of these systems could have created very different adaptive pressures and different pathways of societal development. The different structures of these systems would have had implications for such system components as resource procurement, the organization of exchange systems, alliance-formation processes, social organizations, and the expression of power. Divergent strategies for dealing with population-resource imbalances probably existed. It could also be predicted that how the two systems ultimately collapsed would differ.

Procurement systems differed significantly, owing to the differential distribution of resources. Hohokam communities within the central area generally had access to the same range of natural resources as the next community, and so on down the line. Such was not the case in the Chacoan network. This created within the Chacoan system a dependency situation that required more integration of communities in order to ensure an even distribution of resources. Thus, within the Chacoan system we would predict greater emphasis upon alliance and exchange to ensure the survival of the system. Greater emphasis would be placed upon political federations, upon local intensification for the production of surplus commodities for exchange, and upon ceremonialism as a mechanism of social integration. Communities throughout the Chacoan system invested considerable energy in constructing massive ceremonial facilities, perhaps in part to demonstrate publicly their participation in the system as alliance members of equal political status. Roadways often enter these communities near a great kiva or a massive Bonito Phase structure, further suggesting intervillage ceremonial ties.

The Hohokam did possess an elaborate ceremonial tradition; they also must have experienced periods of population-resource imbalance. More options appear to have been available, however, insofar as they could have: 1) expanded the irrigation system, 2) utilized other agricultural techniques (runoff, ak-chin, etc.), 3) emphasized the harvesting of native desert food sources, or 4) traded for resources. That alliance and exchange systems existed among the Hohokam is suggested by the distribution of similar material cultural
paraphernalia (Doyel 1981). It is possible that such alliances were more “artificially” created as social buffering mechanisms (Cordell and Plog 1979:421) and as a means of obtaining exotic raw materials not found in the desert region, such as turquoise, jet, serpentine, shell, copper, etc. (Doyel 1979). The great dependence of the Anasazi upon rainwater created a concern for scheduling and for supplication through ceremonialism, including the use of astronomical concepts and devices. The Hohokam, on the other hand, unlike the Chacoans, did not rely upon the natural distribution of water for farming, but upon irrigation systems, suggesting that leadership roles in communities may have been more politically structured while relying less on a priestly religious class (Haury 1956).

As mentioned, the different nature of these two systems could have become more pronounced at the time of system collapse or reorganization. The Hohokam regional system reflects a stability over time not visible in the more fragile Chaco system, which was dependent upon and existed by the perceived mutual interdependence of the regional subtraditions (Chuska, Cibola, San Juan, etc). If any segment of the Chacoan system was overtaxed or if alliances were broken, the existence of the system was threatened. Communities dependent upon non-local resources, or communities that served as nodes of transportation along roads, could no longer have been inhabited and consequently would have been abandoned. The shift from the Chuska Valley connection to that with Mesa Verde, as reflected by pottery in Chaco Canyon, could have resulted from political fragmentation and a restructuring of the trade and alliance systems. The Hohokam system, on the other hand, survived several centuries after the demise of the Chacoan, but at some time around A.D. 1450 did undergo a reduction in population and complexity.

To summarize briefly, a comparison has been made between the structure of two regional systems in the prehistoric Southwest: the Chaco Anasazi and the Hohokam. These systems share many similarities; both exhibit agricultural economies, site hierarchies, well-developed ceremonialism, high-status burials, and exchange systems. Another possible similarity that should be investigated extensively is the possible emergence of market systems (Doyel 1977a; Judge and others 1981). Such an investigation would enhance our understanding of the nature of exchange and redistribution systems, and presents an alternative and potentially more productive direction than arguing the attributes of typological levels of social organization (Doyel 1980).

While these systems exhibit numerous similarities, it is suggested that the environmental differences between the two areas led to the development of two different kinds of systems, linear and dispersed, and that these environmental differences could account for much of the variation between the Chaco and Hohokam regional systems. Clearly, environment and ecology cannot account for all of the observed variation. The presence of ball courts, platform mounds, and many other attributes among the Hohokam is attributable to contacts with Mesoamerica. The Chaco Anasazi clearly demonstrate, however, the stamp of ancient origins in the Plateau Anasazi province. This is the subject of another paper and will not be addressed here.

**Cultural Complexity and the Relevance of Ethnography**

While on the subject of comparative approaches to societal development, we should address some comment to the relevance of ethnographic data in model building. While there are those who feel comfortable in freely switching from archaeological to ethnographic data and back again (Ellis 1981), it should be pointed out that there were no Chacoan-type regional systems in the Southwest during the protohistoric or
The Evolution of Regional Diversity

the early historic periods. Likewise, there were no Snaketowns being inhabited in the desert at the time of the Spanish entrada. Numerous scholars have pointed to the differences in culture between the prehistoric and historic periods, whether we are talking about the desert Pima (Fonta 1975) or the Pueblo (Wilcox 1981). There certainly is room to question the direct application of ethnographic models, which have been abused in the past (Gumerman 1972:86; Cordell and Plog 1979:422), if only because ethnographic models cannot account for the variation observed in the archaeological record. Even if we could confidently identify the presence of a dual division in Chaco Canyon (Vivian 1970), it still does not explain the structure of Chacoan society or the Chacoan regional system. Other features, such as roads, great kivas, towers, site hierarchies, and the like, still beg explanation. Furthermore, many prehistoric sites are located in environments not occupied during the historic periods, further arguing against direct comparisons.

All I would suggest here is that ethnographic data are useful but can be abused, and we should not expect to explain the evolution of Anasazi lifeways on the basis of our uneven, generally synchronic accounts of historic Pueblo culture. In some ways, it makes more sense to compare the Chaco Anasazi or the Hohokam with formative periods developments in Mexico or the Near East than it does to compare Chaco with Acoma or Zuni. Different data sets are useful for different reasons, however, and there is equally no sense in ignoring either as a potential source of information when used in a framework of multiple or competing hypotheses.

Conclusion

One thrust of this paper is to point out that we, as Southwestern archaeologists, can contribute to evolutionary anthropology. We can gather quality data relevant to the explication and explanation of relationships among environment, subsistence, and society. Thus far, these differences are most readily apparent on a broad level, such as between the major traditions, but our currently intense level of field work, if conducted and synthesized properly, should contribute substantially to more specific as well as to more general theoretical levels of comparison. Twenty-five years ago, E.W. Haury (1956) proposed that archaeologists begin looking for functional correlates among environment, subsistence, and society. He proposed that a functional relationship existed among pueblo kinship systems, a dry-farming economy, and ceremonialism, in contrast to the Hohokam irrigation farmers, whose society was more politically structured. Sometimes I am not sure how much farther we have progressed, and it does seem that we have a way to go. I would agree with Kidder that we need to look beyond our own squirrel cages and, through the use of a broadly based comparative framework, contribute to and remain in the forefront of modern archaeology.

Questions and Comments

Jim Judge: I don’t see the dichotomy between the diversity of the first system and the linear system quite as sharply as Dave does. I think we are looking now within the Chaco system as much at redundancy as we do at diversity. I don’t know whether in a Hohokam site you have an equal amount of diversity as you do redundancy.
ANASAZI ORIGINS: THE ARCHAIC OR BASKETMAKER I

Thursday, October 1, 1981
Afternoon Session I
Chair: Elizabeth A. Morris
The antiquity of the Anasazi was extended back in time with the discovery by McLoyd and Graham of non-Cliff Dweller remains in the Grand Gulch of southeastern Utah (McNitt 1966:64). Subsequent to their excavations in 1890–1891, Richard Wetherill entered the Cottonwood Creek and Grand Gulch areas and reported the recovery of numerous Basketmaker remains (McNitt 1966:64–65). The formal excavations of Kidder and Guernsey (1919) and Guernsey and Kidder (1921) in northern Arizona firmly established the presence of a stage stratigraphically antecedent to the later pueblo ruins of the Anasazi Southwest.

However, the excavations in northern Arizona and southern Utah did not recover evidence of the ancestors of the Basketmakers, leading to the questions of Anasazi origins. A major outcome of the 1927 conference of southwestern archaeologists at Pecos Pueblo was the institution of a classification scheme for the Anasazi occupation of the Southwest. A hypothetical stage, Basketmaker I, was included in the classification for the eventual placement of the ancestors of the Basketmakers (Kidder 1927).

Subsequent discoveries of an extensive and long-lived Archaic Tradition predating the horticultural occupations of the Desert West (Jennings 1974:154–155) and the development of the Cochise Culture in the Southwest prior to 5000 BC (Willey 1966:181) provided a likely candidate for the Basketmaker I stage of the Anasazi. Willey (1966:181–182) has defined the Southwestern Tradition as those sedentary cultures derived from a Cochise base with the introduction of cultigens and ceramics from Mesoamerica. The Anasazi were defined as having developed from an Archaic base with the addition of pitstructures, ceramics, and cultigens from the already developed Mogollon Tradition (Jennings 1974:298). The Mogollon Tradition developed out of the San Pedro Cochise with the introduction of cultigens from the south (Willey 1966:183).

The Basketmaker I stage has therefore been interpreted as representing the transition between the Archaic and the Anasazi. This current model is widely accepted and applied in the interpretation of Anasazi origins. The purpose of this paper is (1) to question the role of the Archaic in the origin of the Anasazi, (2) to propose that the Anasazi Tradition may have resulted from the adaptation of Mogollon homeesteaders to a frontier situation, (3) to state the test implications of a Mogollon model of Anasazi origins, and (4) to indicate those data needed for evaluation of the model.
Discussion

Two major test implications of the current model of Anasazi origins have been derived from the literature.

Proposition 1: If the Anasazi developed out of a local Archaic variation of the Cochise Culture, then sites exhibiting the transition between a hunting and gathering adaptation and the horticulturally based subsistence pattern of the Basketmaker stage would be identifiable in the archaeological record.

Proposition 2: If the Mogollon were responsible for the introduction of the pitstructures, ceramics, and cultigens that allowed for the development of the Anasazi, then sites exhibiting those characteristics should also be present in the archaeological record.

The importance of the investigation of sites associated with the transition period between the Archaic and the Anasazi guided the research of Irwin-Williams in the Arroyo Cuervo Region of New Mexico (Irwin-Williams 1973). In an attempt to document an unbroken sequence of development between the two archaeological manifestations, Irwin-Williams undertook extensive fieldwork, including survey and excavations, to define a regional sequence. The Oshara Tradition was subsequently defined as representing the transition period between the Archaic and the Anasazi (Irwin-Williams 1973:17).

The evidence presented in the argument for an Archaic to Anasazi transition is unconvincing. Careful evaluation of the published materials resulting from this attempt to document Anasazi origins has led Berry (1980:51–54) to criticize the resulting interpretations as being weak. He cites the lack of published supportive data concerning the assertions of the research, and as a result of his careful evaluation of the antiquity of maize in the Southwest he suggests that there is no support for the proposition of an Archaic to Anasazi transition in the Arroyo Cuervo Region.

The introduction of domesticated plants into the Southwest has been variously cited as having great antiquity, based primarily on maize recovered from Bat Cave, dated at 3000 BC (Dick 1965). Concurrent with the proposition of a definable transition period is the assumption that the introduction of maize had little immediate effect because it was only after approximately 3000 years of maize availability that the horticulturally based Basketmaker stage developed (Berry 1980:63–63).

Research cited in this doctoral dissertation has allowed Berry (1980:Chapter II) to estimate that the entry of maize into the Southwest occurred at approximately 185 BC, based on the reanalysis and reinterpretation of the excavation and dating of Bat Cave, Cienega Creek, LoDaisKa, and the Arroyo Cuervo Region maize associations. This recalibration of the antiquity of maize in the Southwest, if accepted, is in direct opposition to statements such as those offered by Peckham (1968:10) that “... corn did not become a dominant food source until almost a thousand years after its introduction.” Further, Berry presents a critical review of those sites which are purported to reflect the transition from the Archaic to the Anasazi, and in doing so casts doubt on the current model of Anasazi origins.

It is suggested that the transition between the Archaic and the Anasazi has not been reasonably documented and that the current model of Anasazi origins has been accepted despite the lack of support for the primary proposition of the model. Further, it is proposed that evidence of Mogollon influence in Basketmaker sites of the Anasazi area of the Southwest can be interpreted from the perspective of a model which derives the Anasazi from Mogollon homesteaders faced with the exigencies of isolated development in a frontier situation.

Evidence of Mogollon influence in sites dating to the Basketmaker stage of Anasazi development is usually based on trait list comparisons between the two cultures. Such trait list comparisons are well beyond the scope of this paper. Rather, the interpretations of Mogollon influence in the Basketmaker stage of the Anasazi will concentrate on pithouse architecture and ceramic artifacts, two of the three influences listed by Jennings as comprising the Mogollon derived impetus for Anasazi development (Jennings 1974:298). Also, evaluation of architectural and ceramic influences in the Anasazi area of the Southwest will serve as an entry point for the construction of the Mogollon model of Anasazi origins, which does not require an Archaic to Anasazi transition to have occurred.

The lack of evidence for the proposition that there are identifiable transition sites between the Archaic and Anasazi occupations of the Southwest allows for the development of an alternative model of Anasazi origins that accounts for the Anasazi as the outgrowth of small-group Mogollon homesteaders exploiting a horticultural frontier. In support of this model is the recognition that the introduction of maize into the Anasazi area has been shown to roughly coincide with the appearance of the Basketmaker stage in the Anasazi area (Berry 1980:64). The ability to recognize Mogollon style pitstructures in the Anasazi area is critical to this model. Berry (1980: Chapter III) has summarized the architectural forms of early Anasazi sites. Willey (1966:190–192) cites regional and temporal variation in Mogollon pitstructure styles and summarizes the early pitstructures of the Mogollon as being circular with short entryways. Joe Ben Wheat (personal communication 1981) has characterized early Mogollon architecture as representing houses in pits with encircling post arrangements. Although variable, it suggests that Mogollon architectural forms should be recognizable in the Anasazi area.
Proposition 1: If homesteading groups from the northern Mogollon area moved into the Anasazi area, it is expected that their house styles would closely correspond with those of their parent area.

Proposition 2: Because the Basketmaker II stage has been defined as aceramic (Kidder 1966:241), if the initial occupants of the Anasazi area were Mogollon immigrants, it would then be expected that those populations moved out of the Mogollon homeland prior to the introduction of ceramics into the Mogollon Tradition.

The dating for the inception of ceramic manufacture in the Mogollon area is variable, with the most recent citation for its arrival being at 300 BC (Martin 1972:3). Berry (1980:82-84) has questioned the radiocarbon dates used in that age ascription, and instead suggests a date ranging from the second half of the first century BC to approximately AD 200. The beginnings of ceramic manufacture in the Mogollon area during the second century AD would more closely ally with the model of Anasazi origins being presented.

The occupation of the Anasazi area by Mogollon homesteading groups is modeled as representing the immigration of aceramic family groups which can be characterized by their use of Mogollon architecture. Unfortunately, few excavation reports are concerned with the evaluation of site architecture in terms of possible Mogollon affiliation. A notable exception is the discussion of architectural correspondences between the Mogollon and Anasazi house types documented by Morris and Burgh in Basketmaker II sites of the Durango area of Colorado (Morris and Burgh 1954: Chapter 9). The concluding paragraph in their discussion of the Durango Basketmaker houses and comparable houses of the Mogollon area deserves to be quoted in its entirety (Morris and Burgh 1954:84):

If plans and descriptions of early Mogollon houses and those of Basketmaker II at Durango were mixed together and submitted, without labeling as to localities of occurrence, to an unbiased analyst, it is to be doubted if he would find enough consistent differences to indicate that all were not the work of one and the same people.

Dating of the Basketmaker II occupation of the Durango sites has been recently reviewed by Dean with the resulting statement that the structural remains cannot be dated to before AD 200 (Dean 1975:31).

Excavations in the Navajo Reservoir District of northern New Mexico documented pitstructures comparable with those of the Durango area. The pitstructures exhibit cribbed log architecture (Eddy 1961:16) and reflect a date range of occupation between AD 170-270 (Berry 1980:109). Excavations at the Tres Casas site (Eddy 1966) documented two additional pitstructures which apparently represent a house-in-pit concept similar to that noted to be associated with the Mogollon. The recovery of intentionally fired brown ware ceramics associated with the early occupation of the Navajo Reservoir District (Eddy 1961:64-65) may represent the earliest documented Mogollon ceramics in the Anasazi area. Because of the inability of the excavator to assign those ceramics to the Mogollon Tradition, the suggestion that they represent Mogollon influence cannot be properly evaluated.

Two pitstructures excavated by Lipe and Matson on Cedar Mesa in southeastern Utah and reported by Berry (1980:124-129) apparently fall within the period AD 250-300 and may indicate a Mogollon derivation. Both pitstructures exhibit slab-lined entryways: the Veres site exhibits a possible four post roof support system and the Pittman site uses an encircling post arrangement (Berry 1980: Figure 4D and 4F).

The occurrence of ceramics in Basketmaker pitstructures of the Anasazi area has been interpreted as denoting the Basketmaker III stage (Willey 1966:202). Although Morris (1927) suggested a model of local ceramic development in the San Juan area of the Anasazi, it is generally accepted that the impetus for ceramic manufacture was derived from the Mogollon (Willey 1966:203). Mogollon Brown Ware ceramics have been recovered in Basketmaker III contexts in both the Eastern Anasazi (Cordell 1979:124) and the Western Anasazi (Plog 1979:116) areas, apparently often in association with indigenous gray ware ceramics which are peculiar to the Anasazi (Cordell 1979:134).

Proposition 3: If a Mogollon ceramic technology was introduced into the Anasazi area by homesteading groups, then a technological shift—from the production of ceramics in an oxidizing atmosphere of firing (brown ware) to one utilizing a reduction or neutral atmosphere of firing (gray or white ware)—should be recognizable in the archaeological record.

A second influx of Mogollon homesteaders, this time with ceramics, is therefore indicated. It is proposed that a technological innovation had to occur in order for the immigrants to effectively utilize the predominately buff-firing clays of the Anasazi area. Refiring analysis of over 2000 sherds recovered from the mitigation activities of the Dolores Archaeological Program, located north of Mesa Verde National Park in Colorado, has demonstrated that the clays used in the production of gray and white ware become soft and weaken considerably when subjected to an oxidizing atmosphere. It is suggested that the clay resources of the Anasazi area required the development of a reduction regime of firing in order to effectively utilize available resources.

Proposition 4: If such a technological shift had to occur in order for the Mogollon potters to produce
usable vessels, then Mogollon homestead sites in the Anasazi frontier should exhibit a mixture of ceramics, those brought with the settlers from the Mogollon homeland and those derived from resources locally available to the immigrants.

Reanalysis of the ceramics associated with such sites would have to occur in order to evaluate the proposition. The Mogollon ceramics recovered from the excavation of Site 5MTUMR2344, located in Mancos Canyon in southwestern Colorado (Hallisy 1974) have been subjected to a preliminary analysis. A total of 154 sherds, representing at least three vessels in pitstructure floor contact, were noted to contain a distinctive sand temper composed primarily of quartz sand in a brown clay matrix. One bowl exhibited a smudged interior with red exterior slip. The Mogollon ceramics were in association with Chapin Gray, thought to be locally manufactured. Although the tree-ring dates for the site were not available to the author for publication with the report, 32 dates from the pitstructure's construction timbers, including four bark dates, suggest that the pitstructure was constructed in the last half of the fifth century AD (W. Robinson, personal communication 1974). It is suggested that the pitstructure, which is shallow with a four post support system including encircling posts and a southern entryway (Hallisy 1974:Figure 3), and its associated dated ceramics may represent Mogollon colonization into the Mesa Verde Region of the Anasazi prior to AD 500.

Proposition 5: If a small group long distance Mogollon immigration occurred, then the Mogollon derived ceramics should have a limited distribution (as opposed to a widespread distribution resulting from exchange) and a lack of temporal duration due to the isolation of the groups from the parent populations.

The current model of Anasazi origins has allowed for the interpretation of Mogollon ceramics in Basketmaker sites as evidence for exchange. Further, the ascription of those ceramics to the general Mogollon area precludes the use of the ceramic data in pinpointing a specific manufacturing locale of derivation. A resource-based ceramic analysis approach developed for the Mesa Verde Region by the Dolores Archaeological Program allows for definition of intraregional ceramic manufacturing tracts (Lucius 1981). Application of such an approach to the analysis of Mogollon ceramics in Basketmaker sites would be necessary in order to define those areas of the Mogollon represented in the Anasazi sites. Also, reevaluation of the distribution and temporal depth of Mogollon ceramics in the Basketmaker sites would need to be accomplished to evaluate the proposition.

Proposition 6: If the homesteading groups were isolated from parent populations, then ceramic and architectural styles would develop independent of developments occurring in the Mogollon area.

This last proposition allows for the development of Anasazi styles which are distinct from those of the Mogollon. Pertinent to the proposition would be the analysis of regional variation in pitstructure and ceramic styles within the Anasazi tradition and comparison of the similarities and differences in those styles with the regional variations of the Mogollon Tradition.

Conclusions

A prime consideration in the question of Anasazi origins is the status of the Archaic populations in the Anasazi Southwest. The current model accounts for their demise by subsuming them into the Anasazi. The Mogollon model assumes that the Archaic was not an integral factor in the development of the Anasazi. Were they absorbed by the incoming horticultural populations, or were there any Archaic populations in the Anasazi area at the time of the horticultural exploitation? It is suggested that in order to properly evaluate the critical question of Anasazi origins it will be necessary to concentrate on the archaeology of the late Archaic of the Anasazi area in addition to that of the early Anasazi.

The Mogollon model has been presented in an attempt to account for the origins of the Anasazi through the use of a series of testable implications. This is in response to the generally uncritical acceptance of the current model of Anasazi origins. The overriding consideration in the production and presentation of this paper has been the stimulation of discussion concerning the problem of Anasazi origins. The Mogollon model has not been implemented; rather it consists of a series of propositions that can be used to guide the course of investigation into the problem. It may be that the Mogollon model cannot be supported by close inspection of the data, but the routines of data collection and analysis required in the formal evaluation of such a model would result in a better understanding of the problem of horticulturalists in the Anasazi Southwest than is now available.
It now seems clear that hunter-gatherers once occupied most of the Plateau region in which the Anasazi agriculturalists later lived. Indications at present are that these Archaic hunter-gatherers were ancestral to the agricultural Anasazi. The change from hunter-gatherers to early agriculturalists took place approximately between A.D. 1 and A.D. 400. The position taken in this paper is that the social organizations and settlement patterns of the late Archaic hunter-gatherers are poorly known yet highly significant for understanding the nature of early Anasazi culture. After a review of some of the ideas that relate to our thinking about the social organizations and settlement patterns of the pre-agricultural inhabitants of the Colorado Plateau and Great Basin, I propose a model based on analogy with the Owens Valley Paiute of eastern California. The main thrust of this model is that it is capable of generating test implications that can be tested with empirical materials.

Archeologists relatively recently became aware of the artifactual remains of hunter-gatherers in the Southwest; some of the first reliable reports were provided by geologists who found these artifacts in geologically datable contexts (Bryan and Toulouse 1943). Very little sustained effort to deal with these archeological remains was made until after World War II. In the years following the war a number of reports appeared that were based on single-site excavations or limited surface collections (Agogino and Hester 1953, 1956; Campbell and Ellis 1952, Dick 1965, Hibben 1951; Jennings 1957; Wendorf and Thomas 1951). The primary concern of archeologists during this period was to define the artifact inventories of sites, obtain dates on materials, and discern the areal distributions of artifact classes (types) by making comparisons between different localities (Dick 1965). Excavations in stratified caves focused on establishing temporal continuities of artifact classes and assemblages of these classes. Irwin-Williams (1967) has synthesized the empirical data that emerged from this period and endeavored to organize this material into coherent time/space patterns based on artifact typologies, traditions, and areal divisions. This remains a basic study.

By the mid 1950's, archeologists began to attempt to understand the nature of the cultures that had produced the artifacts that, it was becoming apparent, had been distributed over a vast area for a long time. The first important effort along these lines was made by Jennings (1957) on the basis of an archeological continuum in Danger Cave near Wendover, Utah. On the assumption that a continuity of archeological materials implied a continuity of the total cultural pattern, Jennings reviewed the ethnographic literature on the people who were living in the vicinity of Danger Cave in the 19th century. He summarized this information and projected “back from the ethnohistoric horizon many of the lifeways of the tribes of the
It is certain, first of all, that the population was sparse. The effective social unit was small. An extended family—man, wife or wives, children and children-in-law, some infants—numbering no more than 25 or 30 in all would constitute a normal, year-round grouping. The pattern of life was a cyclic wandering, but it was not truly a nomadic one. The small groups moved regularly from place to place, from valley to upland, in search of the seasonal animal or plant resources which centuries of experience had taught them were to be had. The wandering was not aimless; it was based on intimate and annually renewed knowledge of a relatively well-defined territory (Jennings 1957:7).

After making a review of similar archeological manifestations throughout the Great Basin and the Southwest, Jennings (1957:280) proposed that all these materials and the cultures that produced them “can be given one general label—the Desert culture—because these manifestations are more like than unlike one another.” He furthermore proposed that his ethnographic model was applicable throughout the area where these manifestations occurred. He was actually proposing, therefore, that all the archeological materials within the range of the “Desert culture” were the products of bands similar to the nineteenth century Shoshoni in the vicinity of Danger Cave.

Jennings’ Desert culture concept and the Shoshoni ethnographic model upon which it is based have been widely accepted as accurately depicting conditions that formerly existed in the Great Basin and the Southwest (Willey 1966:55–60; Martin and Plog 1973:69–80). However, over the years the reference to the maintenance of territories in Jennings’ original model has been gradually lost (Willey 1966:55), until recent discussions have maintained that territories were not claimed and “Chances are that no one group controlled the food resources of a given locality or maintained exclusive rights to them” (Martin and Plog 1973:77). According to this view, “Visits back and forth between various ecological and resource areas would have probably prevented any one group from claiming any ‘territoriality.’ . . . Such a flexible attitude would have permitted individuals to have changed residence location without any heartbreak” (Martin and Plog 1973:77).

One reason for the current view of the “Desert culture” (as exemplified in the above quote) is that Jennings’ model has come to be equated with Steward’s reconstruction of the Great Basin Shoshoni culture: “Although never clearly spelled out, there can be little question that Steward’s ethnographic description and interpretations of the basic Shoshonean life-way provided the basis for both original and subsequent definitions of the Desert culture” (Thomas 1973:155). Jennings would certainly be able to resolve this question, but it appears that he was not relying upon Steward but was making his own generalizations from the same nineteenth century ethnohistoric sources. It is unquestionable that the two ethnographic reconstructions are similar. However, the main point of Steward’s argument has been that erratic and sparse resources in the Great Basin precluded the formation of stable social groupings beyond the family-band, and that none of these social units owned resources or claimed exclusive access to definite territories (Steward 1938:254, 257, 260). Any social groupings larger than the family-band such as the winter villages, were temporary, shifting in composition from year to year and unstructured by rules of post-marital residence: “The only stable social and political unit was the family” (Steward 1938:260).

Steward has recently stated his point of view: “There is no evidence that either bands or competition for territory existed in the Great Basin before the white man arrived (Steward 1968:x). Steward’s Shoshoni model, then, has gradually come to be regarded by many archeologists as virtually a synonym for the ‘Desert culture’” (Thomas 1973:155).

After Jennings did his original work, Davis (1963) approached the same basic problem in the Owens Valley of eastern California. She obtained ethno- and archeological evidence that the culture of the Owens Valley Paiute had considerable time depth and was probably responsible for most of the archeological remains in that area. She described the subsistence-settlement pattern of a band near Mono Lake that she referred to as “one expression of the Desert culture” (1963:203). The Owens Valley Paiute were intensively studied by Steward (1933, 1938), who determined that they were subdivided into true land-owning bands (1938:50–51). Rather than propose broadly that the Owens Valley Paiute were representative of the cultures that had produced the “Desert culture” archeological remains, Davis left the question open by saying that:

*The concept of an inclusive “Desert culture” has been in use for almost ten years. . . . This generic term is an abstraction, a generalization which classifies and sets apart a number of distinct, ecological adaptations developed by primitive peoples within the Great Basin. These subcultures represent specific adaptations to a diversity of arid habitats.* (1963:202)
cussed were likely to have been varied and, in the Owens Valley at least, similar to those maintained by the Paiute in historic times.

At this point it is useful to digress and discuss ethnographic modeling in archeology generally. Flannery has made it clear that the use of ethnographic analogy does not mean the selection of an ethnographic example which is "projected backward in time to put flesh on the archeological skeleton." Rather, one must use an analogous ethnographic group to construct "a behavioral model to 'predict' the pattern of archeological debris left by such a group" (1967:122). As Thomas has put it, if the people who produced the archeological materials had behaved in the fashion suggested by the ethnographic example, "how would the artifacts have fallen on the ground?" (1973:157). As discussed by Thomas, an ethnographic model has the status of any theory. As such, it can never be verified directly; one can only test the logical consequences of the theory. In this case, the "logical consequences" are the predicted patterns of archeological materials. If the empirical arrangement of archeological materials correlates with the pattern predicted by the ethnographic model, then the model (theory) is substantiated. As Flannery points out, there is rarely a complete verification of a model, and there will nearly always remain factors that are not explained by it. These "residuals" lead to the formulation of modifications which in turn demand more empirical testing until an explanatory model has been constructed that satisfactorily correlates with the empirical archeological materials. The accepted theory becomes that one which best fits the empirical data.

The procedure advocated by Flannery and Thomas indicates that all current propositions regarding the nature of the cultural entities responsible for the "Desert culture" archeological remains stand in the position of hypothetical models in need of empirical testing. On the one hand, there is the archeological data base consisting of artifact descriptions, site inventories, distributional analyses, and dates for assemblages. On the other hand are various models concerning the nature of the cultures responsible for these materials. For the most part, these models can be arranged in two basic categories: those derived from a generalized Owens Valley Paiute model and those derived from a generalized Shoshoni model. As discussed previously, the Shoshoni model is characterized by wandering family groups that did not maintain territories, and the Owens Valley Paiute model by territory-maintaining bands. The essential differences revolve around territory-maintenance combined with social and ecological stability in the latter model versus the converse in the former. Statements in the literature about the nature of the prehistoric cultures in question are, in nearly every case, projections backward from ethnographic knowledge derived from one or the other of these models. There has been little or no attempt made to test the validity of these models empirically.

Thomas recognized these problems and designed an archeological project intended to provide an empirical test for Steward's Shoshonean subsistence pattern. Unfortunately, his project focused on the seasonal round as the "taproot of the entire Shoshonean culture adaptation" (Thomas 1973:156). His research design, therefore, was structured to test for the former existence of a seasonal-round subsistence pattern in the Reese River Valley of Nevada. He produced archeological evidence for the verification of a model postulating a seasonal round in the area. However, since both the Shoshoni and the Owens Valley Paiute subsistence-settlement patterns were based upon a seasonal round to obtain essentially the same resources, the former existence of either pattern could have resulted in the arrangements found by Thomas. His test demonstrated the former existence of a seasonal round in the Reese River Valley, but it did not test the validity of the Shoshoni model versus the Owens Valley Paiute model.

The widespread acceptance by Southwestern archeologists of Steward's Shoshoni model would not be so critical if it were not at variance with worldwide ethnological patterns. Service (1971:46-98) in his survey of band societies, perceived that the Shoshoni family-band, as presented by Steward, was anomalous and did not fit the patterns of social organization observed elsewhere in the world. This situation led him to examine in depth the historical records pertaining to the cultures of the Plateau and the Great Basin. He concluded that the Shoshoni family-bands, lacking territories, were probably the product of historical dislocations resulting from the predatory activities of equestrian Indians after ca. 1750 and the impact of white contact after 1850. Service's argument is that the Nevada Shoshoni had originally maintained territories ("districts") and were socially organized into bands composed of approximately 63 persons per group, "a fairly standard band size elsewhere in the world" (Service 1971:87). It would appear that Service regarded Steward's Shoshoni model as erroneous, resulting in part from the problem of dealing with "memory culture" elicited from informants years after the original culture had disappeared (Thomas 1973:156).

Williams (1974:39-40) has reviewed the situation in regard to both the specific situation in the Great Basin and the nature of band societies in general. Considering the ethnohistoric information offered by Service and the general model of band societies that emerges from a worldwide perspective (Service 1971; Williams 1974; Wobst 1974), the conclusion reached by Williams seems appropriate: "we must conclude that, to say the least, the precontact situation of the Shoshoni is in doubt" (1974:102). In other words, Steward's Shoshoni model must not be accepted unequivocally as representative of the prehistoric
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...cultures grouped together as the “Desert culture.” At the very minimum it cannot be accepted without a convincing empirical archeological test, and this, unfortunately, Thomas’ 1973 study does not provide. This problem assumes considerable importance when attempts are made to reconstruct prehistoric subsistence-settlement patterns (Stuever 1971) and extinct cultural systems (Binford 1962, 1964, 1965; Glassow 1972:289–93) from the spatial distributions of archeological materials (Binford 1962:425–26).

Fred Eggan (1980) has recently reviewed and summarized Great Basin kinship structures and their significance for anthropological theory. This in-depth study emerges from Eggan’s long familiarity with the topic. The Owens Valley Paiute model that is proposed below quite plainly makes no effort to work backward in time from the various implications of the diverse kinship structures and linguistic patterns that existed in the ethnographic present as does Eggan’s study. Rather, my method generalizes the Owens Valley situation into a model that can be tested against empirical archeological remains. In short, it differs from that of Eggan and the ethnologists who have long sought to understand the precontact social organizations of the Great Basin. The method proposed here suggests that archeological data combined with ethnographic models may be able to provide insight into this long standing problem. It does not attempt to work directly back from the ethnographic base as ethnologists have traditionally done.

One obvious problem with working backward from the ethnographic horizon is the possibility that the Shoshoni may have moved into the Great Basin of Nevada and Utah between 1200 A.D. and 1700 A.D. (Eggan 1980:185). If this is true, how could the Shoshoni social organization (as among the Gosiute near Danger Cave) have any direct bearing on the social organization of the late Archaic between 2500 B.C. and A.D. 1?

In light of the above objections to the Shoshoni model, it is proposed that the suitability of an ethnographic model derived from a generalization of the Owens Valley Paiute be considered. This model does not suffer from ethological ambiguities, is more in harmony with the model of band societies advanced by Williams (1974), and has been shown to relate to the archeological cultures being discussed (Davis 1963). In its simplest form this model proposes that the archeological remains encompassed by the broad category “Desert culture” were produced by structured bands inhabiting definite territories. The general implication of this model is that formerly the Basin-Plateau region was occupied by structured bands whose “district” divided this area much as Australia was divided by landholding bands at the time of white contact (Service 1971:48–49).

The Owens Valley Paiute have been described in detail by Steward (1933; 1938). Their subsistence-settlement pattern especially as this pertains to archeological remains, was described by Davis (1963). The following summary is abstracted from these sources. The Owens Valley Paiute were subdivided into independent, land-owning political units. Steward terms these units “districts” and shows the location of each of them on maps he prepared from ethnographic information (1933:Map 2; 1938:Fig. 7). Each territory roughly consisted of a long narrow rectangle that spanned the Owens Valley from the crest of the Sierras on the west to the White and Inyo Mountains on the east. This division of land gave each band access to the major ecological zones in the area, from the high mountains on either side to the valley floors between. The territories were commonly related to the streams flowing east from the Sierras. “Food getting was carried on within the band’s own territory, except where people were invited to participate in hunts elsewhere” (Steward 1938:52). Steward states, “permission to gather food was sometimes given outsiders, especially during abundance”; to obtain this, however, “the head man secured popular sanction at a meeting” and “trespassers were asked to leave by the head man” (1933:305). Physical force and magic were “potent deterrents to trespass” (1938:52). The primary economic resources of each band were obtained from within their territories; these resources were the property of the band and access to them was carefully regulated.

Within its territory, each band followed a regular pattern of seasonal movements from one locality to the next (Davis 1963). This cyclical, seasonal round was determined by the maturation schedules of economic plants and other resources. In the course of these moves, “they re-used sites time and time again over centuries and millennia (Davis 1963:204). Over the years, as tools were lost or discarded, an accumulation of artifacts occurred at each regularly visited locality; these artifacts were the ones appropriate for and used in the performance of the tasks at each locality. Because the resources in the different localities varied, the activities performed, the structure and composition of the social units present, and the types of tools required also varied. As Davis found on an archeological survey of sites utilized into the ethnographic present, the “different types of sites have different artifact assemblages” (1963:204). There was thus present in the territory of each Owens Valley band a series of artifactually differentiated, complementary sites, situated in different topographic and ecological settings, which had been produced in the course of many seasonal rounds. The pattern described by Davis on the basis of a combination of ethnographic and archeological information closely parallels the results of the Mousterian study by the Binfords, where tool kits were defined which they proposed correspond with specific kinds of activities and varied between sites depending upon the activities carried out at them (Binford and Binford 1966:238–95). In this study, they introduced the useful
term “facies” to refer to the differentiated but complementary sites that together make up the full range of localities used and visited by a single group in the course of a yearly cycle.

Each economically autonomous band operating in its territory would constitute a “cultural system” in the sense discussed by Binford (1962, 1964, 1965), Rappaport (1967), and Glassow (1972:189–93). Each band would be analogous to a “local subsystem” (Rappaport 1967), and could be regarded as part of an ecosystem in the manner proposed by Rappaport for the Tsembaga of New Guinea:

It is convenient to regard the Tsembaga as a population in the ecological sense, that is, as one of the components of a system of traphic exchanges taking place within a bounded area. Tsembaga territory and the biotic community existing upon it may be conveniently viewed as an ecosystem . . . An ecosystem is a system of material exchanges, and the Tsembaga maintain against other human groups exclusive access to the resources within their territorial borders. Conversely, it is from this territory alone that the Tsembaga ordinarily derive all of their foodstuffs and most of the other materials they require for survival (Rappaport 1967:18–19).

A band within its territory thus can be viewed as an economically autonomous system. Exceptions would be social relationships with neighboring bands that systematically allowed resource sharing in times of variable production and minor trading between band/systems.

Socially, each Owens Valley band would correspond with the “minimum band” discussed by Wobst (1974:151–152), which is basically equivalent to the lineage-band defined by Williams (1974:25–26). The aggregate of all Owens Valley bands, interacting in such matters as marriage exchanges, interband communal hunts, and ceremonialism, would correspond with Wobst’s “maximum band” (1974) and William’s “connubium” (1974:27–29).

Davis (1963) offered archeological and environmental evidence that the type of subsistence-settlement pattern observed ethnographically in the Owens Valley had considerable time depth. She described the nature of the seasonal round followed by all of the Owens Valley bands, which she called “seasonal transhumance,” then focused on the particular round of one band near Mono Lake, the various localities used and the resources that brought the band to each place year after year. She was taken to these localities by Paiutes familiar with each place and the resources obtained. Her examination of the archeological materials present in these various localities indicated that each locality had been visited for a long time and that, therefore, the pattern of seasonal transhumance had also been in operation for a long time:

Archeological evidence (and the physiographic and ecological situations of the sites themselves) suggests the same kinds of uses of these biotopes over many thousands of years. The stability of this way of making a living is one of its most interesting aspects. . . Along the piedmont of the Sierra Nevada, and in the adjacent deserts, there are archeological clues which indicate a considerable antiquity for Desert Culture practices, particularly the custom of seasonal transhumance between high and low altitudes. The occurrence of Pinto and Silver Lake points in mountain meadows suggests that transhumance is as old as the Desert Culture itself. (1963:202–203).

Subsequent to Davis’ work and to my initial work with this subject (Schaafsma 1974, 1975, 1977) Bettinger worked on the archeology of the Owens Valley (Bettinger 1977). His overall interpretation of the culture history of the Owens Valley is at variance with Davis and the simplified model that is being presented here. On the one hand, I am sure that in any given area events will become more complex upon detailed analysis. This may be the case in the Arroyo Cuervo region of New Mexico where Irwin-Williams (1973) has described a complex local culture history. On the other hand, I believe it is best to start with a simplified general model. There are also definite objections to Bettinger’s interpretations (Madsen 1981) which I do not wish to deal with here. I believe that a simplified model lies latent in the Owens Valley Paiute example that can be generalized regardless of the actual situation that may have existed in the Owens Valley.

The implication of the generalized Owens Valley Paiute model is that once a particular system comes into being and achieves an adequate adaptation to the total environment, there is no reason for it to be altered with the passage of time. Once set in motion it goes on and on, and it will continue as long as conditions to which it is adapted remain constant. As Flannery observed “Under conditions of fully achieved and permanently maintained equilibrium, prehistoric cultures might never have changed” (1972:230). Thomas (1973:155, 158) assumed a similar stability for the Reese River subsistence-settlement system in his computer simulation model for the period from 2500 B.C. to 1840 A.D. In addition, Weide (1974:73) offers archeological evidence that the North Warner Valley subsistence network was in existence for the 1800 year period between 1200 B.C. and A.D. 600. The stability of band-cultural systems over time is also discussed by Wobst (1974: 156).

Just how long any particular band/territory/seasonal-round subsistence-settlement system was in operation must be demonstrated with empirical evidence from the archeological record. However, a number of the general statements offered above provide clues that help order the data found “on the ground.” First of all, it has been suggested that once
a cultural system has achieved a successful adaptation to a given environment, it will remain essentially constant as long as the total environment remains constant. Davis has suggested a valuable clue to the continuity of the Owens Valley bands. She suggests that the physiographic and ecological determinants of the seasonal round have remained basically unchanged since the physical arrangement of cultural activities was made. This may have been at the end of the Altithermal, or ca. 2500 B.C. Each system demanded a definite range of localities (a resource base). Once trails were made between these localities, sites established for seasonal habitations, and boundaries defined between neighboring systems, then the total system could have been maintained indefinitely.

An example of a band that survived within a definite territory with very little change from the end of the Altithermal until the recent past (ca. 1851) is possibly provided by the Areneno band of Sand Papagos inhabiting the Sierra Pinacate in northwestern Sonora. "Of present interest is the clear indication that the enclave (Sierra Pinacate) was occupied continuously from early post Altithermal times to the twentieth century by one of two bands of a single people known in the historic period as Arenenos or Sand Papago, a dialect group of the Papago" (Hayden 1970:87). The primary data presentation for this conclusion and an extraordinarily intriguing map of the trails used by these people over time were presented in an earlier article (Hayden 1967). What Hayden concluded was that the Sierra Pinacate was the territory of the Pinnecateno band, which continuously occupied these mountains for approximately 5000 years until they were decimated by yellow fever in 1851 (1967:335). Hayden's conclusion that this desert enclave was occupied by the same band from the late Altithermal onward is in harmony with the general proposition that once a cultural system becomes adequately adapted to a total environment it can persist over time until disturbed by conditions upsetting its adaptation.

In summary, the model presented here proposes that band territories can be expected throughout the Basin-Plateau region, or in the province of the "Desert culture" as originally defined by Jennings (1957). These would be most readily definable and most important for understanding the origins of early Anasazi agriculture in the period roughly 2500 B.C. to A.D. 1. I have previously suggested (Schaafsma 1978) that social organizations that regulated the utilization of wild plant resources may have been a necessary precondition for the transition to agriculture. Social and ecological stability are basic features of the model being proposed here. Stable conditions of this nature may well have been necessary for the emergence of agriculture among the early Anasazi of the Plateau.

Toward the end of defining these territories and testing the model, adequate methodological tools are required. One of the goals of the author's work at Abiquiu Reservoir (Schaafsma 1975, 1977) was to develop the necessary analytic methods. This work plus other advances since about 1973 have provided us with the analytic tools needed to deal with the questions being posed here.

The model must be tested in as wide a variety of ecological and topographic settings as possible. The ecological relationships in any research area must be worked out concurrently with the strictly artifactual work. While Thomas' study (1973) considers the question of cultural dynamics and ecological relationships and lays the foundation for dealing with energy flow models, it is necessary to discriminate the discrete cultural systems which were the primary locus of cultural process before these energy flow models can be placed on a firm footing. I submit that at this time we really do not know the nature of subsistence-settlement systems during the late Archaic and we must propose models and test them in order to gain some reliable ideas about this important period.

Testing the Owens Valley Paiute Model

The Owens Valley Paiute model has the status of an untested hypothesis (theory) and needs empirical verification through the testing of its logical consequences, that is, the predicted arrangement of archeological materials "on the ground." If the archeological materials are found to be of the nature predicted by the model, evidence will exist for regarding the model as valid. Verification of the model in this manner requires that a series of propositions be devised from the model and tested. The following proposition can be deduced from the generalized Owens Valley Paiute model:

1) There will be found in any given territory a set of differentiated sites whose features and artifactual composition reflect the activities formerly carried out in each location. These complementary sites will make up the archeological record of the band formerly inhabiting the territory. The sites will be located in the range of ecological and topographical situations formerly visited by the resident band. Because the resources in the different locations would have varied, there would have been variations in the activities performed, the structure and composition of the social units present, and the types of tools required.

2) Sites of adjacent bands in the same ecological setting where the same tasks were performed and similar tools were used and discarded will be similar. In other words, equivalent facies sites of adjacent bands will exhibit similar artifactual inventories.

3) The complementary sites of any given band will be situated in a geographic space sufficient in size and variety of resources to provide the primary resources of the resident population. That is, the territory of any band, in which its sites are to be found, will be sufficient to provide for the resource needs of the band.
Preliminary support for these propositions was derived from the author’s work at Abiquiu Reservoir (Schaafsma 1977, 1978).

In addition to the above primary deductions from the model, the following conditions may have existed that could lead to the identification of band/cultural systems in the archeological record:

1) Variant technological traditions within each band could lead to unique constellations of artifact attributes which might result in minor attribute variations in artifact classes that would otherwise be similar for adjacent bands. It is anticipated that these variations in minor attribute forms and associations between attributes will appear most evident on items that receive the most preparation, such as projectile points, bifacial knives, and so on. Such variations may originally have been group identification symbols or means by which the artifacts of a given band were identified in interband hunts. Variations of this nature can potentially help to discriminate between the artifacts of different bands.

2) The distribution of geological materials is a potentially major way in which to discriminate social groups in the archeological record. Individual bands could be depicted if a) quarries were located in the territories of individual bands and the bands controlled access to these resources; b) different bands visited different quarries in the course of making seasonal rounds. In both cases, each band would have had access to a unique set of geological materials that would result in a distinctive lithic set in the artifactual inventory. (For an application of this technique see Weide 1974.) Rocks and minerals are often found in archeological contexts which are quite distant from their geological proveniences. It is clear by their incorporation into the artifactual structure that materials were transported in the course of human activities conducted within the patterned framework of now-extinct cultural systems. The intriguing suggestion for archeology is that these materials were selected, extracted, transported, and used according to systematic patterns in the cultural systems of which they became a material component. In the consideration of their geological proveniences, spatial distribution, and incorporation into the artifactual structure of cultural systems, there lies a wealth of information reflecting the regional contacts, spatial range, and functioning of the original culture.

3) In addition to the potential variability that relates to differential access to geologic materials in the environment, there is the possibility that the members of different bands could select particular lithic types from the available quarries according to culturally regulated patterns. An ethnographic analogy from the comparably adapted hunting and gathering bands in the Western Desert of Australia is of interest here:

The Nyatunyatjara and northern Ngatadjara men prefer the yellowish quartzites and creamy yellow cherts found in their area. These preferences have little to do with the actual working qualities of the different materials, for, in fact, these are all satisfactory materials for stone-chipping. Rather, these preferences appear to be a reflection of the close totemic ties each man has to the particular region in which he was born and from which he claims totemic descent. Thus, a man may have a sense of kinship with some of these localities, and he will value the stone material from them as a part of his own being. Stone materials thus acquired are not sacred in any strict sense but are nevertheless valued highly enough to be transported over long distances by the owners. The same general sentiments apply to pebbles and rock slabs used for grinding seeds and to the small pebbles used as hammer-stones for percussion flaking. . . . Situations like this make it easier to understand how lithic materials can be found occurring on sites many miles from the localities where they were quarried or collected (Gould, Koster and Sontz 1971:161#62).

4) While variability between bands may exist in regard to the extraction of material types, variability may also derive from the selection of particular material types for different artifact classes. Patterns of preference may have varied from band to band. These patterns would be evident as statistical regularities characterizing the artifacts of each band.

Through the use of adequate model building and satisfactory empirical testing we may someday be able to confidently state what the social organizations and settlements patterns were during the late Archaic. The proposal made here is that the late Archaic was orderly and that social organizations and settlement patterns existed to integrate human populations and available resources in a systematic manner. Such conditions may have been essential for the transition to agriculture and settled village life.

QUESTIONS AND COMMENTS

Jim Muller: In looking for analytical tools to handle hunters and gatherers, I wonder if the old idea of structural poses may have a usefulness?

Curt Schaafsma: In a sense, what I was referring to there was simply adequate devices and methodological/technological approaches on our part for handling masses of archeological tools - little stone tools, hammers, choppers - and in order to reasonably read the archeological record. Not so much the structural pose kind of thing, but the ability to handle the kind of archeological material that you come up with when you deal with Archaic materials. You deal with thousands of artifacts scattered all over the countryside; that's what Thomas ran into when he was working at East River, and that's what I ran into in the Chama Valley. And I think that we now have, through various kinds
of lithic analysis methods, the ability to deal with thousands of lithic tools and discriminate the minor variations in site inventories that clue you in to what was happening in any given locality. That's the kind of thing that I was talking about, just techniques.

Elizabeth Morris: I can't resist saying a couple of things, myself. I've spent the last ten years or so working in the opposite corner of the state in the South Platte River drainage, mostly finding Archaic sites. When I went up there, my background was the same as very many of us that are here, almost entirely based on the Southwest. I knew lots about ceramics and lots about architecture, different culture areas and theoretical squabbles about how to divide them up and why. I could hardly find any sites up there. The Indians never got around to architecture except for teepee rings, and potsherds only occur on about one out of 50 sites and they usually fit into the palm of one hand. Not very informative. But I had some smart students (trained mostly by Jim Judge) and they were able to show me some lithic sites which are the prevailing kind of site up there. And after a decade or so, we are beginning to learn a little bit about them.

Curt passed over, as he was reaching the end of his paper, a statement which was going to be a plea for data. We need lots more sites. The Archaic people inhabited the Southwest for thousands of years longer than the Anasazi. Whether they are directly ancestral to them or not, they were here. There are sites. The Oshara typology is a sketch. It is based on quite a lot of data. I don't know whether it's ancestral Anasazi or not, but it does detail a sequence of phases of Archaic people probably doing different tasks on different sites in a hunting and gathering context. What we need is all of you, out there, recording the lithic sites as well as the architecture and pueblos and the rock art.

Acknowledgments

This paper emerged as a Ph.d dissertation proposal at the University of New Mexico in 1974. Early discussions with Dr. W. James Judge, Chairman of my 1974 committee, that led to this basic research design are gratefully acknowledged. The Owens Valley Paiute model then guided my research for the School of American Research at Abiquiu Reservoir between 1974 and 1978. The present version relies heavily upon the 1977 SAR report. (Schaafsma, 1977) The approval of Mr. John Beal at SAR to use this material for the present paper is much appreciated.
THE STATUS OF ARCHAIC PERIOD SITES IN THE ANASAZI AREA

Elizabeth Ann Morris

(Editor's note: The following comments by Dr. Morris were written after the Anasazi Symposium. Dr. Morris volunteered to provide this summary of the status of past and present studies of the Archaic in the Anasazi area to provide a broader background from which to view the comments made in the two previous papers.)

Archaic sites in the Southwestern culture area have been largely overlooked, with some notable exceptions, because of the prevalent interest in the architecture and ceramics of later times. Dendrochronological temporal control makes the Southwest the most precisely dated archaeological culture area in the world (for example, Bannister, Hannah and Robinson, 1955 and the accompanying volumes). The resulting capability for developing method and theory has focused the attention of generations of workers. This is not surprising in view of the high visibility, density and informative potential of the more recent sites; but it is a little surprising that the prehistory of the last two thousand years has received so much attention and the approximately six thousand years preceding it have received so little.

The perspective of the writer is one of a scholar who spent the formative years of life in the Southwest and who has spent the last decade studying the archaeology of the South Platte river drainage basin in northeastern Colorado. Geographically this is not much further from the Anasazi area than is the Hohokam area in the drainage of the lower Gila River, but there are vast differences in the nature of the prehistoric remains. The typical site assemblage consists of a scatter of flakes and an occasional scraper, chopper, or projectile point and some fire-cracked rock. Rarely we find ground stone tools. Even more rarely a site will have beads, ceramics, burials, or historic trade goods. Rarest of all are rock art and architectural remains consisting of rock shelters, sometimes game drives, and “teepee” rings (Morris 1981). Our minimal site definition is two (2) of anything. Most of our datable sites are pre-ceramic, preagricultural, and date before the time of Christ. The remarks that follow are therefore colored by this perspective.

Archaic period sites and tool assemblages for the Greater Southwest have been identified in earlier years. Some of the references are Sayles and Antevs (1941), Bryan and Toulouse (1943), Haury (1950; 1967), Mohr and Sample (1959), Kelley (1959), Hunt and Tanner (1960), Irwin (1964), Rinehardt (1967), and Hayden (1970) to mention but a few.

An important volume, because of the large number of artifacts and types as well as an extensive comparative discussion, is Emil Haury's Ventana Cave report (1950). This was the reference of most use to this writer when trying to assign age and cultural affiliations to lithic sites found in the highest portions...
The Status of Archaic Period Sites . . .

of the Mogollon Rim on the Arizona-New Mexico border (E.A. Morris in press).

The survey of Wetherill Mesa (Hayes 1964) reported no Archaic sites. No doubt the solid carpet of later utilization of the mesa tops has obscured any predict- able, delicately represented Archaic remains. Discussion with Mesa Verde archaeologists indicated that a logical and accessible place to look, (as opposed to, for instance, tearing down Cliff Palace to see if anything is under the masonry) would be deeply stratified open sites that have escaped serious erosion (Ingmanson 1981, personal communication; Smith 1981, personal communication).

In their archaeological surveys of Chaco Canyon, Hayes, Brugge, and Judge (1981) describe surveys that resulted in locating 2220 sites. Seventy are identified as Archaic, including the Basketmaker II period. The authors credit Dennis Stanford, then one of their employees, for focusing their attention on the typically small lithic scatters. Out of the 70 sites, 21 are assigned to chronological “phases” on the basis of associated projectile points. One is listed as of Plainview affiliation, three are Early Archaic or “J”, seven are Pinto Basin, eight are San Jose, and two are Basketmaker II. Interestingly, especially when compared to Rehrer’s survey and Simons’ excavations (to be discussed below), location of sites was often on a south facing exposed position near a mesa edge or on a bench. Partially this is attributed to where the sites are visible enough to be found, but it almost surely reflects a preference for enotive utilization and possibly climatic and “look-out” factors as well.

Stuart et al (1981) describe four surveys done in northern New Mexico mostly under the administrative direction of the late Frank J. Broilio. The first, a survey of 3,000 acres on the upper Pescado drainage on the Zuni reservation, recorded 50 sites, none of which were earlier than approximately 1000 A.D. This amazing circumstance may be attributable to the revegetated sand dunes that are common to the area and may cover earlier sites, but it also seems possible that the surveyors missed sites consisting of small lithic scatters.

The second survey is of 800 acres on the Tetilla Peak Recreation area on the Cochiti reservation. Fifteen sites were located and four of them consisted of collections of basalt flakes. One also had Pueblo IV ceramics on it. This one and two others are so similar as to be interpreted as Pueblo IV functionally specific chipping sites. The fourth lithic site (LA11591), is described as having “gross differences in lithic assemblage when compared to those of the known Anasazi sites in the area.” The report does not describe the differences except to mention a higher proportion of debitage than usual, and that “diagnostic artifacts are lacking.” This writer suggests that most artifacts are undiagnostic the first time that they are found and that collection and later comparative studies may eventually be informative. This underscores the importance of making as complete as possible a site collection at the time of the first visit, of recollection of the site as frequently as the results seem to warrant in order to increase its interpretive potential, and of meticulous laboratory study.

A second Archaic site (LA11590) had previously been found in the area but it could not be relocated. Maybe it was too small to find. On the other side of the complete collecting strategy indicated above, if really completely collected, the site literally disappears from the face of the earth and exists only in the laboratories and field records of the archaeologists.

The third project discussed in this volume is entitled the Wesco 8.8 mi. Burnham Access Road Survey. Located on the east side of the Chaco Wash, 20 sites were located in 1,304 acres. Two produced undiagnostic lithics and are described as Archaic. It is to the credit of this crew that they had expected more Archaic sites. They went on to explain the low proportion by referencing Rehrer’s (1977) statement that Archaic sites were more likely to occur in upland areas with richer vegetational resources than in lower areas like theirs.

Finally, the Gasco Survey was a 100 mile pipeline from Star Lake to Gallup, New Mexico. There were 52 sites recorded in 3,175 acres, two of which are described as Archaic. Both were in upland areas and are attributed to the San Jose phase of the Oshara tradition (Irwin-Williams 1973).

Rehrer has edited a volume describing survey on the Lower Chaco (1977). Having received his initial training in Wyoming, he was more sensitive to the information potential in lithic scatters and his report reflects this clearly. Ninety-nine Archaic sites were identified out of a total of 718 sites. A considerable amount of useful discussion accompanies the tabulation, and it is particularly interesting with regard to the ecology of settlement pattern location and the utilization of local resources by hunting and gathering peoples. Of the admittedly limited number of reports referred to in this paper this is clearly the outstanding example to follow.

A report on the 1980 excavation of a number of Archaic sites north of Chaco Canyon and east of the Chaco Wash by Allen H. Simmons will be published in the near future by the Navaho tribe. It is generally keyed into the Oshara tradition phase sequence and reportedly contains a wealth of information (C. Breternitz, 1981, pers. comm.). This should develop Rehrer’s survey information usefully. Similarly, it will constitute a test of the utility of the Oshara phase sequence definition.

A significant landmark in the growing recognition of the existence of Archaic peoples in the Anasazi area is Cynthia Irwin-Williams’ definition of the Oshara tradition (1973). Based on extensive survey and excavation in the Arroyo Cuervo and nearby regions of northern New Mexico, this preliminary for-
The post Paleo-Indian, pre-Anasazi sequence begins with the Jay phase dating from ca. 5500-4800 B.C. It is characterized by "large, slightly shouldered, projectile points reminiscent of those termed "Lake Mohave" in Arizona and California" (Irwin-Williams 1973:5). It is followed by the Bahada phase, ca. 4800 B.C. to 3200 B.C., that has points with somewhat longer, straighter-sided stems and indented bases. These points continue into the following San Jose phase. The shape and especially the later part of the temporal range are suggestive of the McKean complex in the northwestern Plains.

The San Jose phase, dating from about 3200 B.C. to 1800 B.C., besides containing the points described above, is additionally characterized by a shorter blade-to-width ratio and includes an increasing proportion of serrated edges. It is followed by the Armijo phase, dating from about 1800 B.C. to 800 B.C., and including the first evidence of maize agriculture. Armijo projectile points include shorter serrated blades with rather wider bases and shallow side notches and a second unserrated type with a very narrow proximally reducing triangular stem. This is followed in turn by the En Medio phase, dating from about 800 B.C. to about 400 A.D., and considered to be a Basketmaker II development in Anasazi culture. Agricultural evidence is more numerous and the points are short and triangular in shape with corner notches. Finally, the Trujillo phase, 400-600 A.D., is considered to be Basketmaker III and includes the addition of ceramics and the bow and arrow.

Additional phase attributes such as flaked and ground stone tools found in association, hearth forms, associated climatic indicators, and settlement pattern data are supplied in the original definition. The sequence of projectile point types is roughly suggested by obsidian hydration dating in the Rio Grande National forest just north of the New Mexico border in the San Luis Valley of Colorado (Burns 1981). Some of the types occur further north in Colorado and a few of them are in dated contexts that tend to support the dates suggested by Irwin-Williams (Gooding in prep.; Marcotte, Mayo and E.A. Morris 1978). Still others occur in extreme northeastern Colorado as do most of the defined Paleo-Indian complexes, and raise the complicated question of what kind of communications and linkages existed between prehistoric peoples who shared artifact types and assemblages over considerable or even vast areas of time and space.

There have to be more pre-pottery sites in the Southwest than have been found. It is true that some will be obscured by overlying later materials, and the few early lithics, unless extremely diagnostic, will be lost among the similar later remains. Other sites, however, will not be masked by later events and may be distinguished from Anasazi lithic sites in the same way as they are distinguished from each other. Namely, this will depend on differences in tool typology, site assemblage attributes, quarry source utilization and location, as well as various methods of chronological control. It is fundamental to our understanding to locate more sites by being aware of what to look for and where to find it. Larger collections and revisitation, when it is possible, will require more investment of time and other resources; but it will result in increasing our knowledge of temporally, culturally and functionally diagnostic artifacts and the range of variation of these artifacts. Debitage analysis, as one of the lowest possible common denominators of a lithic flake scatter, indicates reduction procedures and suggests directions for functional interpretation of sites. Quarry source analysis, as another of the commonest of denominators, can reflect the geographic range of the group, possible cultural exchanges, contacts, and in some cases temporal and cultural identity by utilizing comparisons with other site assemblages with better chronological control. This type of research may proceed slowly, but until better means are available they do provide a foundation to build upon. Increased understanding of this important, relatively little understood period in prehistory will largely depend on the industry and sensitivity displayed by the field workers of the future.
AN ANASAZI MISCELLANY

Thursday, October 1, 1981
Afternoon Session II
Chair: Susan M. Collins
KAYENTA CRAFT SPECIALIZATION AND SOCIAL DIFFERENTIATION

J. Richard Ambler

Introduction

Trade has long been a topic of interest to students of the Anasazi. Items easily seen as exotic to particular locales and the region as a whole have been cause for mention and speculation since the first explorations, and there is hardly a site report produced since the 1930’s, when ceramic typologies were becoming formalized, that does not at least include some listing of “trade wares.” To Anasazi archaeologists, ceramics have long been a favorite artifactual class for “defining” trade, because the interlocking set of attributes that combine to make a vessel—materials manufacturing methods, shapes, and decoration, to mention the most obvious—are often discrete from one area to another and combined in different ways, thus leading to the relatively easy definition of types, varieties, styles, wares, and other archaeological constructs thought to have temporal and spatial (and therefore cultural) validity. Although the “compositionally complex” (Arnold 1980:148) nature of ceramics was clearly pointed out by Shepard decades ago (e.g., Shepard 1936, 1939, 1954) and used by her to postulate ceramic trade in the Anasazi region, it appears that until recently, few archaeologists have had the time, energy, inclination, and/or abilities to continue along the meticulous routes pioneered by Shepard. Spurred on by the masses of materials available for study as the result of federal legislation, adequate funding (for some projects), the development of a much greater range of analytical techniques, greatly increased attention to detail, more specifically defined questions, and the need to perform original research in order to obtain the advanced degrees that serve as union cards, some Southwestern archaeologists have productively turned to more detailed studies of Anasazi artifacts of all kinds, often with the defined goal of more precisely defining trade relationships (e.g., Plog 1980; Deutchman 1980; Toll, Windes, and McKenna 1980; Windes 1977; Chapman 1977). Although in their infancy, such studies are beginning to show that extensive trade was not a phenomenon restricted to the large regional centers or towns, but was probably a facet of everyday village life (Plog 1980).

Coupled with the increased interest in detailed studies, and in large measure supported by these studies, the past decade of archaeological work among the Anasazi has seen a great increase in attempting to define and explain social and behavioral processes. Statements concerning “expanding exchange spheres,” “complex redistributive economies,” or “trade controlled by local and regional elites” are becoming commonplace, yet one wonders how much we are really learning about the typical Anasazi, as almost without exception the emphasis has been upon what happens to the goods that were produced, not on the people that produced them. The postulation of an elite class necessitates the corollary: a large number of non-elite, who were busily engaged in producing food,
goods, and services that became concentrated in the stomachs, dwellings, or graves of the elite. Even the existence of simple trade networks implies that individuals concentrated to some degree on the production of certain items, exchanging them for others.

This paper is an admittedly very preliminary and generalized attempt to examine some aspects of and implications of craft specialization among the Kayenta Anasazi. For purposes of this paper, the discussion will center upon the “core” area of the Kayenta, largely excluding what has been called Western Kayenta, as well as what has been called the “Black Mesa Anasazi” (e.g., Gumerman and Euler 1976:164; Kessert and Layhe 1980), but what probably be better termed Southern Kayenta.

This area is generally comprised of the Klethla Valley-Tsegi Region, the Shonto Plateau, and the general vicinity of Navajo Mountain and lower San Juan. We have essentially no data from the Kaibito Plateau, and only a little from Monument Valley. Even the Shonto Plateau has been relatively little examined, so most discussion will center around the Tsegi and Navajo Mountain.

Any discussion of specialization and trade must rest upon a demonstration of some degree of contemporaneity, which necessitates the definition of finer cultural-temporal units than “Pueblo II” and “Pueblo III.” Both the Pecos Classification and Colton’s (1939:52-59) scheme have been found to be inadequate for the Peabody coal lease area of Black Mesa (Gumerman, Westfall, and Reed 1972), and I would suspect that the differences perceived between the coal lease area and neighboring regions (Gumerman and Euler 1976:164) are due more to the inadequacies of Colton’s early attempt than to major discrepancies between the Black Mesa folk and their northern neighbors, since similar classificatory difficulties exist off the mesa. To simplify the following discussion, without going into detail for the rationale and with full expectation that finer divisions will become possible as our knowledge increases, I propose the following phase sequence for the Klethla Valley-Shonto Plateau-Navajo Mountain region during the 12th and 13th Centuries:

<table>
<thead>
<tr>
<th>Phase</th>
<th>Approx. Dates</th>
<th>Pecos Equivalent</th>
<th>Principal Ceramic Associations</th>
</tr>
</thead>
<tbody>
<tr>
<td>Klethla</td>
<td>1100–1170</td>
<td>Late P II</td>
<td></td>
</tr>
</tbody>
</table>

This paper will focus upon the individual level of craft production, particularly pottery, with some discussion of the relationship of the individual craftspeople to the community and the locality. Most of the discussion is intended to point out lines for future research (hypotheses to be tested, if one prefers buzzwords) since hard data is woefully lacking, and no pretense of having exhausted the potential of all available literature is made.

Potters

Evidence that certain individuals specialized in pottery manufacture is well known for the Kayenta region. Perhaps the most definitive information suggestive of individual pottery specialization comes from the individuals themselves, or at least what is left of them and with them after death. At RB568 (Beals, Brainerd, and Smith 1945:79), burials of five potters were recovered. One of these was accompanied by not only tools and supplies for making pottery but two unfired Kayenta Polychrome bowls as well. During the course of an NAU field trip visit to Long House in 1969, a burial was noted eroding out of the sand nearby. Salvage excavation revealed several burials, one of which had two unfired Tusayan Polychrome bowls associated. I assume that this individual was also a potter of the Tsegi Orange Ware tradition. Guernsey (1931:110–111) reports a potter’s burial found a few hundred yards behind the trading post at Kayenta, which appears to be Shonto Phase, judging from the associated pottery. This burial was accompanied by an extraordinary number of tools used for ceramic manufacture. Thus, we have at least seven burials found that appear to have been of pottery specialists. All are Pueblo III, from the Tsegi and Shonto Phases.

It has been clear since the time of Kidder and Guernsey’s (1919) pottery descriptions that the Kayentans made three distinct classes of pottery, which were subsequently formalized as Tusayan White Ware, Tsegi Orange Ware, and Tusayan Gray Ware.
(Colton and Hargrave 1937; Colton 1955, 1956). Each of these wares differs markedly from the others in many respects: the raw materials are completely different, some forms (e.g., bowls) are similar but others are distinct, surface treatment of the gray wares is obviously distinct from the painted wares, the designs on the two painted wares differ markedly and firing techniques are assumed to be neutral to reducing for the white and gray wares and oxidizing for the orange wares. I tentatively propose that these distinct differences indicate that individual potters concentrated their efforts on producing one ware.

Of the five potters' graves found at RB568, "several" were thought by Beals, Brainerd, and Smith (1945) to show that the woman was involved in making more than one class of pottery. Burial #1 is the only one described in enough detail to make any judgements. The pottery making tools that accompanied that burial would be equally useful for Tusayan White Ware, Tsegi Orange Ware, and Tusayan Gray Ware, except that polishing tools would not be needed for the gray ware. The large-grained white sand does not sound like the fine material used in making the Pueblo III black-on-whites, although it may have served for the gray ware. At most, the accoutrements with Burial #1 may show the capability of making Tsegi Orange Ware and Tusayan Gray Ware, with nothing specifically indicating that the lady had made Tusayan White Ware. Specifics regarding the other burials are not given.

At first glance, the most convincing evidence presented by Beals, Brainerd, and Smith (1945:131–133, Figure 70G) that both Tsegi Orange Ware and Tusayan White Ware were made by the same individual(s) is the quartered bowl that has two quadrants in black-on-white (what I would consider Flagstaff style) and the other two in late Tusayan Polychrome style ("Variety B"). The catalog number of this bowl would suggest an association with Burial #46 at RB568, but it is not so listed in Map 6, so the associations are unknown. Without clear temporal assignment, this bowl could be an heirloom passed down for a generation or two and buried during the Dzil Nez Phase, or made during the late Shonto or Tsegi Phase, but with an earlier black-on-white style. I favor the last possibility because of the late appearance of the polychrome. Perhaps the maker ordinarily produced polychromes and, as a result of the motor habits built up as a result of her continued use of the broad-line polychrome style, had not gained the control necessary for the production of fine Tusayan Black-on-white, so resorted to the more simply executed Flagstaff style. Even if no definitive statement can be made based on this vessel, the combination of black-on-white and polychrome styles on the same piece certainly gives us food for thought regarding ceramic materials and firing technology.

It is obvious any argument for pottery specialization by ware is based on rather nebulous evidence, and a detailed examination of the technology and associations of the material recovered from RB568 would assist greatly in determining the extent of pottery specialization among the Kayenta Anasazi during Pueblo III.

In his usual inimitable style, Smith (1962; 1971:606–611) has argued for a school of potters all well versed in all aspects of contemporary ceramic manufacture, and the reader is forced by the sheer weight of his logic to agree. His implications are, however, that all potters participated fully in the production of all classes of pottery, whereas I would prefer to view the Kayenta potters as developing their own specialties within the school, and that these specialities correspond to what we define as wares.

Postulating this degree of pottery specialization among the Kayentans may appear to be at variance with Smith's concept of a ceramic school, and with the earlier statement (Beals, Brainerd, and Smith 1945:138) that "one woman could make all the contemporaneous wares." However, I see nothing incompatible with accepting the idea that all potters were familiar with each other's work, had probably at least tried the materials and techniques used by others, yet either through choice, ability, or necessity concentrated on only one major class.

Familiarity with the clays, the development of specific motor habits and artistic techniques, the promotion of contacts and sources for specific materials, the maintenance of trade outlets, and the growth of individual reputations would all have contributed to personal emphasis on one ware. The actual evidence for individual ceramic concentration to this degree is admittedly sparse, but it is of more than idle interest to note that in the cases of the Kayenta burials mentioned above, accompanied by unfired pottery, that unfired pottery was all Tsegi Orange Ware in both cases.

Any potter should have been able to make her own cooking pottery when the need arose, and the finely corrugated exteriors of occasional black-on-white bowls, particularly during the Klethla Phase, is an indication that the indented corrugation technique had been mastered by some of the black-on-white potters. However, the making of only a single gray ware vessel may well have involved a special trip to obtain temper and another to get clay, so it would seem to have been generally easier to trade a Tusayan Black-on-white bowl with a woman down the street or up the valley for a finished Kiet Siel Gray jar. The exchange ratio would be interesting to compute. In terms of time, energy, and artistic ability expended, I would guess that one small Tusayan White Ware bowl might have equaled a medium sized gray ware jar, and a large Tusayan Black-on-white jar would have been worth several Kiet Siel Gray jars.
The functional (or "social") position of Tsegi Orange Ware pottery has been such an enigma that few archaeologists have cared to touch the subject. During Pueblo II and III, the Anasazi in the Mesa Verde area, the Chacoans, and most others seemed to get along quite well with only gray wares and black-on-whites, yet the Kayentans evidently felt the need for a third distinct set of ceramics. The forms of Tsegi Orange Ware are generally close to those of Tusayan White Ware, with the exception that the large elaborate white ware jars are not generally duplicated in either size or shape by the orange wares, which could easily be a function of the relative quality of the clays.

On the occasions when decorated pottery was used as a culinary ware, Tsegi Orange Ware seems to have been selected (e.g., Beals, Brainerd, and Smith 1945:127). This propensity for using Tsegi Orange Ware vessels over the fire, coupled with the fact that surfaces of this ware are more easily abraded, might indicate that Tsegi Orange Ware was a second class pottery, and not held in such high esteem as Tusayan White Ware. More likely, however, would be the supposition that the clay body of Tsegi Orange Ware would stand the stresses of repeated heating better than would Tusayan White Ware, and hence could be used for preparing culinary delights. Tsegi Orange Ware was apparently more in demand as a trade ware in the hinterlands (see below), and thus probably cannot be considered to have been ranked as less desirable or more commonplace than Tusayan White Ware. Tusayan Gray Ware may not have carried any special interest as a prestige item, but a good cooking pot is always necessary, so would definitely have been in demand. Although I think it highly likely that some individuals specialized in the making of Tusayan White Ware, others in Tsegi Orange Ware, and others in Tusayan Gray Ware, and that the wares themselves had different uses and meanings, I see no real reason to think that the makers of the various wares had appreciably different ascribed statuses, particularly if we invoke Smith's ceramic school concept. I also fail to see any evidence that the status of potters would have been noticeably different than that of other craftspeople. Any prestige that accrued to individual potters would presumably have come about as a result of the way that potter fulfilled her status, i.e., her craftsmanship and artistry. However, a careful examination of grave offerings accompanying potters and other individuals may shed light on this problem.

A discussion of the temporal development of pottery specialization by ware is difficult because of the lack of data, and to even speak of potters in general as specialists prior to the Shonto Phase is based more on inference than facts. It would seem that specialization by crafts was present at least as early as Pueblo II, and may well go back to Basketmaker III. Beals, Brainerd, and Smith (1945:133, 149) have succinctly pointed out the similarities, differences, and evolutionary trends in the black-on-white and orange ware types, and little can be added to their summary. If Klethla Phase potters were specializing by ware, we would have a situation where white ware specialists were drawing upon the same design traditions as the orange ware potters—hatched designs on Dogoszhi Black-on-white and Tusayan Black-on-red, and broad-lined designs on Sosi Black-on-white and Citadel Polychrome. It is only later that we see the different design traditions concentrated on the different wares. Perhaps during the Klethla Phase the specialization was in the realm of design, not ware, but this makes little sense in view of the later postulated specialization by ware. I would suggest that specialization within the potter's craft was not pronounced during Pueblo II, and only becomes an important aspect of the ceramic industry at the time particular design styles are largely confined to particular wares, starting with the Dzil Nez Phase.

Other Craftspeople

Beals, Brainerd, and Smith (1945:138) report that several adult females were richly furnished with awls, metates, and "other household utensils," and Ward (1975:30) reports one burial from Inscription House with eight awls and a large number of other offerings. The Anasazi are well known for their fine basketry, and a specialization in this craft seems indicated. Unfortunately, our information on basketry is much more limited than for pottery, so any extended discussion appears impossible at this time. It is worthy of note, however, that Adovasio and Gunn (1977) were able to distinguish the efforts of six or seven individual basket weavers at Antelope House.

Cloth weaving was another craft practiced by the Kayentans. It is commonly assumed that most weaving took place in the kivas, judging from the presence of loom anchor sockets commonly found therein. These kiva looms appear to have been broad, about 1.5 m. wide, and therefore used for the production of large cloth. It is difficult to envision that all men participated equally in this weaving, for some kivas have no loom anchor holes, and only rarely do we see more than one set. Perhaps, then, only some of the men actually wove in the kiva.

There is also strong evidence that weaving of narrow garments was a specialty of some, for a late Shonto Phase pithouse apparently devoted to this purpose was recently excavated at Dogtown in the Klethla Valley (Ambler and Andrews 1981). The loom holes in this structure are in pairs, 25 cm. apart. A perusal of the literature showed that similar structures exist at other nearby sites and as far north as Paiute Mesa. I would therefore suggest that weavers, too, were specialized during Pueblo III, some concentrating on wide garments and others on sashes and other narrow fabrics.
This, however, is about as far as specific evidence takes us. What of the remainder of the Kayenta population—were they all generalized consumers? I think not, for the presence of some specialists necessitates others, and I can envision many individuals spending considerable time making sandals, metates, chipped stone tools, or jewelry, others that spent more than their share of time conducting curing or communal ceremonies, and, of course, those hunters and farmers that produced more than their families could eat.

I would doubt that very few, if any, of the Kayenta individuals could be classed as full-time specialists. In the truest sense, full-time specialists are found only in completely stratified societies, and then only those people at the top of the hierarchy—ordinary people do their own cooking, house repairs, and other household and subsistence activities. However, it appears that many Kayentans were specialists to the degree that their time and skills allowed. Utilizing the burial data from RB568 as a rough guide, and employing a lot of guesswork, it would appear that more than half of the adult population were craft specialists. Three out of ten women may have been potters, two out of ten wove baskets, and several more probably had other things going. For the men, perhaps three out of ten may have been weavers, and two or three more may have been deeply involved in ceremonial activities. As is commonly assumed, every family probably had a plot of corn and other crops, but some probably produced surpluses that would have served as the basis for exchange.

**Implications and Speculations**

Going beyond the specialization by individuals, specialization at the community and local levels is easily postulated on logical grounds, but difficult to demonstrate because of our inadequate data base. A great deal of the territory occupied by the Kayenta Anasazi is characterized by a surficial geology of Navajo sandstone and aeolian sands, and many special resources are therefore localized. For example, riverine deposits of quartzite, chert, and other siliceous cobbles are largely concentrated along the San Juan and Colorado Rivers, although other siliceous materials, generally of poorer quality, are also found weathering out of the Chinle formation and also occur as occasional veins elsewhere. It would be expectable that those Kayentans close to supplies of good knapping stone would be trading both raw materials and finished artifacts to those at greater distances. Indeed, Turner and Cooley (1960) put forth a good argument that this was exactly the case, and conclude that material used for the chipped stone artifacts found in the Tsegi area comes, at least in large measure, from the Colorado and San Juan gravel deposits. Although Turner and Cooley note close correspondences between projectile points from the Tsegi and those found in the Glen Canyon region, they do not commit themselves concerning the question of whether it was the raw materials that were commonly imported to the Tsegi or the finished products. A cursory examination of stone tools and manufacturing debris from sites near Navajo Mountain, only a few kilometers from the San Juan, with those from the southern edge of the Shonto Plateau indicates that not only did the Navajo Mountain folk utilize stone more extensively, but that they had much better control of knapping techniques, with efficient biface reduction (Phil R. Geib, personal communication). The evidence therefore strongly suggests that the people in the Navajo Mountain area were specialists, exporting finished stone tools. A careful examination of collections and source materials is in order.

Pottery clays in the Kayenta area are confined to essentially two principal sources: the Mesa Verde formation as exposed along the northern edge of Black Mesa clays and hence was localized in the Klethla Valley-Marsh Pass region, since trade in unfired pottery is manifestly highly improbable. Large numbers along the western side of Monument Valley. Most of the Chinle clays are too indurated, silty, or alkaline for the manufacture of pottery, but some suitable ones are known. The clays associated with the Mesa Verde group are well known for their quality. Except to the west, on the Kaibito Plateau, clays of one sort or another would have been available to most Kayentans within less than ten miles of home. Since the different Kayenta wares are apparently made from different clays, it would seem likely that the sources for those clays may have been separated by some distance.

The presence of unfired polychrome vessels with burials near Black Mesa is a strong indication that Tsegi Orange Ware manufacture utilized the Black Mesa clays and hence was localized in the Klethla Valley-Marsh Pass region, since trade in unfired pottery is manifestly highly improbable. Large numbers of misfired (reduced) Tsegi Orange Ware sherds occur on Tsegi Phase sites in this area, hinting that mass production during the late 1200’s resulted in more poorly controlled firings. Perhaps, however, fuel supplies were running short, necessitating a change in firing methods that was not always successful. Misfired pots would make poor exports, and hence would be used at home.

If the center of manufacture of Tsegi Orange Ware was near Black Mesa, one might expect that the relative proportion of Tsegi Orange Ware compared to Tusayan White Ware would decrease as one progressed to the north away from the area of production, but this is not the case. Indeed, the Tsegi Orange Ware-Tusayan White Ware ratio at ten Pueblo III sites in the Navajo Mountain locality is approximately 2.5:1 (4564 vs. 1817 sherds) (Lindsay et al. 1968), whereas in the Tsegi it is closer to 2:1 for the ex-
Kayenta Craft Specialization . . .

The limited data available from other Pueblo III sites in the south (e.g., Ambler and Olson 1977; Ambler and Andrews 1981; Fairley 1981) indicates even smaller proportions of Tsegi Orange Ware in and near the Klethla Valley during the Shonto Phase. We are thus faced with the apparent dilemma that Tsegi Orange Ware is relatively less common in the area of its manufacture. The quandry is resolved, however, when we note that in the Tsegi, proportions of white, orange, and gray wares are roughly equal, whereas near Navajo Mountain the gray wares are two to three times as plentiful as the white and orange wares combined. The obvious conclusion is that both Tusayan White Ware and Tsegi Orange Ware were made from Black Mesa clays and traded northward. Tsegi Orange Ware appeared to be the more popular trade item, perhaps because of its striking color, perhaps because it was less costly, or perhaps because its more friable nature necessitated more frequent replacement. It would be logical to postulate that the gray wares were in turn made from Chinle clays and traded south. Much more information is obviously needed, but the suggestion is strong that pottery production was localized, and regular pottery exchange occurred between localities forty miles apart.

As for those Kayenta Anasazi not living in reasonable proximity to good clay sources, the manufacture of baskets, sandals, cloth, and stone tools, the procurement of pigment, animal products, and special lithic materials, and the production of agricultural surpluses would have been important for maintaining the balance of trade. In the heavily populated areas both game animals and wild plant foods were probably scarce, and providing meat to the iron-deficient diets of those individuals in the population centers could well have been an important specialty.

It would be interesting to speculate that certain communities within any one locality also specialized in the production of a limited range of goods and crafts (such as at Hopi), but if anything, the scanty evidence indicates otherwise. Both potters and basket-makers appear to have been present at RB568, and both weavers and potters appear to have been living at Dogtown (Ambler and Andrews 1981; Ambler 1981).

It is tempting to postulate that during Pueblo III in the Kayenta area certain kinds of specialists such as priests may have resided at the larger villages, whereas the people in the nearby hamlets concentrated their efforts at scrounging a living, hoping to produce a crop surplus and producing utilitarian items for the elite, a situation similar to that postulated for Chaco Canyon a century and a half earlier (Vivian 1970).

For the Kayentans, however, strong class distinctions do not appear to be highly probable. Settlement layout, village size, and architectural differences may simply be differences in the lifestyles between the larger ceremonial/market centers and the smaller villages, and do not necessarily indicate the presence of an elite class. Differences in burial accoutrements are sometimes striking, and perhaps a detailed study would indicate a consistent patterning indicative of class distinctions. Certain individuals appear to have had differential access to particular goods, but to assume that these people formed a distinct class is hazardous. Individuals with more “wealth” were undoubtedly of more than ordinary importance in the community, but may have been those with status ascribed as clan heads or religious society leaders, and were probably not part of a separate elite class.

Kayenta society may well have been ranked, but if so, I would suspect that the ranking was by clan, not by class. The inequalities evidenced in the burial patterns may therefore have been a reflection of clan inequalities as well as position within the clan. No matter how one looks at the evidence, however, it appears probable that there were specialists in religious and political activities that were at least in part supported by the craft and subsistence specialists. The ceremonial and political activities may indeed have been concentrated at the larger villages, which may in part explain the apparent higher proportions of Tsegi Orange Ware at those sites than at the small villages.

Assuming that at least some of the postulates presented above can eventually be shown to be highly probable, we can return to the question of when specialization began among the Kayenta. I think it probable that limited specialization occurred as early as Basketmaker II and even in the Archaic, for individuals often have particular skills, and the localized nature of some raw materials may have stimulated trade at a very early date. It does appear that by at least 1100 the main specialities we see in Pueblo III were well developed, and that the amount and degree of specialization increased during the succeeding two centuries. The genesis for the Pueblo III specialization must therefore be sought at least as early as Pueblo II.

The underlying causes leading to specialization may prove to be more difficult to define than the presence or timing of such. Arnold (1980) has pointed out that the traditional explanation for the development of craft specialization and the subsequent trade is based upon the unequal distribution of particular resources, but goes on to favor the postulate that individuals with an inadequate subsistence base would increasingly turn toward craft production as a means of making a living. However, it is noteworthy that his strongest ethnographic examples of craft specialization being correlated with land of inadequate quality and insufficient quantity for the local population come from societies with a background of millennia of specialization. I would submit that specialization leads to inadequate land supplies, not the reverse. Farmers, too, are specialists, and develop increasingly sophisticated techniques for raising crops. This in turn leads to larger yields for each skilled farmer, which can be
traded to those with craft skills. As food supplies increase, the population grows. Patterns become stabilized through succeeding generations, and individuals and their descendants become locked into their specialities, which are directly related to the availability of resources. This availability is not controlled solely by natural distributions but also by traditional rights and exchange networks.

Searching for a single cause for craft specialization is manifestly fraught with difficulty and probably a futile exercise. The positive and negative feedback systems involving distribution of resources, population growth, population movements, behavior patterns, religious ideas, social differentiation, exchange networks, and climatic changes, to name only a few of the obvious variables, all contribute to and are in turn reinforced by an increase in craft specialization. To postulate a single cause is equivalent to choosing sides in the chicken and egg argument.

If specialization was common among Kayentans, what was the mechanism of exchange? It is easy to assume that larger villages served as marketplace or trade centers, but no merchant class seems indicated and no formal markets have been identified. Reciprocity and barter were probably important, but the apparent regularity of exchange over long distances argues for a continuing exchange network not based solely upon personal relationships.

With a breakdown in relative agricultural productivity as a result of continuing population increase and climatic deterioration during the late 1200’s, the core of the exchange network may have been sapped, which would have reduced the survival rate of all but farmers, lessened demand for non-utilitarian craft items, and eventually resulted in the collapse or large scale migration of the entire system, a system that was only in its infancy at the end of Pueblo III.

Acknowledgements

During the course of the preparation of this paper, I received helpful comments from Michael J. Andrews, Helen Fairley, Phil R. Geib, John J. Wood, and Watson Smith, for which I am most appreciative.

Questions and Comments

Jim Mueller: Is there any evidence that certain women were making and shaping and forming the pots, and other women were painting, and other women were firing?

Ambler: Not that I know of. There is in the Shonto and Tsegi Phases a higher and higher proportion of unpainted Tusayan White Ware. What that means, I don’t know.

Question: What you say sounds so logical, but I’m puzzled by the lack of data. If someone is specialized, does not that mean, inherently, that their stuff is better? And then, shouldn’t we be able to identify that, just as today you can tell Blue Corn’s pottery from Maria’s or somebody else’s? Do you or anyone else here know of any assemblage of pots we can say, “Ah, there was an individual who did these six pots?” If we can identify that today, could we then—and that would seem to be data for you—and if it doesn’t exist, it’s really got to be explained.

Ambler: Do you have an answer to that, Joe?

Joe Ben Wheat: Yes, I do. That’s what I was going to talk about anyway. It is a commonplace that you can take a pot into Hopi, or Zuni, or wherever, and all the women will know who made that pot. So, we reasoned that, found that, you could identify individual potters in an archeological assemblage if you had a controlled enough sample. And that sample came from Kawaikuh, and one of our graduate students, Hannah Hughes, whom I’m sure you know, went under the assumption that it might be possible to identify individual potters in an archeological assemblage. This assemblage covered about 200 years and came from a number of graves dug by Earl Morris. We held it provenience-free so that she analyzed the pottery simply on its intrinsic merits. She came up with a number of individual traits such as the color of the slip, how it was fired, polishing techniques, variations in vessel shapes, certain designs and the way the designs were applied, and in many cases almost a key signature occurring as an individual motif on the outside of the pot. In these particular cases she was able to come up with somewhere around 26 individual potters that she thought had made this group of 250 pots. Then we gave her the provenience data, and these things held together at somewhere around 90 percent. The grave lots came from particular potters. That’s available, I think, through the Ann Arbor microfilm thing—that was her doctoral dissertation. I think that is an excellent example not only of what you were talking about, about individual specialists and the recognition of individual potters, but also it seems to me to be an indication that many of the collections that were made years ago, under less than ideal conditions perhaps, are today still usable for particular kinds of anthropological studies.

Ambler: I think that on the very subjective level—and Lex, would you agree—that you can look at certain pots and say, “These really look alike; they look like they were made by the same person.” But I don’t think anybody has really tried too hard yet to really pin it down, something like Hannah did.
**Question:** You said something about going 50 miles for stone, the stoneworkers. Could you say a little bit more about that, about why they would go so far to obtain their stone?

**Ambler:** Mostly because you don’t have any good rocks down here. Your best chert gravels are along the San Juan and along the Colorado. You have some cherts over by Tuba City.

**Question:** Is it mainly for clays and like that?

**Ambler:** Yes, I would guess that. Turner and Cooley sort of suggest that it was the finished products that ended up down here, not the raw materials. In other words, these folks were not going up and bringing it back, but these folks up here were making it and then trading it not as projectile points but junky, ordinary stuff—choppers, scrapers, those sorts of things. There are some outcrops in the Chinle Formation that are silicious enough to make sort of cruddy artifacts out of, and it may be that they gathered that locally; but the better quality materials are only found up farther north or, if you went way south, on the Little Colorado too.

**Question:** But it was mainly cherts that they were looking for?

**Ambler:** Yes. There was probably some fairly widespread trade in things for ground stone artifacts as well—mostly metates, say, and manos. In a lot of this country we are talking about the surficial geology is Navajo sandstone which is useful for building blocks, but that’s about it. When you start grinding your corn on it, you end up with a sand gruel instead of corn meal.

**Ted Frisbie:** You mentioned the 50 mile radius. I did a mineral study at Zuni, discussing the acquisition of minerals today, going to where they are still going for their traditional minerals. I wanted to find out how far out they are going to the traditional places where they say they have always gone to get minerals. What it turns out as is 50 mile radius. They go just about 50 miles out, all around the pueblo, to get things that they need. Most of these are paint sources, but other things as well. This seems to be the old pattern: 50 miles out would be where they would traditionally go, no farther than that. Otherwise they would have stuff traded in.

**Ambler:** What is your time range on this?

**Frisbie:** All I can tell you is that they say they go where they used to go in the old days, and they continue to go back as long as the sources are there. They also are always looking for new sources that may be closer.

**Ambler:** Of course, you are running into your same problem here of ethnographic analogy—“Back in the old days . . .”

**Frisbie:** When they go to Zuni heaven, when they go to the sacred lake in the west, 45 miles barefooted, they also pick up limonite (yellow ochre). And they have done this ever since they have been doing that, supposedly. They have other places where red ochre, azurite, malachite—and at least back to Cushing’s day, apparently the same places.
IS MESA VERDE A PREHISTORIC CULTURAL ANOMALY?

David A. Breternitz

Obviously, this paper is misnamed. What I am going to do is essentially what Dick Ambler did for the Kayenta; that is, you can’t characterize the Kayenta, or the Mesa Verde in this case, in the manner that has been done in the past. Really what I want to do is to discuss the Mesa Verde Region through time. Not all districts in the Mesa Verde Region, and here I am using “district” as defined by Lehmer in his 1971 monograph, have duplicated cultural histories. Within the Mesa Verde Region there are districts, and localities, that have different cultural sequences. I want to point out that what was done in the past, and what Jack Smith and I did so excellently a few years ago, is probably wrong.

The Mesa Verde Region as currently defined in the literature, and as it has been defined since Kidder (1924), as well as in Wormington (1947), and the Mesa Verde pottery volume (Breternitz et al. 1974), is basically the maximum extent of Pueblo II habitation sites in the northern San Juan area. Thus, the Mesa Verde Region is defined as extending from the Colorado River on the west, east to the Animas River, and from about the San Juan River north as far as you can grow corn, about a line between Dove Creek and Monticello. But this boundary does not reflect the situation for all periods of time within that area. The boundaries of the Mesa Verde Region are dynamic through time and it is a mistake to define the Mesa Verde Region through time using static boundaries.

At certain times within the Region people were not inhabiting certain localities or districts while others were flourishing in their own individual ways.

As Joe Ben Wheat said earlier this morning, what we have here on the Mesa Verde proper is somewhat different because of its physiography, and so I will go on somewhat along that tack. The earliest dated Anasazi habitations on the Mesa Verde date back to AD 575, and the latest Anasazi construction dates are in the 1280’s. Consequently, the literature says that the Anasazi occupied the Mesa Verde proper from AD 550 to 1300, or to put it simply, there is a sequence from Basketmaker III through Pueblo III and then they all went somewhere else. This sequence is displayed in the museum, you can read it in the literature, and it has been accepted as typical of the cultural history of the Mesa Verde Region. What I am saying is that the Mesa Verde proper may be an anomaly because we do not get this same picture in other parts of the Mesa Verde Region, which translates to the fact that we do not get the same story on the Mesa Verde that we get in other districts/localities of the Region.

Let me back up to the Archaic. There are some lithic scatters on the Mesa Verde (and at last count there are 3895, archaeological entities recorded within Mesa Verde National Park; there are no undiscovered ruins within the Park boundaries). None of these lithic scatters have been definitely identified as
Archaic. Reexamination might identify Archaic components from some of these sites but they might just as well be aceramic Anasazi sites, or sites representing limited use from a variety of time periods. There are some isolated “Archaic” projectile points recorded from the Mesa Verde which are usually found on Pueblo habitation sites and probably represent collectables (some Anasazi-type found the item when out hunting, gathering, or collecting and brought it home to show the family, to the tune of “Look at this neat thing I found,” and his spouse said “Throw it away, there is enough junk around here now.”)

There are also no definitely identified Basketmaker II sites within the Park boundaries. At one time Bob Lister sent a group of lads out to specifically search for Basketmaker II sites; they were unsuccessful in their efforts. Art Rohn (1977) also excavated a site that he thought might be Basketmaker II, but he came to no definite conclusions.

Consequently, the cultural sequence we have within the Park is Anasazi and extends from AD 575 through Pueblo III, or about AD 1300. Within the park there is a dynamic situation as shown by the fact that about AD 1150 the upper portion of the mesa top, specifically the Far View locality, is abandoned and the population concentrates in the lower portion of the mesa where we have the late cliff dwellings, or even into Johnson Canyon where we have a building period in the early 1200’s (Nickens 1977). There was site relocation on the mesa top through the total time period of Anasazi occupation.

Let’s look back. The Durango District has been included in the Mesa Verde Region and is best known from the Basketmaker II sites excavated by Morris and Burgh (1954). From the AD 200’s we have a continuous occupation in the Durango District until AD 850, at which time that portion of the world, including Ridges Basin, is abandoned by the Anasazi. This is not the same sequence already noted for the Mesa Verde.

In the Dolores Project area we have been working primarily within the Dolores River Valley. Here we have evidence of Archaic occupation, probably seasonal utilization, beginning about 5000 B.C. We have some Archaic-looking lithic scatters that look like periodically occupied sites that may even be associated with some sort of Archaic “transect” — Archaic peoples may have utilized that area from the San Juan River to the higher country north and east of the Dolores Project, and consequently passed through the Dolores Project area while going from one end of the transect to the other. We do not have any Archaic sites with depth, but there is evidence that they used the local resources, built a few fires, broke a few rocks, and did whatever Archaic people were supposed to do when moving from one area to another trying to scrounge a living. Presently, in the areas where we have worked, we believe we have an Archaic lifestyle represented from about 5000 B.C. until about A.D. 600.

Beginning about A.D. 600 we have honest-to-goodness Anasazi, i.e. Basketmaker III. This is followed by a florescence of Pueblo I hamlets and villages that constitute real community clusters. The population climax is reached between A.D. 850 and 900 in the Dolores River Valley, at which time we estimate that approximately 2,300 persons were living in and around the Dolores Valley, and concentrated in at least nine large Pueblo I villages. By A.D. 950 the Dolores Valley is abandoned except for a few later farm house sites attributed to the 1000’s and 1100’s.

Joe Ben Wheat has been working near Yellowjacket since about three days after the Anasazi left. Around Yellowjacket Joe Ben has Basketmaker III, no Pueblo I occupation, and then a proliferation of Pueblo II and early Pueblo III habitations. Apparently there is no Pueblo I because they were all down in the Dolores River Valley, and when they left that territory they moved back up to the upper reaches of the drainages that are tributary to the McElmo and San Juan. What Joe Ben has been telling us for years at these conferences appears to be true—certain Basketmaker III peoples moved into the Dolores River Valley, Pueblo I proliferated in the valley, and then for some reason, about A.D. 950, they returned back to the areas which we characterize today as the “beanfields.”

Hovenweep has cultural materials beginning in the Archaic, and then there is an influx of population late in the Anasazi sequence that concentrates in both small and large sites around the heads of canyons, usually in close proximity to water sources, i.e. springs.

So far, we have seen that the cultural sequence from the Mesa Verde, the Durango District, the Dolores River Valley, the Yellowjacket vicinity and Hovenweep are all somewhat different.

Another question, as an aside, which arises from time to time, concerns so-called Chaco outliers in the Mesa Verde Region. As far as I know, there is nothing even remotely resembling a Chaco outlier on the Mesa Verde proper. We do have what have been characterized as Chaco outliers at Lowry, Ida Jean, the Wallace Ruin, the Escalante Ruin, and perhaps Yucca House, although in this latter instance no adequate excavation has been undertaken to date. Thus, there are hints of Chaco outliers north of the San Juan River, but there is no evidence of them occurring on the Mesa Verde proper.

Mesa Verde National Park has been used, on the basis of the cultural sequence, the architecture, and the ceramic composition, as the standard for what happened prehistorically in the Mesa Verde Region. There are logical reasons for the circumstance—Mesa Verde National Park was protected early (it became a National Park in 1906) and through excavations to develop and interpret the Park this is where archae-
Mesa Verde National Park is at the eastern end of the Mesa Verde Region; I think the center of the Mesa Verde Region is to the west, from perhaps about the Montezuma Valley-Goodman Point area on west into southeastern Utah. I am not sure you can identify the Mesa Verde Region as a cultural entity (if such does exist) until after A.D. 850, as Joe Ben Wheat has indicated earlier today. And even then there is no way the Mesa Verde Region can be extended eastward past the La Plata River, even though several recent publications (including Breternitz et al. 1974) show the Mesa Verde Region extending to the Animas River—that is wrong! (see Morris 1939).

What I am saying is that none of the districts within the Mesa Verde Region are “anomalies”—each has its own cultural sequence and history, as seen by our present knowledge of Mesa Verde National Park, the Dolores Project area, the Durango District, Hovenweep, Yellowjacket (which have been cited as examples), as well as our knowledge of southeastern Utah (e.g. Alkali Ridge, Cedar Mesa work of W.D. Lipe, recent work by Plano Archaeological Consultants, and various Utah BLM operations and reports). At the present time, we are able to recognize these local differences in human adaptation, and begin to forge an overall cultural reconstruction of the Mesa Verde Region. We are beginning to recognize local/district differences in ceramics, lithic sources, inter-and intra-regional exchange networks, and architectural variations. Among other things that are appearing is the recognition that within the Dolores Project contemporary villages apparently had affiliations with differing areas, i.e. one village has the vast majority of lead-arsenic glaze pottery from the Durango District, another shows that the majority of its trade ceramics comes from the San Juan River area and south, and another has more Kayenta pottery than any other contemporary site. Bill Liciu and Dick Ambler have both pointed out these situations in their respective papers. These connections could be economic, ethnic, political, social, ceremonial, something else, or a combination of these elements or other elements. Hopefully, some answers will be forthcoming.

It boils down to the situation that if we are able to evaluate materials from the Mesa Verde Region at the same level of analysis, we will be able to probably say that every locality and district in the Mesa Verde Region is an anomaly when compared to all the others. The prospects are exciting because it will enable us to eventually determine local sequences, relationships, and perhaps the underlying reasons for these cultural variances. In other words, Mesa Verde National Park is not in itself a cultural anomaly, nor are other culturally recognizable entities within the Mesa Verde Region, but rather, each entity has its individual characteristics which will permit us to eventually reconstruct both local and regional cultural histories.

Let us not blame the history of archaeological research in the Mesa Verde Region for this situation, but rather use the story of research to assist in making the next step to understanding the intricacies of the cultural past in a complicated and fascinating area.

Questions and Comments

Curt Schaafsma: I worked with Al up here in 1959 and I’m very much aware of the lack of Archaic material up here, and that does remain one of the peculiar things in the Southwest. The only thing is that I’ve never seen anyone try to put Arrhenius’ (the Swedish soil scientist who was working here in the 1960’s) I’ve never seen anyone put his results into practice relative to trying to explain this type of thing because his basic early meter deep Mesa Verde loess tested on the order of 2500 BC, as I recall. I don’t know what this means, right out of hand, but if his thesis of the “rain of loess” is correct, and that a meter deep loess deposit is on the mesa, there is obviously an indication that some of the early Archaic base could be here, underneath the loess. Has anyone tried to pull that together?

Dave Breternitz: As far as I know, not. I don’t think Bob Heyder is going to let us take a blade and take the top meter off the top of the mesa to look for that stuff. There have been a lot of borrow pits, pipeline ditches, water lines, sewer lines, and other things dug across here, and I think, at least recently, that all of those things have been checked; I know Al Lancaster used to check them back in the 1930’s. And as far as I know we have never been fortunate enough to find anything of that type buried, not that they are not there.

Schaafsma: The chances of finding them are just about zero in terms of randomly digging a ditch and finding them. But if his paper, in the series in American Antiquity, is accurate, then we may be looking at some kind of physical geological explanation for at least some of the reasons we don’t find Archaic sites up here.

Ted Frisbie: For Basketmaker II, you say there is none at Mesa Verde. Wouldn’t it be likely that under the cliff dwellings proper, there might be such remains but we can’t get to them? Since they seem to be the best case for occupation during later times, it’s a little
Is 'Mesa Verde' a Prehistoric Cultural Anomaly?

hard to get down underneath and see what is there. Have tests been done to actually check it out?

**Breternitz:** We know people lived in the caves prior to Basketmaker III; and in some cases there is pretty good evidence that they had urban renewal, and that when the final occupation came in that they cleaned out the earlier stuff, used what available rock and timber they could, and threw the rest out in the trash. Tests in that trash have found earlier materials, and in some cases, like at Long House, there is a Basketmaker III house underneath some of the remains that were not cleared out. But the Park Service simply is not going to let us clean out Cliff Palace to see if there is any Basketmaker II down there. It is true that simply because it hasn’t been located doesn’t mean that it isn’t there, but there has been a lot of looking around up here.

**Art Rohn:** One comment along that line might help to answer your question. Jesse Nusbaum spent a number of years specifically looking for Basketmaker II remains, behind Spruce Tree House, in a cave in Fewkes Canyon, and at other places of this sort. He was stimulated to dig at Step House by the idea of earlier materials than the cliff dwellings. There have been a few attempts, although granted it's difficult to see what is underneath one of those ruins when they are still standing.
MESA VERDE ANASAZI ROCK ART: A VISUAL COMMUNICATION SYSTEM?

Nancy H. Olsen

Introduction

During the last decade Southwestern archeology has addressed many topics concerned with the cultural complexities in Anasazi communities. One question raised while pondering such complicated systems concerns the lack of writing. How did groups of people such as the Anasazi document decisions of authority? Who designated access to economic goods, water for fields, or land use? How were these decisions recorded in order to maintain that organization successfully? Those who are familiar with Western Pueblo ethnography are well acquainted with the oral tradition of clans, societies, and villages. All there is to know about being Hopi or Zuni is contained there. Anthropology has learned much about social organizations, world view and economic processes through that oral tradition. Our answer lies there in part.

By applying anthropological views to archeology (Longacre 1970), Southwestern archeologists have begun to reconstruct social organizations and search for evidence of economic adaptation to the semi-arid environment. Through the prehistoric economic systems and possible social order seen in the archeological data, rock art placed in context indicates further behavioral patterns. This paper offers a new set of data concerning the use of Anasazi motifs, not only painted and incised on boulders and rock shelters but also seen on ceramics, textiles, baskets, building stones, kiva plasters and so forth, that may denote a method of documentation. When traditional archeological typologies are cut across in search of single motif locations, it becomes evident that Anasazi graphic signs may be a visual communication system.

Theoretical Background

Three anthropological premises form the basis of the proposition that the Anasazi at Mesa Verde and surrounding country may have used a graphic system. The first premise, earlier observed by Mallory (1886), and more thoroughly developed by Levi-Strauss (1955, 1962), in Charbonnier (1969), and by Munn (1971, 1973) establishes that all cultures produce a means of expressing and ordering experience visually. Tables 1 and 2 illustrate the graphic system traditional to the Western Pueblos; it supports the oral tradition of the Hopis and Zunis mnemonically and their social organization through redundancy (Olsen 1980). The ethnographic record of rock art use (Cushing 1920; Colton 1960; Beaglehole 1937; Fewkes 1892, 1894, 1897a, 1897b, 1906; Forde 1931; Parsons 1936; Stevenson 1905; Titiev 1937, 1944) illustrates the fact that neither were the rock art images made randomly, nor are they unique to that category. Hopi and Zuni rock
art motifs appear on many other cultural forms for specific reasons. Strong evidence exists, therefore, that historic Hopi and Zuni pictographs and petroglyphs are used semantically, within contextual boundaries dictated by custom.

Since the Anasazi are thought to be the ancestors of the present Pueblo people (Martin & Plog 1973), it is logical to propose that rock art associated with those Anasazi sites in the Mesa Verde area may represent a graphic system analogous to the ethnographic one of the Western Pueblos.

The second premise states that cultures, literate and non-literate alike, use graphic systems to identify social groups (Levi-Strauss 1955, 1962; Munn 1973), political statusues (Levi-Strauss 1955; Griaule and Dieterlen 1954; Malin 1978), religious concepts (Parsons 1939; Colton 1949; Geertz 1970; Munn 1971), and economic values (Fischer 1971; Bunzel 1929). Ethnographically, the Western Pueblo representational system also supports that premise. Table 1 illustrates clan symbols used in religious and secular locations to identify ownership, ceremonial participation, and identity. Table 2 sets forth motifs which are used in the representation of clan migration myths, religious concepts, land boundaries, political statuses, argument resolutions, historical alliances and secular authority. Thus, since the documented signs are limited in their cultural context by their semantic use, an analogous distribution of image type may be observable in the archeological data as well.

A third premise states that if visual symbols are an alternate means of communication, and are culturally selected, they will display syntactic order which reflects a deep structure of the culture’s collective mind (Colby 1975; Whorf 1975; Glassie 1975; Deetz 1977). Voegelin and Voegelin (1961), Munn (1971), and Washburn (1977) have devised methods for observing and analysing graphic systems that define syntactic strategies. If the relationship of the images to the culture is communicative, then we can apply semiotic analysis to visual systems just as a linguist scrutinizes a verbal communication system.

A semiotic approach to prehistoric rock art provides two advantages. First, a semiotic re-definition of rock art assumes that it is a subsystem of the cultural process, a visual extension of the oral tradition. Second, it allows us to ask questions about prehistoric rock art that can be tested by observable data.

Semanticism of Pueblo images is observable in two ways. Alexander Stephen’s journal (Parsons 1936) details each discrete symbol, its color and/or meaning. This practice, through a thousand pages of notes, several seasonal cycles, and dozens of informants, shows that there is a common use of graphic signs whose meaning is known to all. Second, Stephen’s journal documents the Hopi’s consistent use of symbols to indicate the same concept in different places (such as the rain cloud, rainbow, sun and lightning). Hence the semicircular rain cloud with dependent lines can be seen, for example, in rock art, kachina masks, clan symbols, altar figures, kilt decorations, in corn meal effigies of the Flute Ceremony, and corn meal signs made on the path by the War Society for the purification of returning warriors.

All through the interrelated contexts of social function, economic ownership, illustration of myths and clan origins, or religious concepts, a unifying strategy for organizing symbols suggests that a structural manipulation of signs exists in the collective cultural mind. Just as kinship terms are a manifestation of social structure (Eggan 1950), so use of signs in specific contexts manifests the conceptual processes by which the pragmatic world is joined to the intangible cognitive universe in an ordered set of relationships. An analogous set of relationships should be observable quantitatively in Anasazi motifs if the same use of graphic signs was in operation. Provenience of motifs identifies the type of context in which the semantic choice is made, but no single “translation” can be made of any sign prehistorically (Hopi Cultural Center 1981).

Data and Testing

A research project designed to investigate the agricultural adaptations of the Anasazi to Cajon Mesa furnished an opportunity to test the three propositions and look for patterns of behavior in the archeological record. Three summers of survey and excavation in Hovenweep National Monument and surrounding three thousand acres (Winter 1975, 1976, 1977) located 714 sites; fourteen contained rock art. Other rock art locations were photographed and studied (such as Montezuma Creek and Cross Canyon) but were not included in the Hovenweep study since it was felt that the rock art sample should parallel the statistical sampling of the research project. Further, the archeological information from the research sites supplied contextual information such as fossil pollen, dendrochronological dates, carbon-14 dates, sherd and lithic patterns of use, faunal presence, lithic patterns of use, and environmental data which was not available in those other places. That site information gives necessary information about context for understanding the patterned placement of rock art.

All of the rock art was found in association with other site features. None occurred outside of a site context or alone, as is sometimes suggested in the literature (Dalley 1972; Castleton 1978; Fewkes 1919; Rohn 1977). All of the rock art was judged to be of Anasazi origin since it was intimately and consistently connected with Anasazi habitation, probable farm fields, or water control sites. Nine of the fourteen Hovenweep rock art sites clustered around mid-mesa canyon heads in Pueblo II and Pueblo III complexes (ca. A.D. 900–1100 and ca. A.D. 1100–1300; Winter 1975). Comparison of Hovenweep rock art with graphic elements on other cultural materials showed
<table>
<thead>
<tr>
<th>Clan Symbol</th>
<th>FEWKES 1897</th>
<th>CACTUS CLAN</th>
<th>FEWKES 1897 ANTELOPE CHIEF PRIEST</th>
</tr>
</thead>
<tbody>
<tr>
<td>Deer Clan</td>
<td>FEWKES 1897</td>
<td>CACTUS</td>
<td>FEWKES 1897 ANTELOPE CHIEF PRIEST</td>
</tr>
<tr>
<td>Horn Clan</td>
<td>COLTON 1960</td>
<td>DEER CLAN</td>
<td>FEWKES 1897</td>
</tr>
<tr>
<td>Mountain Sheep Clan</td>
<td>MALLORY 1886</td>
<td>HORN CLAN</td>
<td>DOVE CLAN</td>
</tr>
<tr>
<td>Antelope Chief</td>
<td>STEPHEN 1936</td>
<td>HORN CLAN</td>
<td>FEWKES 1897</td>
</tr>
<tr>
<td>Tadpole Clan</td>
<td>MALLORY 1886</td>
<td>MOUNTAIN SHEEP CLAN</td>
<td>FEWKES 1897</td>
</tr>
<tr>
<td>Raincloud Clan</td>
<td>FEWKES 1897</td>
<td>REED CLAN</td>
<td>FEWKES 1897</td>
</tr>
<tr>
<td>Raincloud Clan</td>
<td>COLTON 1960</td>
<td>EAGLE CLAN</td>
<td>FEWKES 1897</td>
</tr>
<tr>
<td>Raincloud Clan</td>
<td>FEWKES 1897</td>
<td>SUN CLAN</td>
<td>FEWKES 1897</td>
</tr>
</tbody>
</table>

Note: a, b, c, d represent different slant species.
<table>
<thead>
<tr>
<th>RABBIT CLAN</th>
<th>COYOTE CLAN</th>
</tr>
</thead>
<tbody>
<tr>
<td>b. Rabbit track</td>
<td>a, b, MALLORY 1886</td>
</tr>
<tr>
<td></td>
<td>c, d, COLTON 1960</td>
</tr>
<tr>
<td>RABBIT CLAN</td>
<td>MOON CLAN (extinct)</td>
</tr>
<tr>
<td></td>
<td>COLTON 1960</td>
</tr>
<tr>
<td>BADGER CLAN</td>
<td>SPIDER CLAN</td>
</tr>
<tr>
<td>a, b, c, d, e, f, g</td>
<td>COLTON 1960</td>
</tr>
<tr>
<td>BEAR CLAN</td>
<td>RED ANT CLAN</td>
</tr>
<tr>
<td>a, b, c</td>
<td>COLTON 1960</td>
</tr>
<tr>
<td>BEAR-STRAP CLAN</td>
<td>BUTTERFLY CLAN</td>
</tr>
<tr>
<td>a, b, c</td>
<td>a, b, c COLTON 1960</td>
</tr>
<tr>
<td>LIZARD CLAN</td>
<td>a, b, c, MALLORY 1886</td>
</tr>
<tr>
<td>a, b, c, d</td>
<td>b, c COLTON 1960</td>
</tr>
<tr>
<td>HORNED TOAD CLAN</td>
<td>BLUEBIRD CLAN</td>
</tr>
<tr>
<td>a, b, c, d, e, f, g, i</td>
<td>a, b, c, d, e, f, g, i</td>
</tr>
<tr>
<td>PUMA CLAN</td>
<td>PARROT CLAN</td>
</tr>
<tr>
<td>a, b, c</td>
<td>a, b COLTON 1960</td>
</tr>
<tr>
<td>SAND CLAN</td>
<td>CRANE/HERON CLAN</td>
</tr>
<tr>
<td>a, b, c, d, e, f</td>
<td>a, b, c, d, e, f</td>
</tr>
<tr>
<td>EARTH CLAN</td>
<td>FEWKES 1897</td>
</tr>
<tr>
<td>TURKEY CLAN</td>
<td>MALLORY 1886</td>
</tr>
<tr>
<td>-------------</td>
<td>--------------</td>
</tr>
<tr>
<td>BITTENRNS (wading birds)</td>
<td>MALLORY 1886</td>
</tr>
<tr>
<td>Unidentified bird tracks</td>
<td>MALLORY 1886</td>
</tr>
<tr>
<td>PIGEION-HAWK CLAN</td>
<td>FEWKES 1897</td>
</tr>
<tr>
<td>GOPHER CLAN</td>
<td>FEWKES 1897</td>
</tr>
<tr>
<td>RABBIT-STICK CLAN</td>
<td>FEWKES 1897</td>
</tr>
<tr>
<td>RABBIT BUSH CLAN</td>
<td>COLTON 1960</td>
</tr>
<tr>
<td>TOBACCO CLAN</td>
<td>FEWKES 1897</td>
</tr>
<tr>
<td></td>
<td>a. Hano</td>
</tr>
<tr>
<td></td>
<td>b. Oraibi</td>
</tr>
<tr>
<td>Symbol Group</td>
<td>Interpretation</td>
</tr>
<tr>
<td>--------------</td>
<td>---------------</td>
</tr>
<tr>
<td>I</td>
<td>Body paint mark</td>
</tr>
<tr>
<td></td>
<td>Tally or count</td>
</tr>
<tr>
<td></td>
<td>Eyes</td>
</tr>
<tr>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>Border markers</td>
</tr>
<tr>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>Eyes</td>
</tr>
<tr>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
</tr>
<tr>
<td>II</td>
<td>“Warrior marks”</td>
</tr>
<tr>
<td></td>
<td>Frog tracks/ Rabbit eyes</td>
</tr>
<tr>
<td></td>
<td>Bottom of Kachina Kilt</td>
</tr>
<tr>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>Boundary Markers</td>
</tr>
<tr>
<td></td>
<td></td>
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<tr>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>IV Snow Rain Star Mountains Year Count</td>
</tr>
<tr>
<td></td>
<td>Rabbit track Rabbit Clan</td>
</tr>
<tr>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>IX Morning Star Star</td>
</tr>
<tr>
<td>Symbol Group Interpretation</td>
<td>Location</td>
</tr>
<tr>
<td>----------------------------</td>
<td>----------</td>
</tr>
<tr>
<td>Sky Symbol</td>
<td>War Chief's house Flute Society Altar</td>
</tr>
<tr>
<td>i</td>
<td>Pesrum Kachina figurine, Goat Kiva</td>
</tr>
<tr>
<td>i</td>
<td>Zuni Altar</td>
</tr>
<tr>
<td>i</td>
<td>Broad Stick</td>
</tr>
<tr>
<td>Antelope prints</td>
<td>Praverstick Horn Kiva Winter Solstice</td>
</tr>
<tr>
<td>XI</td>
<td>Masks from Horn Kiva</td>
</tr>
<tr>
<td>Crow Clan Snipe track Mouth Duck Track</td>
<td>Morning Kachina</td>
</tr>
<tr>
<td>Crane Track</td>
<td>Nata’shka father Mask</td>
</tr>
<tr>
<td>Crane Track</td>
<td>Pottolukon</td>
</tr>
<tr>
<td>XII</td>
<td>Begun on rising of Morning Star 3AM meal</td>
</tr>
<tr>
<td>Lightning Sticks</td>
<td>YayatQ red/yellow</td>
</tr>
<tr>
<td>Symbol Group Interpretation</td>
<td>Location</td>
</tr>
<tr>
<td>----------------------------</td>
<td>----------</td>
</tr>
<tr>
<td>Medicine Bowl Zuni</td>
<td>Stevenson 1905</td>
</tr>
<tr>
<td>Water jar for Snakes, Snake Ceremony</td>
<td>Stephen 1936</td>
</tr>
<tr>
<td>Nōvā‘kchina Kachina Mask</td>
<td>Stevenson 1905</td>
</tr>
<tr>
<td>Mask of Shumai‘koli of the Zenith Zuni</td>
<td>Stevenson 1905</td>
</tr>
<tr>
<td>Lightning</td>
<td>Hopi Zuni</td>
</tr>
<tr>
<td>Lightning Stick</td>
<td>Held by Mulyi‘wa Lalakon Altar</td>
</tr>
<tr>
<td>Snake</td>
<td>Altar Antelope Kiva Snake Ceremony</td>
</tr>
<tr>
<td>Snake</td>
<td>Altar Image Horn Society Horn Kiva</td>
</tr>
<tr>
<td>Plant Stick</td>
<td>Buffalo Dance Buffalo Girl</td>
</tr>
<tr>
<td>Tree</td>
<td>Nashabki, Kiva</td>
</tr>
<tr>
<td>Symbol</td>
<td>Group</td>
</tr>
<tr>
<td>--------</td>
<td>-------</td>
</tr>
<tr>
<td>XXVI</td>
<td>PalOlukona</td>
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</tr>
<tr>
<td>Symbol Group</td>
<td>Interpretation</td>
</tr>
<tr>
<td>--------------</td>
<td>---------------</td>
</tr>
<tr>
<td>Meal Altar</td>
<td>All Corn Colors</td>
</tr>
<tr>
<td>Body Paint</td>
<td>Kachinas</td>
</tr>
<tr>
<td>Rock Art</td>
<td>on trail to Sun Spring</td>
</tr>
<tr>
<td>III</td>
<td>All Hopi Farm land</td>
</tr>
<tr>
<td>Eyes Teeth</td>
<td>Kachina masks Broadfaced Kachina</td>
</tr>
<tr>
<td>Eyes</td>
<td>woven into belt</td>
</tr>
<tr>
<td>Shrine</td>
<td>Sityatki Ceramics</td>
</tr>
<tr>
<td>Bean Sprout</td>
<td>eye of Morning Kachina</td>
</tr>
<tr>
<td>Plantingstick Prayer Stick Altar Crook</td>
<td>On &quot;planting stick&quot; staff Winter Solstice</td>
</tr>
<tr>
<td>Cloud</td>
<td>Antelope Prayer Stick Horn Kiva</td>
</tr>
<tr>
<td>Cloud</td>
<td>Stephen 1936</td>
</tr>
</tbody>
</table>
an overlap of sign presence of 37%. Identical motifs were found on ceramics from Mesa Verde, Alkali Ridge, Lowry Ruin, the Piedra District, and Hovenweep; buildings stones, basketry, and woven sandals from Chapin and Wetherill Mesa carried more duplicate motifs.

Repetition of images observed in the same location, remade, in several places, in pairs and/or with identical site features demonstrated that relationships between graphic signs and users, the site context, and each other were more complex and non-random than expected (Olsen 1980). Frequency variables showed local use of certain elements while others were distributed across space in a homogeneous manner, suggesting restricted use on the one hand and universal use on the other.

Mutual chronological use of symbols on objects and rock art was not often present. The majority of motifs shared by both rock art and ceramic categories, for example, appeared early in Chapin and Piedra Black-on-White ceramics throughout the district. By Pueblo II and Pueblo III phases when the rock art was presumably made in association with the Hovenweep sites, the ceramic designs changed; the former ceramic motifs shifted to petroglyphs. This change of location from pottery to petroglyphs through time suggests that the cognitive network used by Anasazi may have existed since Basketmaker III or earlier and that a developmental sequence appears to have taken place.

Conclusions from the Hovenweep project could only be suggestive since the data base was limited by the sparing use of rock art by the Anasazi. In the interest of developing a broader data base and testing the first proposition concerning the presence of a semantic system of signs, a survey of Anasazi motifs and locations was initiated this summer.

Results showed a range of motifs with location limits of twenty-five. They are as follows: petroglyphs, pictographs, ceramic pitchers, kiva jars, lids, seed jars, water ollas, mugs, dippers, bowls, effigy figures, “gaming pieces,” bone tubes, corrugated ware, pendants (ceramic and stone), woven cloth, sandals, cradleboards, coiled baskets, kiva plasters, room walls, building stones and doorway shapes. The motifs are classified by discrete formal structure into geometrical, curvilinear, zoomorphic and anthropomorphic classes. The sources for the survey were documents and artifacts available for study from Mesa Verde (Hayes 1964; Hayes & Lancaster 1975; Cattanach 1980; Rohn 1971, 1977; Nordenskiold 1893; Scott 1979; Swannach 1969; Fetterman 1976; MVRC 1951, 1954, 1959, 1960, 1966), reconstructed ceramics from the Dolores Archeological Project, ceramics in the Chappell Collection, ceramic sherds from the Mockingbird Mesa Bureau of Land Management survey, Lowry Ruin, and Hovenweep (Washburn 1975, 1976). This preliminary survey of motifs was limited by time and should not be construed as a definitive study, but rather as a test probe to determine the feasibility of the proposition.

When the population of graphic signs (500+) was plotted out, location vs. graphic element, relationships appeared to be significant enough that a statistical Chi-square test was designed to see if there was a mathematical basis for the relationships.

The test was put in the form of a null hypothesis which states that there is no relationship between the elements and their locations. The alternative hypothesis states that there will be a relationship between the motifs and places where they are observed.

Table 3 is the Contingency Table that identifies relationships, or lack of them, that might occur. O is the number of locations where a particular element was observed, E is the number expected to be seen. The table condenses the location categories from 25 to 7, grouping the subclasses all under the major headings of rock art, ceramics, woven materials, architecture, stone, bone, and effigy figures. The square containing the greatest difference and most significant Chi-square value has been indicated with additional lines. Table 4 gives the Chi-square value for each cell. The differences between motif categories and location classifications in all cells dramatizes the significant relationships between the observed data and the statistical probabilities that choice determined the placement of signs on surfaces.

Before discussing the relationships, an explanation of the Chi-square results would be welcome. The sum of the cell Chi-square values is 718.19. Using a degree of frequency \( (df = (row - 1) \times (column - 1)) \) of 18 and an error probability of .01, the critical value for the Chi-square curve is 34.8. Since the test statistic was greater than the critical value, the null hypothesis is strongly rejected and the alternative is accepted. There is a definite relationship between location and type of motif design. In order to determine the strength of the relationship between dependent motifs and independent locations, a Kramer’s V was calculated, \( V = 0.40 \) in this study validating a moderately strong dependent relationship of the type of sign element with the location.

In the rock art category which includes both pictographs and petroglyphs, the anthropomorphic images indicate the strongest location dependence while the zoomorphic and curvilinear motifs present a strong dependence, in that order. Ceramics, on the other hand, is predominated by geometric motifs. Since the graphic elements were collected from pottery ranging from Basketmaker III through Pueblo III periods and includes the early motifs which later appear in the rock art category, it appears that the iconographic shift seen earlier may not have affected the preference for use of geometric motifs on painted and plain ware.

The next largest dependent classification indicated by the Chi-square numbers is stone which includes only building stones and pendants. Geometric figures
TABLE 3

<table>
<thead>
<tr>
<th>LOCATION</th>
<th>Geometric</th>
<th>Curvi-linear</th>
<th>Zoomorphic</th>
<th>Anthropomorphic</th>
<th>Totals</th>
</tr>
</thead>
<tbody>
<tr>
<td>Rock Art</td>
<td>0=65</td>
<td>0=68</td>
<td>0=51</td>
<td>0=47</td>
<td>0=231</td>
</tr>
<tr>
<td></td>
<td>E=60.97</td>
<td>E=29.23</td>
<td>E=17.28</td>
<td>E=8.01</td>
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</tr>
<tr>
<td>Ceramics</td>
<td>0=248</td>
<td>0=108</td>
<td>0=42</td>
<td>0=3</td>
<td>0=401</td>
</tr>
<tr>
<td></td>
<td>E=105.84</td>
<td>E=50.73</td>
<td>E=30.00</td>
<td>E=13.91</td>
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</tr>
<tr>
<td>Weaving</td>
<td>0=7</td>
<td>0=0</td>
<td>0=0</td>
<td>0=0</td>
<td>0=7</td>
</tr>
<tr>
<td></td>
<td>E=1.84</td>
<td>E=.89</td>
<td>E=.52</td>
<td>E=.24</td>
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<tr>
<td>Architecture</td>
<td>0=15</td>
<td>0=6</td>
<td>0=8</td>
<td>0=0</td>
<td>0=29</td>
</tr>
<tr>
<td></td>
<td>E=7.65</td>
<td>E=3.67</td>
<td>E=2.17</td>
<td>E=1.33</td>
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<tr>
<td>Stone</td>
<td>0=39</td>
<td>0=1</td>
<td>0=1</td>
<td>0=0</td>
<td>0=41</td>
</tr>
<tr>
<td></td>
<td>E=10.82</td>
<td>E=5.18</td>
<td>E=3.07</td>
<td>E=1.42</td>
<td></td>
</tr>
<tr>
<td>Bone</td>
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<td>0=0</td>
<td>0=0</td>
<td>0=0</td>
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</tr>
<tr>
<td></td>
<td>E=.53</td>
<td>E=.25</td>
<td>E=.15</td>
<td>E=.07</td>
<td></td>
</tr>
<tr>
<td>Effigy Figures</td>
<td>0=12</td>
<td>0=3</td>
<td>0=8</td>
<td>0=1</td>
<td>0=24</td>
</tr>
<tr>
<td></td>
<td>E=6.33</td>
<td>E=3.03</td>
<td>E=1.80</td>
<td>E=.83</td>
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</tr>
<tr>
<td>Totals</td>
<td>0=388</td>
<td>0=186</td>
<td>0=110</td>
<td>0=51</td>
<td>N=1470</td>
</tr>
</tbody>
</table>

E = (row total x column total) / Total Population

carry significant relationships with those location classifications, while in architecture they become secondary to zoomorphic figures.

Effigy figures, dominated by zoomorphic subject matter, were made from clay or ground stone. Clay forms were either hollowed-out vessels or miniature heads of a proto-kachina-type mask, fired, painted, or natural. Stone shapes were polished and decorated with motifs or were left plain.

Bone tubes showed the least dependence of motif to location although the two motifs observed on bone are zig-zags, found in rock art, building stones, room wall painting, and ceramics. Possibly a larger sample size of bone tubes with motifs on them would produce more significant relationship indicators.

Woven materials include basketry, cloth sandals, and a cradleboard. The significant Chi-square value for the geometric category probably indicates the limiting nature of the material. Any motif applied to a woven article must be abstracted in order to conform to the technique. The statistic does not show, however, that all of the woven motifs are duplicated in rock art, ceramics, bone tubes, and building stones. Thus woven geometric shapes may not have been chosen only because of adaptibility, but also for the contextual use.
<table>
<thead>
<tr>
<th>LOCATIONS</th>
<th>Geomorphic</th>
<th>Curvilinear</th>
<th>Zoological</th>
<th>Anthropomorphic</th>
</tr>
</thead>
<tbody>
<tr>
<td>Rock Art</td>
<td>(X^2 = 0.27)</td>
<td>(X^2 = 51.42)</td>
<td>(X^2 = 65.80)</td>
<td>(X^2 = 189.79)</td>
</tr>
<tr>
<td>Ceramics</td>
<td>(X^2 = 190.94)</td>
<td>(X^2 = 64.65)</td>
<td>(X^2 = 4.8)</td>
<td>(X^2 = 8.56)</td>
</tr>
<tr>
<td>Weaving</td>
<td>(X^2 = 11.80)</td>
<td>(X^2 = 0.17)</td>
<td>(X^2 = 0.00)</td>
<td>(X^2 = 0.28)</td>
</tr>
<tr>
<td>Architecture</td>
<td>(X^2 = 7.06)</td>
<td>(X^2 = 1.41)</td>
<td>(X^2 = 13.09)</td>
<td>(X^2 = 0.52)</td>
</tr>
<tr>
<td>Stone</td>
<td>(X^2 = 73.39)</td>
<td>(X^2 = 3.37)</td>
<td>(X^2 = 0.80)</td>
<td>(X^2 = 0.80)</td>
</tr>
<tr>
<td>Bone</td>
<td>(X^2 = 1.78)</td>
<td>(X^2 = 0.25)</td>
<td>(X^2 = 0.82)</td>
<td>(X^2 = 2.64)</td>
</tr>
<tr>
<td>Effigy Figures</td>
<td>(X^2 = 5.08)</td>
<td>(X^2 = 0.07)</td>
<td>(X^2 = 18.5)</td>
<td>(X^2 = 0.13)</td>
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</tbody>
</table>

\[ a = \frac{(0-E)^2}{E} \] for each cell if \( E \leq 5 \). Then \( a = \frac{((0-E)-0.5)}{E} \)

\[ X^2 = \sum a \quad \text{or} \quad 718.19 \]

Kramer's \( V = \sqrt{\frac{X^2}{X^2_{\text{max.}}}} \) or \( \sqrt{\frac{718.19}{4410.0}} \) \( V = 0.40 \)
**TABLE 5**

OVERLAP OF ICONOGRAPHIC MOTIFS
HISTORIC AND PREHISTORIC

<table>
<thead>
<tr>
<th>ANASAZI</th>
<th>HISTORIC</th>
<th>ANASAZI</th>
<th>HISTORIC</th>
<th>ANASAZI</th>
<th>HISTORIC</th>
<th>ANASAZI</th>
<th>HISTORIC</th>
</tr>
</thead>
</table>

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Negative relationships also indicated a strong absence of certain types of motifs in certain categories. Many more anthropomorphic figures should be seen in ceramics than actually appeared; no human-like forms were observed in any architectural locations although a small number was expected. More curvilinear elements were expected to be seen in stone such as building stones and pendants than were found. This absence may give greater support to the probability of contextual limits. An absence of any other shape category but geometric on bone tubes may indicate ceremonial or social semantic use since the context provenience was with a burial.

Distribution of geometric motifs among the twenty-five categories was the most even; the Chi-square values suggest a strong cultural preference for geometric shapes. Many of them may have a pan-Anasazi meaning or use. The curvilinear elements, distributed between ceramics and rock art, suggest possible pan-Anasazi meaning and/or ceremonial use. Mountain sheep, hoofprints, and two “bird track” (♀ and ♂) elements showed homogenous distribution in nearly every category, and at Hovenweep next to water control/agricultural sites just in the same fashion that Hopi clan symbols shared agricultural locations and a variety of other contexts, both religious and secular. Mountain sheep and/or bird images appeared in ceremonial locations combined with storage and were repeated singly and together in Wetherill Mesa kiva plasters at Long House, Step House and Mug House.

Table 5 represents the overlap of Anasazi motifs with those found in present Western Pueblo iconographic systems. In a total of seventy-nine image classifications, forty-nine of them are historically viable, or 62%. All but two of the zoomorphic motifs are still used by Hopis as clan symbols. In prehistoric terms we cannot separate the clan symbol from society symbol, and ethnographically there is still evidence of certain clans maintaining the Chief Priest office in an hereditary fashion such as the Snake Clan at Hopi and the Snake Society (Parsons 1936). This ethnographic fact presents the possibility that clans and ceremonial societies were more closely tied together prehistorically than at present.

Comparing Table 1 and Table 2 with Table 5, we can see a decrease over time in the use of geometric classifications (from 36 to 21), a decrease from 21 to 19 curvilinear motifs, an increase in zoomorphic motifs (16 to 32) and a dramatic decrease of anthropomorphic images. Of all the categories, the zoomorphic motifs are the most unaltered by time, inferring analogous use of those signs as clan symbols. The decrease of some geometric and curvilinear motifs can not yet be explained, but certainly the changes and continuities indicate a viable communication system subject to change and adaptation over time just as language does.

Conclusion

Preliminary quantitative data infers preferential use of graphic signs suggesting an Anasazi visual communication system was present. If prehistoric iconography supported an oral tradition mnemonically across space, and social/economic/political and religious systems through redundancy chronologically, then the graphic system may have provided a strategy for documenting the oral tradition, marking noteworthy points within the culture as it evolved.

Acknowledgements:

Data for this paper would not have been possible without the loan by Jerry Fetterman of his Mesa Verde photographs of rock art, the willingness of Louise Stiver and Margo Surovik-Bohnert to give up spare time at the Dolores Archeological Project lab, and the cordiality of Mr. and Mrs. Chappell of Mancos, Colorado.
STRESS AND SUCCESS AMONG THE BLACK MESA ANASAZI

Shirley Powell

Introduction

Models of cultural change for populations inhabiting the plateau Southwest have focused on continuity and stability. Interpretive generalizations emphasize intravillage economic autonomy, sedentary village life, and subsistence based on production of cultivated plant foods (Lipe 1978:341; Martin and Plog 1973:266, 277, 296; McGregor 1965:172, 183, 216). In this paper I present interpretations from data collected by the Black Mesa Archaeological Project between 1967 and 1981 that suggest an alternative picture. Further, I suggest that the revised generalizations are not unique to Black Mesa. Rather, the organizational responses observed on Black Mesa are expected under conditions of population growth and environmental variability. Because these two conditions characterize most areas of the plateau Southwest, the applicability of the Black Mesa patterns should be considered for modeling cultural change in all areas of the American Southwest (the Black Mesa study area is shown in Figure 1).

Widely accepted generalizations about prehistoric subsistence, settlement patterns, and organization suggest that cultigens were introduced in the Southwest by 2000 B.C. Cultigens are viewed as a pervasive part of the subsistence base by A.D. 1. Martin and Plog (1973:277) state that "It seems increasingly likely that where archaeologists have not found evidence of corn by the time of Christ or a few centuries thereafter, it is because the corn has not been preserved or not been found, not because it was not grown."

By A.D. 700, conventional wisdom has southwesterners living in pithouse villages and relying heavily on cultigens for subsistence (Lipe 1978:341). Populations grew rapidly during this period in response to the introduction of improved varieties of cultigens and new technology. Initial summaries of Black Mesa prehistory mirror these generalizations. Swedlund and Sessions state (1976:146) that after A.D. 800 the Black Mesa Anasazi relied on cultigens as a major source of food. Additionally, population increase and favorable climatic conditions forced and allowed the prehistoric occupants of Black Mesa to colonize previously uninhabited upland zones. Dry farming was viewed as the major economic activity of the Black Mesa Anasazi. Finally, each village was viewed as an autonomous productive unit. "Each settlement was an independent entity consisting of a few families at the most, practicing subsistence agriculture with some reliance on hunting and gathering" (Gumerman 1970:118).

Thus, the prehistoric record of the plateau Southwest in general and Black Mesa in particular was interpreted similarly. Peoples practiced a sedentary settlement pattern, subsisted on cultigens, and lived in economically and organizationally auton-
BLACK MESA AND VICINITY SHOWING AREA OF SURVEY AND EXCAVATION

Figure 1
omous villages. Over time, as population grew, villages increased in size, the degree of reliance on agriculture increased, intravillage organizational patterns became more complex, but villages remained largely autonomous.

Theoretical Background

These interpretations are grounded in cultural evolutionary theory. This theoretical orientation creates several ordinal levels of cultural complexity; most prehistoric Southwestern groups would fit into an intermediate position on a continuum of organizational complexity, demographic scale, and geographic extent. This continuum ranges between conditions of spatial boundedness and organizational complexity. Increases in territoriality and organizational complexity have been linked with the innovation of food production, conflict over limited resources, and the need for mechanisms of conflict resolution (Sahlins 1968; Service 1971). The term “tribe” is used frequently to describe this intermediate position on the cultural evolutionary scale.

However, recent studies of human social organization propose that the evolutionary process of regional network intensification occurred as an organizational response to conditions of population growth and environmental uncertainty, two conditions which characterize Black Mesa and the plateau Southwest. These studies emphasize that environments vary in both cultural and physical dimensions, and that social responses to environmental uncertainty involve “increasing social segmental integration but not necessarily political unification or centralization” (Braun and Plog 1980:1).

Braun and Plog (1980:6) note that the survival of living systems depends on their capacity to respond effectively to environmental variability, risk, and/or uncertainty. Two abstract properties characterize environmental variability, risk, and/or uncertainty in all environments, the spatial and temporal unpredictability of individual environmental properties and the number and diversity of environmental properties, that must be monitored and responded to by the living system. Response mechanisms in living systems, then, are viewed as “an abstract hierarchy of successively more deeply embedded ‘control’ mechanisms” (Braun and Plog 1980:7). The more deeply embedded responses deal with generalized or long-term properties of environmental uncertainty, while specific short-term responses meet short-term environmental uncertainties. “Environmental regularity thus generates organizational regularity, in the sense that the more regular the behavior of a particular property of a system’s niche, the more permanent will be the system’s response to that property” (Braun and Plog 1980:7). More concretely, then, human societies are viewed as hierarchies of social networks that represent organizational responses to environmental uncertainty; the depth of embeddedness of a response within the social network depends on the frequency as well as the predictability of environmental uncertainty confronting the network.

One expectation that can be generated from the above model is that “intensification in the level of integration will result from a long-term or effectively permanent increase in the uncertainty of some critical environmental property on a regional scale” (Braun and Plog 1980:9). Such “critical environmental variables” include population increase or variation in the availability of critical resources such as water or arable land. Braun and Plog (1980:9–10) cite an example of an expected response to an increase in population density. They note that if increased population density restricts mobility, that group will be faced with a limited spatial sample of the environment. Such a restriction, in turn, increases the likelihood of spatial and/or temporal variation in the yield of critical resources. Braun and Plog (1980:9–10) predict that under conditions of increasing population density and decreasing mobility, “concomitant increases in the level of cooperation and communication between neighboring localities” also will occur.

Based on this theoretical model, predictions can be made about prehistoric Southwestern subsistence strategies, mobility strategies and settlement pattern, and productive autonomy under conditions of environmental uncertainty and population growth. Subsistence strategies are unlikely to be highly specialized, given extreme variability in critical environmental parameters such as temperature and rainfall. Productive specialization will result from intensification, which is a necessary response to population/resource imbalance. Mobility will become restricted as a result of population growth; the settlement pattern then will include larger, sedentary villages as well as seasonally occupied camps as population density increases. This contrasts with the earlier, low density pattern which consists entirely of seasonally occupied sites. Under conditions of very low population density and high mobility, widespread social ties are necessary to ensure access to resources in other areas as well as to maintain a viable mating network. As population density increases, the spatial extent of the mating network may decrease. However, in variable and marginal environments such as the American Southwest, the need to maintain access to resources and information from other areas still exists. Thus, although the scale of social networks may contract somewhat over time, village autonomy will never be the expected situation.

The subsequent sections of this paper will discuss change over time in subsistence, settlement patterns, and exchange networks on Black Mesa. The applicability of the Black Mesa generalizations to other areas in the American Southwest will be considered.
Stress and Success . . .

Subsistence and Settlement

A recently completed study has questioned assumptions about the relationships between subsistence and settlement strategies for the Black Mesa Anasazi (Powell 1980). That study suggested that subsistence strategies were more diverse than previously supposed. Such diversification is important because of the theoretical interplay between behavioral variety and selective processes. For example, if a specialized population with little behavioral variety experiences an environmental change, “it may not possess any individuals practicing strategies appropriate to the changed conditions” (F. Plog 1974:52). However, if a population practicing diverse adaptive strategies encounters an environmental change, “there is a much higher probability that at least some individuals in the population already will be practicing a . . . strategy” appropriate to the new conditions (F. Plog 1974:52).

It is well documented that the Southwest in general, and Black Mesa in particular, is an environment characterized by great diversity and uncertainty in critical parameters (e.g. precipitation, temperature, and the availability of many critical resources) (Ford 1972). Yet, generalizations about prehistoric subsistence and settlement strategies emphasize the reliance of the prehistoric Anasazi on cultigens as well as their occupation of settled farming villages. This specialized adaptive strategy seems highly unlikely given the environmental uncertainties constraining the Anasazi. Indeed, recent summaries of Anasazi subsistence suggest reliance on an extremely diverse array of wild plant foods and cultigens (Gasser n.d.). These findings parallel generalizations made for the Black Mesa Anasazi (Powell 1980).

Ethnographic literature suggests strategic regularities between the prehistoric Anasazi and groups similarly organized, occupying similarly uncertain environments, and subsisting on a similar mixture of hunted, gathered, and horticultural products. Such peoples practice a seasonal settlement round with occupation of distinct winter and summer villages. This ethnographic pattern contradicts generalizations about prehistoric Anasazi settlement strategies.

To test these generalizations, a series of material correlative data collected from recently abandoned, seasonally occupied Navajo camps. Data collected from Black Mesa Anasazi sites were then examined to determine whether their artifactual, feature, and structural configurations matched those of the Navajo seasonal occupations. The patterns observed on the Anasazi sites tentatively indicated that the resources of northeastern Black Mesa were seasonally exploited prior to A.D. 1050. Only between A.D. 1050 and 1150 was northeastern Black Mesa inhabited on a year round basis. The A.D. 1050 date coincides roughly with the beginning of a period of rapid population growth (Layhe 1977). Further, macrofloral evidence suggests that economic plant procurement strategies were intensified at this time through diversification in the resources exploited and not through increased reliance on cultigens (Powell 1980:241–248).

Data from paleopathological analyses of the Black Mesa burial population provide corroborative evidence for these conclusions. The osteological data suggest that “the Black Mesa population was not being subjected to the stresses which normally result from sedentary and agricultural life-styles. Rather, they seem to exhibit low-level, possibly sporadic or seasonal, nutritional stress and physiological disruptions” (Martin 1981:6–7). Thus, several data sets independently suggest that the Black Mesa Anasazi were seasonally mobile and not highly reliant on cultigens.

Conclusions so generalized might be questioned by archaeologists who feel that Black Mesa was an Anasazi backwater and that subsistence and settlement strategies practiced on northeastern Black Mesa might have been an extreme adaptation unrepresentative of the plateau Southwest as a whole. However, recent studies of Anasazi subsistence practices indicate that an eclectic hunting, gathering, and horticultural economy was the rule, not the exception (Gasser n.d.), and environmental studies continue to document the uncertainty under which the Anasazi lived. Thus, seasonal mobility may more appropriately characterize all Anasazi settlement strategies, under conditions of low population density, than the generally accepted picture of agriculturalists living in settled villages.

Exchange and Social Networks

The theoretical model also predicts that under conditions of low population density, exchange networks will be widespread. The scale of the exchange network is a response to at least two factors: widespread social networks are necessary to ensure access to resources in other areas as well as to maintain a viable mating network (e.g. Wobst 1974; Wiessner 1977). Plog has estimated, using population density figures for the Great Basin Shoshoni and an estimated viable mating network of 475 individuals, that “the spatial extent of marriage networks . . . should be about 19,700 km², or a radius of 79.2 km around a given community” (Plog and Powell 1981a:7–8). This low density network would have encompassed, from a Black Mesa center, portions of the entire Four Corners area.

As population density increased, the spatial extent of the mating network may have decreased. However, the need for information and resource exchange still existed due to the unpredictable and marginal southwestern environment. These environmental conditions would have precluded social or economic village autonomy.
Analyses of lithic raw materials (Green n.d.; Fernstrom 1980) and ceramics (Deutchman 1980; S. Plog 1980a; 1980b; Hartman and Plog n.d.) have documented considerable exchange between Black Mesa and surrounding regions, as well as a decrease in the spatial extent of the exchange networks over time. "Between A.D. 800 and 900 much more material from distant sources was present on sites than during the period A.D. 1050 to 1150" (Plog and Powell 1981a:9–10). Fernstrom (1980) analyzed patterns in the distribution of chipped stone from three source-distance categories on Black Mesa sites. She demonstrated that although over time, and with population increase, the volume of chipped stone increased, the extent of the lithic exchange network decreased. Less lithic material was coming from the more distant sources and more was derived from the closest sources.

Similar patterns of variation have been shown in the distribution of two ceramic wares. San Juan Red Wares are generally believed to have been produced in southeastern Utah and/or southwestern Colorado, while Tsegi Orange Wares were produced in the area to the north of Marsh Pass, in northern Arizona. Thus, San Juan Red Wares derive from sources more distant from Black Mesa than Tsegi Orange Wares. However, distance alone does not explain the distribution of these two ceramic wares on Black Mesa sites. "San Juan Red Ware was most abundant during the earliest time period, prior to A.D. 900, and then gradually decreased in frequency through time while Tsegi Orange Ware gradually increased" (Hardy et al. 1980:6). Thus, ceramics from the more distant source decreased in abundance, while those from the closer source increased. Again, this decrease in the scale of ceramic exchange coincides roughly with a period of population increase (Layhe 1977).

In sum, evidence from both lithic and ceramic analyses show that there was considerable exchange over time between Black Mesa and surrounding populations, and that the scale of the exchange networks decreased over time. Further, this decrease in scale coincides roughly with a period of population increase, restriction in seasonal mobility and intensification of subsistence strategies.

Conclusions

In conclusion, data collected by the Black Mesa Archaeological Project during the past 14 years suggest a picture of seasonal population relocations and participation in widespread exchange networks during the period between A.D. 850–1050. For the period between A.D. 1050–1150, the data suggest population increase, decreasing mobility, and more compact exchange networks. Furthermore, reconstruction of the subsistence base of the Black Mesa Anasazi suggests reliance upon a mixture of hunted, gathered, and cultivated resources throughout the occupational history of the area (ca. 600 B.C.– A.D. 1150). This subsistence pattern correlates ethnographically with seasonal population relocations.

Thus, the Black Mesa data suggest an alternative picture to the generally accepted settled agricultural, autonomous village model of the prehistoric occupation of the Colorado Plateau. The data suggest seasonal mobility and participation in widespread regional exchange networks. These patterns are consistent with predictions made from the theoretical model discussed above. The Black Mesa Anasazi apparently were increasing the level of interregional cooperation and communication among neighboring localities under conditions of population increase and environmental variability.

Since most researchers concur that population increase and extreme environmental fluctuations characterize most areas of the Plateau Southwest, the applicability of the ecological model should be considered for modeling organizational change in the American Southwest.
ENVIRONMENT AND THE ANASAZI

Friday, October 2, 1981
Morning Session I
Chair: George Gumerman
The theme that I wanted to pick up this morning is Anasazi influence on the environment. It is a theme that has been neglected in the Southwest for reasons that I don’t fully understand. And I think this is also true in the Anasazi area. It may be that paradoxically, this reflects the successes of early environment studies in generating a climatic geologic model of considerable interpretive potential. But probably the reasons lie in the methodical definition of archaeology as a sub-discipline of anthropology. I make no apologies for treating this theme though I found it very difficult because the evidence is disparate and I’m not entirely satisfied with some of the conclusions that I have reached here. It may be, of course, that partly this reflects the fact that the full implications of the study really are more within the ancillary sciences than within archaeology proper. But in any case, for convenience I have divided the theme up into three sections entitled water control, deforestation, and salinization.

Now, Anasazi water control systems have been defined in several areas and are characterized by considerable diversity. Really, only in Chaco Canyon do we have something that looks like large scale planning which would have other than local environmental effects. This is at least the area that I’d like to focus on in talking about water control, principally because of some of the early work that Bryan did in making the climatic argument for erosional cycles in the Southwest. It is interesting that although evidence for buried channels has been found in other Southwestern valleys, it is really in Chaco Canyon that the pivotal argument for climatic agency has been made. And yet, it is here that evidence for considerable environmental influence exists, this mainly in the form of field systems. Now, I think the evidence for these field systems is somewhat dubious, and certainly they should be the subject of critical study, but I’ll leave this for future workers, perhaps. In any case, the question becomes of deciding really what the environmental impact was in Chaco Canyon, and I am curious as to whether we are dealing (and here I’m making a distinction that Lawton and Wilkie make) whether we are dealing with the runoff systems or water harvesting systems, because if we are dealing with water harvesting systems we are dealing with treatment of the watershed to maximize runoff. It strikes me that the water control systems in Chaco Canyon as postulated by Vivian are really too elaborate to be simple runoff systems. So I think we ought to be looking for treatment of the watershed to maximize runoff, to harvest water, in fact, and this really gets us at the guts of the environmental question. Now, there is some doubt as to whether the Chettró Ketl fields are, in fact, prehistoric, and I think it is incumbent upon us to carry on with this environmental work, to define the problem, to reconstruct the systems as fully as we possibly can. So, in
summary, then, what I am saying is that the climatic argument that Bryan makes seems very dubious in the context of Chaco Canyon. And if that is the case, we really have to examine the implications of runoff systems or water harvesting systems in terms of the erosional cycle. Unfortunately, there has been precious little work done here, but I am presenting this simply as a methodological statement.

Evidence for deforestation in Chaco Canyon is meager and open to question, but it is quite clear that if deforestation was involved and occurred during the height of the Anasazi developments in the canyon, this would have had considerable impact on erosion. This may be another qualification of Bryan’s climatic argument. Pollen evidence is less ambiguous in Mesa Verde, and it is quite clear that we are dealing here with secondary forest succession. The climatic argument has been made, but it seems to me more economical and convenient to make a cultural one. Exactly what the effect in Mesa Verde of deforestation is, I’m not entirely clear. I don’t believe there has been much work done in the area. It is clearly not as pivotal as Chaco Canyon for the alluvial chronology of the Southwest. Nonetheless, it is a theme that should be examined. What intrigues me here is Steiger’s postulate of shifting cultivation on Mesa Verde, which seems to me inherently unlikely in a semi-arid environment, particularly since we are dealing with a successional period of 100 to 300 years in reestablishing forests. But this is something that could be tested with information on population and the suitability of agricultural land; I think some sort of modeling could be done here. It’s worth testing. I do think it inherently unlikely, however. Interestingly enough, I believe, in Chaco Canyon Greminger has argued that the development of water control systems is linked to deforestation. What I think we ought to focus on more clearly is the larger theme of environmental influence, or the influence of man on the environment.

Salinization, now. Early work by Judd in Chaco Canyon was somewhat problematic. It is unclear exactly where the samples were taken, and clearly any argument of salinization must deal with the extent and the definition of the agricultural areas. This concept was resurrected by Love in Chaco Canyon, but I don’t think it is very satisfying because here the same problems exist as to the extent and the definitions of the arable area or the agricultural areas. Other attempts to postulate salinization as a factor in the Anasazi area . . . this is, I guess, Fosberg’s work in the Cochiti area . . . remain unconvincing and do not bear critical examination. They are premised on the basis of soil geography, and the soil association maps are simply too general to reach any conclusions as to the importance of salinization.

Well, what I have been trying to do here, and I guess not very successfully, is to argue, to make a plea for defining a field which, for better or worse, I could call something like environmental archaeology as a theme in its own right, that really any conclusions that we form are simply premature at this point. The evidence is tantalizingly vague, but there is a theme which I think is of interest to environmental scientists generally; it has been neglected by archaeologists for various reasons. I think it has probably wider implications within the earth sciences, and here I think earth scientists have been as lax as the archaeologists in reorienting their methodology. Anyway, the paper that I am presenting this morning, hopefully, indicated the nature of the problem and the limited evidence available. I would have preferred to make some simple methodological comments, but I have tried to avoid a climatic cast to this thing. I don’t know if I have done that successfully or not. Separation of climatic and cultural factors is inherently difficult in paleoenvironmental studies, and I think this is possibly the difficulty that we have been laboring under for several years.

Editor’s note: Mr. Kosse declined to send us a copy of his paper. We therefore transcribed, essentially verbatim, his presentation at the Symposium. There is no bibliography available.

Questions and Comments

Jim Judge: I’m interested in your distinction between runoff and water harvesting systems.

Alan Kosse: Well, this is a distinction that some other workers have made. Basically, with a simple runoff system there is little or no treatment of the watershed to maximize runoff. In the case of water harvesting there is treatment of the watershed; and by treatment I mean just clearing of vegetation but, as in the Middle East, you get mounding up of rocks, clearing of desert pavement, exposing the soil surface to create an impermeable surface. There are various
treatments that would maximize runoff which would leave permanent traces. It strikes me that the Chaco Canyon system, as Vivian postulated it, is simply too elaborate to be just a water runoff system, far too complicated, that the correlate of that should be that you would find some attempt to maximize runoff, some attempt at water harvesting rather than simply collecting runoff. It is ironic, in a way, that the pioneer work of Bryan was done in Chaco Canyon, really the one area that we have evidence for large scale planning of agriculture systems with all the implications for influencing runoff and sediment yield. So the argument, because it is essentially premised on the work done in Chaco Canyon, is of necessity qualified. Recently, Leopold in a 1976 article has made this same climatic argument as Bryan to effect that we can exclude the influence of man as a factor in accounting for the earlier erosional cycle in Southwestern valleys. Simply, it doesn't bear critical examination, or at least let's say it's a rather dubious point in the case of Chaco Canyon. Whether or not it holds in other Southwestern valleys I don't know, but certainly one must be aware of the potential of prehistoric man to influence the environment. The sad fact is that the final verdict is not in, that this has been a theme that has been ignored in Southwestern archaeology, perhaps in archaeology generally. It is very difficult to present a critical review of the evidence; in fact I've found it very unsatisfying. But I do think that it is a theme that will be picked up again. I hope that environmental scientists will not have to continuously keep on justifying interest in themes like that, and here I really don't think that the sociocultural implications are really the important conclusion; I think that the implications in geological studies or geomorphic studies or palynology are far greater for the immediate . . . and I would argue, eventually would require a methodological redefinition of the fields. So, the implications are mainly in ancillary sciences, I'm afraid. Secondary salinization is sort of a convenient *deux ex machina* that I think got into the Anasazi area via the Hohokam, and I really doubt it was much of a factor in the area from what I can see. Now, that's not to say that further work won't indicate that that is the case.

Comment: I wanted to point out if you take Vivian's mapped fields and the field map by Father Kelly, and give Chetro Ketl the benefit of the doubt for being prehistoric, you only have seventy acres of intensely modified agricultural areas, and, say, in the San Juan River Valley it takes about eight-tenths of an acre per person, if there's about twenty percent outside your agricultural district, either urban wage laborer or hunter-gatherer or whatever, so that formalized field systems only supported maybe 90 to 100 people. I seriously question if those fields had much to do at all with the price of eggs in Chaco.

Kosse: The question of whether they are really prehistoric is really open and will require further work, I think. In a way I really hate to use Vivian as a strong man here, but its awfully hard to resist in this case simply because, I suppose, of the accident of Bryan's work in Chaco Canyon, his laying the foundations for the alluvial chronology of the Southwest. His assumption that climate was the primary agent here, in that cultural influence was not important, is something that will have to be critically tested.

Frank Eddy: You are saying that the water control facilities are responsible for the patterning and the cut and fill that Bryan discovered?

Kosse: No, I'm not saying that. Bryan’s argument for climatic agency depends essentially on ruling out the effects of man. This is seen as a dubious procedure in Chaco Canyon because of the evidence of large scale water control systems which are possibly more sophisticated than any in the Anasazi area.

Eddy: I would like to point out that the alluvial chronology of Bryan pretty well fits other valleys for the Colorado Plateau.

Kosse: Yes. Here I'm not arguing that it is cultural agency or climate, simply that if the argument is premised on Bryan's work done on Chaco Canyon that the argument becomes very dubious when examined carefully. I'm not saying that climate was not responsible for the erosional cycle that we see in the Southwest, only that if Chaco Canyon is viewed as a prime example, and it seems to have been by Bryan and now by some of his former students, that . . . I don't want to say that it doesn't hold water but that it ought to be examined critically, that’s all.

George Gumerman: I've noticed in a number of papers yesterday and in very recent literature a tendency to downgrade the effects of environment on human behavior. I don't hesitate to remind you that where we are this morning (Recreation Hall) as opposed to yesterday (outdoor amphitheater) is because of climatic variations.
CULTURAL AND CLIMATIC IMPLICATIONS
IN ANASAZI FAUNAL EXPLOITATION:
A REVIEW AND PERSPECTIVES

Steven D. Emslie

Introduction
This paper will attempt to review what we currently know concerning the use of animals by the Anasazi. I will not attempt to present data on a site by site analysis procedure. This information can be located in previously published and unpublished site reports. Instead, I will concentrate on presenting general trends through time in Anasazi faunal exploitation following the Basketmaker through Pueblo IV classification system. Though much of this synthesis is based on currently published data, I will also incorporate unpublished data as available to me. In addition, this paper will concentrate on bird and mammal remains only as these groups form 95% of most faunal collections from an Anasazi site. Not considered here are variations in preservation factors, soil conditions, and other components which may affect the amount of bone present in a site (taphonomy), or worked bone recovered at these sites. This study will be approached by first looking at general trends in faunal exploitation, sources of the fauna with evidence for cultural implications including trade, animal domestication, and patterns in faunal recovery at Anasazi sites, and finally, climatic interpretations of these fauna.

General Trends
Anasazi sites are located in a variety of geographic and ecological zones ranging from desert shrublands to transition zones between pinyon-juniper and mixed coniferous forests. Consequently, the local fauna available to the Indians varied a great deal. It is therefore not possible to present a pan-Anasazi trend in faunal exploitation through time at the species level. Instead, trends become more visible when taxa are viewed from the family or order levels.

For example, I believe most faunal analysts of Anasazi material would agree that the dominant groups in these collections are rabbits (Lagomorpha) and large game (Artiodactyla). However, depending on the location of an individual site, one site may show exclusive or dominant utilization of Pronghorn (Antilocapra americana), while another site is dominated by Mule Deer (Odocoileus hemionus), American Elk (Cervus canadensis), or Bighorn Sheep (Ovis canadensis) remains. In addition, while most Anasazi sites contain an abundance of cottontail (Sylvilagus sp.) and Black-tailed Jackrabbit (Lepus californicus) bones, sites at the Dolores Archaeological Program (DAP) in southwestern Colorado have also produced remains of Snowshoe Hare (Lepus americanus) and Pika (Ochotona princeps). I know of no other Anasazi sites to have these latter two species represented in the faunal material. Consequently, while most Anasazi sites are dominated by Lagomorpha and Artiodactyla remains, the species exploited in each group at each site may vary from region to region.
Another group which was undoubtedly utilized extensively by the Anasazi is the rodents. Though many archaeologists tend to view all or most rodent remains in a site as intrusive and not related to cultural activities, these animals would have provided an important and abundant food source. I have commonly observed cut marks and burning on bones of the Porcupine (*Erethizon dorsatum*), Beaver (*Castor canadensis*), and Yellow-bellied Marmot (*Marmota flaviventris*) from Anasazi sites, and burned bone of many of the smaller rodents including mice (*Cricetidae*) and ground squirrels (*Sciuridae*). Small rodents would also have been abundant and easily trapped near agricultural fields and trash deposits.

Less abundant but commonly present in Anasazi sites are bones of carnivores. In particular, the Badger (*Taxidea taxus*), canids (coyote, fox, and Indian Dog), Bobcat (*Lynx rufus*), Bear (*Ursus* spp.), and mustelids (skunks and weasels) frequently occur in Anasazi sites. One possible reason for the lower density of this group from the sites may be the relatively fewer numbers of these species in a given area compared to the greater densities of rabbits and rodents. Artiodactyls would also have been more preferred for their higher, and more edible, meat content.

Bird remains are relatively rare in most Anasazi sites except where the domestic turkey played an important role. Often sites which contain a paucity of bird bones do have many small fragile rodent bones in the strata suggesting that preservation factors are not the explanation for the absence of bird remains. In addition, Pueblo IV sites in New Mexico such as Pottery Mound and Picuris Pueblo have produced hundreds to thousands of bird bones (Emslie 1981a, 1981b), as well as some earlier sites in Chaco Canyon. These sites suggest that birds were of great importance to the Anasazi as a food source, for feathers, and for ceremonial objects by the PIII-PIV periods. However, certain species of birds were seemingly important to the Basketmaker Anasazi as well, as evinced by cave material recovered by Kidder and Guernsey (1919) and Guernsey and Kidder (1921).

In summary, the Anasazi exploited a variety of animal taxa but seem to have concentrated on rabbits and artiodactyls. Rodents were probably utilized extensively; carnivores were regularly hunted but not as extensively as other groups. Birds remains appear throughout the Anasazi time range but appear to be most important as an economic group in the later periods. Table 1 summarizes the most commonly occurring species of birds and mammals in Anasazi sites.

### Sources of Anasazi Fauna and Evidence:

Anasazi fauna was obtained from one of four sources:

1. Hunting and trapping of species local to the area, particularly those near agricultural fields
2. Long distance hunts away from the pueblos
3. Trade from outside sources
4. Domesticates kept at the sites

Most species of animals used by the Anasazi could have been hunted or trapped within a small radius of the pueblos, even though a large diversity of animals are represented in the remains. This is particularly true, as the Anasazi had an economy based on agriculture since the Basketmaker periods. Agricultural fields tend to attract greater densities and diversities of plants and animals than would normally occur in an area. This agroecosystem model has been discussed in detail in other papers (e.g., Linares 1976; Bye 1981; Emslie 1981a, 1981b) and will not be discussed at length here. Basically, fields are known to attract greater densities of fauna for three major reasons:

1. They are formed of uniform, highly edible crops which attract high numbers of insects and their predators
2. Fields break down habitat barriers allowing intermixing of previously separate biotic communities
3. Field edges form ecotones between two habitats where the “ecotone effect” will increase species diversity.

By continually trapping their fields, the Anasazi would not only obtain a diversity of fauna in their diet, they would also be protecting their crops from damage by predators. In addition, many animals are highly attracted to agricultural fields and human settlements and can increase their distributional range because of human intervention to new areas. This has been documented for several species of birds and mammals (see Emslie 1981b), and archaeological records indicate that certain species, such as the sandhill crane and marmot, had greater distributions in the Southwest during Anasazi occupation. Other taxa highly attracted to fields which frequently occur in Anasazi sites include rabbits, rodents (particularly prairie dog, gopher, and porcupine), deer and elk, geese and ducks, American Kestrel, sandhill crane, mourning dove, horned lark, and corvids (jays, raven, and crows). As a society becomes more dependent on an agricultural economy, particularly if irrigation systems are also built and maintained, less time becomes available for long distance hunting; again, trapping near the fields and villages becomes more economical.
Emslie

TABLE I.

Certain species were also obtained by the Anasazi through trade. Most familiar here are the parrots and macaws traded from Mexico and Mesoamerica (Hargrave 1970). Interestingly, parrot and macaw remains are not found, or are rarely found, in the northern Southwest. For example, there are only three records of macaw from Utah and all of these consist of only feathers or feather fragments of the Scarlet Macaw (*Ara macao*) (Hargrave 1960, 1979; Emslie and Hargrave 1978). In addition, there are no records of parrots or macaws from southwestern Colorado, although evidence exists for other forms of trade from the south (i.e., shell artifacts from the Hohokam). The farthest northern occurrence of macaw skeletons were those found at Kiet Siel, northern Arizona. The absence of macaws farther to the north may have been due to one of several factors: macaws could not survive climates farther to the north, macaws were so valuable to the Indians that normal trading processes could not supply the increasing demands to the north, or trade this far north was not possible before a decline in macaw trade occurred. At Pottery Mound, a PIV site on the Puerco River southwest of Albuquerque, not a single bone of parrot or macaw was recovered though dozens of these birds are depicted on the numerous kiva murals uncovered at the site (Hibben 1975). This site was occupied by A.D.1325-1350 and abandoned by 1450-1490 (Voll, 1961) supporting arguments that a breakdown in trade with Mesoamerica occurred in the 1300’s (Kelly and Kelly 1975).

Another probable trade species was the bison (*Bison bison*). In southwestern Colorado bones of this species are scarce and only a few phalanges have been identified at sites in Mesa Verde National Park, suggesting that only skins were brought to these sites. Although large open grasslands once existed west of the present town of Cortez which would have been suitable habitat for bison, the archaeological evidence indicates they were not present there. Sites in the area, particularly at Mancos Canyon west of Mesa Verde, have produced numerous pronghorn remains, prob-

TAXA OF MAMMALS AND BIRDS WHICH MOST FREQUENTLY OCCUR IN ANASAZI FAUNAL COLLECTIONS OF ALL TIME PERIODS.

<table>
<thead>
<tr>
<th>Mammals</th>
<th>Birds</th>
</tr>
</thead>
<tbody>
<tr>
<td>Cottontail (<em>Sylvilagus spp.</em>)</td>
<td>Ducks and Geese (<em>Anatidae</em>)</td>
</tr>
<tr>
<td>Black-tailed Jackrabbit (<em>Lepus californicus</em>)</td>
<td>Red-tailed Hawk (<em>Buteo jamaicensis</em>)</td>
</tr>
<tr>
<td>Prairie Dog (<em>Cynomys spp.</em>)</td>
<td>Golden Eagle (<em>Aquila chrysaetos</em>)</td>
</tr>
<tr>
<td>Rock Squirrel (<em>Spermophilus variegatus</em>)</td>
<td>American Kestrel (<em>Falco sparverius</em>)</td>
</tr>
<tr>
<td>Ground squirrels (<em>Spermophilus, Ammospermophilus</em>)</td>
<td>*Common Turkey (<em>Meleagris gallopavo</em>)</td>
</tr>
<tr>
<td>Gopher (<em>Thomomys spp.</em>)</td>
<td>Sandhill Crane (<em>Grus canadensis</em>)</td>
</tr>
<tr>
<td>Kangaroo Rat (<em>Dipodomys spp.</em>)</td>
<td>Mourning Crane (<em>Zenaida macroura</em>)</td>
</tr>
<tr>
<td>Beaver (<em>Castor canadensis</em>)</td>
<td>Great Horned Owl (<em>Bubo virginianus</em>)</td>
</tr>
<tr>
<td>Mice (<em>Peromyscus spp.</em>)</td>
<td>Common Flicker (<em>Colaptes auratus</em>)</td>
</tr>
<tr>
<td>Moles (<em>Microtus spp.</em>)</td>
<td>Horned Lark (<em>Eremophila alpestris</em>)</td>
</tr>
<tr>
<td>Woodrats (<em>Neotoma spp.</em>)</td>
<td>Common Raven (<em>Corvus corax</em>)</td>
</tr>
<tr>
<td>Porcupine (<em>Erethizon dorsatum</em>)</td>
<td>Jays (<em>Corvidae</em>)</td>
</tr>
<tr>
<td>Coyote (<em>Canis latrans</em>)</td>
<td>*Species kept as domesticates</td>
</tr>
<tr>
<td>*Indian Dog (<em>Canis familiaris</em>)</td>
<td></td>
</tr>
<tr>
<td>Red Fox (<em>Vulpes vulpes</em>)</td>
<td></td>
</tr>
<tr>
<td>Long-tailed Weasel (<em>Mustela frenata</em>)</td>
<td></td>
</tr>
<tr>
<td>Badger (<em>Taxidea taxus</em>)</td>
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</tr>
<tr>
<td>Bobcat (<em>Lynx rufus</em>)</td>
<td></td>
</tr>
<tr>
<td>American Elk (<em>Cervus canadensis</em>)</td>
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<tr>
<td>Mule Deer (<em>Odocoileus hemionus</em>)</td>
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</tr>
<tr>
<td>Pronghorn (<em>Antilocapra americana</em>)</td>
<td></td>
</tr>
<tr>
<td>Bighorn Sheep (<em>Ovis canadensis</em>)</td>
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Cultural and Climatic Implications...

Escaped or abandoned turkeys we now find in various localities in the Southwest. McKusick (1980) has shown that two breeds of turkey, a Small Indian Domestic (SID) and a Large Indian Domestic (LID), were traded into the Southwest. These breeds can be distinguished by size and osteological characters (Ibid.). Positive evidence for turkey domestication (whole skeletons, eggshell, bones of young individuals) in the northern Southwest does not occur at most sites until the Pueblo II period. However, a Basketmaker III site (GG 70-187) on Cedar Mesa, southeastern Utah, has produced a number of turkey bones (identified as the LID by McKusick), suggesting domestication was occurring here (Bill Lipe, personal communication). Since few other sites in Utah or Colorado are indicative of turkey domestication prior to the Pueblo II period, it appears the species was being domesticated locally at specific sites and not accepted widely until later. At the DAP, turkey remains are scarce at most sites which date up to the late PII period. Interestingly, grous remains are far more common at DAP sites than turkey, a trend I have not observed at any other Anasazi locality. Apparently the Anasazi of the Dolores region found the wild native Galliformes more economical than keeping domestic turkeys.

Large PIII and PIV sites in New Mexico suggest specialized trading systems were also in use. At Picuris Pueblo, the avifauna is dominated by large numbers of hawk and eagle wing bones (out of a total of 1,920 bird bones from this site, 674 are eagle and 328 are hawk) suggesting feathers and wing fans may have been traded from here (Lyndon Hargrave, pers. comm.). At Sapawe Pueblo, large numbers of domestic turkey skeletons were recovered (out of a total of 2,347 bird bones from this site, 2,075 are turkey and include at least 38 individual skeletons). Florence Hawkley Ellis (pers. comm.) believes this site may have been a “turkey farm” where turkey feather blankets were manufactured for trade to other areas. The avifauna at Pottery Mound was dominated by turkey and sandhill crane remains (out a total of 3,478 bird bones from this site, 2,227 are turkey and 349 are sandhill crane). This site may have been a food processing and distribution center, as many of the bones had butchering marks and few articulated skeletons were recovered (Emslie 1981a). Anasazi sites in the Dolores area have also shown this high dominance for one or two species in the faunal remains. For example, site 5MT 2854 contained 2,445 animal bones of which over half (1,329) are of small mammals and 605 (24.78%) are of rabbits (cottontail and jackrabbit). Only 1.88% of the bone was of large mammals, a pattern opposite the usual findings at sites in this area. Additionally, a relatively large number of worked jackrabbit tibiae (17) was found at the site. These data strongly suggest the inhabitants of this site were involved in specialized processing activities of small mammals, particularly rabbits. All of the above examples seem to indicate that certain Anasazi sites may have been involved in specialized butchering, processing, and/or trading activities, at least concerning faunal exploitation.

Other patterns occur in faunal remains at Anasazi sites evincing intrasite butchering and food distribution. At DAP, site 5MT 4475 has produced bone clusters consisting of two to four bones of the same element of the same side of the same species, or of the same element of several species. For example, one provenience (FS 555) contained a right femur of a cottontail, porcupine, jackrabbit, and two right femora of a prairie dog, all with similar breakage patterns. Another provenience (FS 77) contained 20 jackrabbit bones which were parts of three left hind feet of three individuals. Similar occurrences have been observed at other sites as well, and I believe these patterns are not coincidental but the result of cultural activities. Perhaps certain parts of an animal obtained on a hunt were always divided among the clan or kinship group in a similar manner. These patterns may also indicate locations of meat storage areas or trash deposits. Careful collection of faunal materials at other large Anasazi sites in the future should reveal similar patterns in cultural use of fauna, particularly when analyzed in relation to room clusters and other artifact assemblages in a pueblo (Emslie MS).

Climatic Implications

It has already been discussed as to how cultural modifications of the local ecology can affect animal densities, diversities, and distributions. However, a number of species recovered from Anasazi sites require more complex explanations. At Picuris Pueblo, for example, bones of the Passenger Pigeon (Ectopistes migratorius) and Boreal Owl (Aegolius funereus) were both recovered from strata dating to A.D. 1300-1350 (Hargrave and Emslie 1980; Emslie 1981b). Two Passenger Pigeon bones were also recovered from Una Vida in Chaco Canyon, New Mexico dating to A.D. 975-1030. At DAP, several sites dating to no later than A.D. 1000 have produced bones of pika, snowshoe hare, wolverine, and pine marten. All of the above records occurred far outside the current range of these species and, with the excep-
tion of the Passenger Pigeon, all these species occupy higher elevations: talus slopes at and above timberline and boreal forests. It is possible, but unlikely, these species were brought to the sites through trade or long distance hunting expeditions.

Another explanation for these occurrences has been offered by Hargrave and Emslie (1980). These authors believe that minor mesic intervals, perhaps during the A.D. 900’s and 1300s allowed certain groups of taxa to shift their distributions southward. The mesic period during the 1300’s is also supported by pollen evidence at Picuris Pueblo where Schoenwetter (1970) found increases in effective moisture during the period A.D. 1335-1425. These cooler periods may also have affected local growing seasons in some areas causing crop failures, and possibly initiating Anasazi population movements to the south where the seasons were longer. It is interesting to note that these faunal records roughly correspond in time to when the Anasazi were undergoing major shifts in population and cultural modifications. It is apparent that considerably more data are required to adequately study these relationships. A shift in climatic patterns to a period of mild winters and cool summers, which also occurred in the Southwest during the Pleistocene (Van Devender and Spaulding, 1979) may have caused severe impacts on Anasazi settlements in the four-corners region. This is viewed as an alternative explanation (rather than drought) for pueblo abandonment which has been mentioned by previous authors but never really studied in depth.

Conclusions

This paper presents a brief overview of Anasazi faunal exploitation for which more detailed studies are now in progress. Not presented here were considerations of taphonomy, worked bone, and specific site data presentation. Instead, current knowledge and recent research were synthesized to formulate general trends and implications in Anasazi faunal exploitation.

The Anasazi utilized a diversity of fauna but seemed to concentrate primarily on the Lagomorpha and Artiodactyla, supplementing their diet with a variety of rodents, carnivores, and birds. Other animal taxa, including fish, amphibians, and reptiles, have not been discussed here but were of varying importance to the Anasazi. In addition, by the later time periods (PIII-PIV) specific sites may have had specific functions regarding faunal exploitation. Several sites have provided evidence for dominance of single taxa or related taxa in the faunal record. Intrusite butchering and distribution patterns of fauna may also become more apparent, particularly at the larger pueblo sites, with more careful collection and recording procedures by the archaeologists. Finally, temporary shifts in climate to periods of mild winters and cool summers may have altered distributions of certain species which have occurred in Anasazi sites as well as causing severe impacts on Anasazi society.

Questions and Comments

Doug Scott: Your idea of bison being traded in from the plains is intriguing, but you might look at an alternative to that, or at least an additional area, and that would be the area to the north—the Uncompahgre, Gunnison, and San Miguel River valleys. Hurst in the 1940’s picked some bison up in Tabeguache country, and the Ute Prehistory project also got bison although not a great deal of it. I wonder if that might be a more likely source than even the plains.

Emslie: Yes, that’s true. I had forgotten about that one but I remember the reference.

Question: Is there any evidence at all that bighorn sheep were living down there?

Emslie: Oh yes, there is quite a bit of bighorn sheep remains that have been recovered from Mesa Verde and Mancos Canyon. In fact, there is a trend, at least at Mancos Canyon, where mountain sheep tend to increase through time, in how they were utilized, compared to deer and other artiodactyls.

George Gumerman: I think Steve’s paper underscores what we have been hearing more and more, and that is the incredible variability in exploitation of the natural environment, something that we really haven’t taken enough consideration of over the years until quite recently.
ENVIRONMENTAL CHANGE AND SOCIOCULTURAL CAUSALITY

John A. Ware

Recent research on the Colorado Plateau (Euler, et al. 1979) has confirmed what archaeologists have long suspected to be a high correlation between environmental and prehistoric cultural and demographic changes. In this paper I will argue that in spite of these correlations, environmental explanations of cultural change are valid only when the environmental event is catastrophic—that is, when the environmental event is of sufficient magnitude or duration to alter or eliminate cultural systems or subsystems without regard to the adaptive variability expressed by the system.

The argument advanced in this paper is built upon a simple and familiar matrix game (Ashby 1956) that models the relationship between adaptive strategy variety and unpredictable environmental variety. The model illustrates several important relationships between adaptive systems and their environments, and provides insights into the evolution of cultural buffering mechanisms and the growth of sociocultural complexity.

Before discussing the model and its implications, we must define the relevant components of environmental variance.

Environmental Variety and Uncertainty

The variety expressed by a constantly fluctuating environment can be classified in a number of different ways. For example, Slobodkin (1968) makes a useful distinction between catastrophic and noncatastrophic variance. Catastrophic events that eliminate members of a population without regard to genotype “will have the effect of selecting for the ability to increase rapidly, without regard to the specific source of the catastrophe that initially depleted the population.” (Slobodkin 1968:200) whereas a noncatastrophic perturbation is normally perceived by the organism and evokes a series of specific behavioral and physiological responses to counteract the perturbation. In the event that these low-level responses are unsuccessful and a certain fraction of a population either succumbs or fails, due to the stresses imposed, to reproduce, then the genotype of surviving generations of organisms will be altered accordingly. Thus, noncatastrophic environmental events typically produce a hierarchy of responses on the part of the organism that may or may not lead to selective genotype changes.

Within the category of noncatastrophic variance we can make a further important distinction between predictable and unpredictable variance. Whether or not an environmental event is predictable or unpredictable has a major influence on the strategies employed to cope with the event. In coping with predictable environmental variance, an organism or adaptive system can formulate and employ an optimal
coping strategy. This is possible because the perturbation can be anticipated prior to the onset of change, and the most efficient of a wide variety of alternative strategies can be selected. In short, predictable variance allows an almost unlimited variety of responses; even high risk, radical responses can be used that could not even be considered in uncertain situations. As a result, predictable environment variance poses less of a challenge to the adaptive system than variance that cannot be predicted.

In coping with unpredictable external variance, an optimal response is usually impossible because the precise nature of the perturbation cannot be known in advance. The uncertainty that forces suboptimal responses to environmental disturbances, however, also forces the organism, population, or organization to maintain a certain degree of flexibility and response variety in order to successfully cope with a variety of unpredictable disturbances.

Thus, predictable and unpredictable environmental variance impose different . . . require much different response patterns . . . strategies on the part of the individual organism, population, or organization. In an environment with a high frequency of unpredictable variance, organisms and societies must maintain more adaptive flexibility and strategy variety than would be necessary in a more predictable world. According to Slobodkin and Rapoport (1974:197):

> . . . a newborn eutherian mammal must have the capacity to suck, but the world will supply the milk-filled teat . . . It does not require, at least at birth, a complexity of behavior sufficient to search out a mother. In short, predictable complexity in the world relieves an organism from having to have a complex response pattern to the world."

It will be argued below that the relationship between unpredictable noncatastrophic environmental variance and the variety of adaptive response options is one of the most important relationships . . . will in large part determine the effects of a given environment change on an evolving cultural system.

The Law of Requisite Variety

In his general theory of regulation and control in cybernetics, Ross Ashby (1956) provides us with a potentially useful model of the relationship between unpredictable environmental variety and strategy variety in adaptive or self-regulating systems. Let us suppose that a sociocultural system is attempting to control the outcomes of a variety of unpredictable environmental moves. Let us further suppose that the system is a subsistence agricultural society employing three different cultivation strategies (strategies A, B, and C), the success of each strategy being strictly dependent on precipitation from a varying and unpredictable environment. Let us specify that of three potential outcomes, a = high yields; b = moderate yields; and c = no yields; and that the society must score high yields in at least one strategy per turn (season, year) in order to survive.

If society R exercises all three of its cultivation strategies in anticipation of environment D's move, regardless of the amount of precipitation (assuming that precipitation does not fall outside the range of 8 to 20 inches), R is assured of high yields in at least one of its strategies. If R were to restrict itself to two strategies, let us say A and C, high yields would be assured only if precipitation remained between 12 and 20 inches. If for some reason R were to restrict its options to strategy C alone, then subsistence success would be strictly dependent on a very narrow range of precipitation values indeed.

The principle that underlies the relationship is called the Law of Requisite Variety. It can be summarized as follows:

> If R's move is unvarying, so that he produces the same move, whatever D's move, then the variety in the outcomes will be as large as the variety in D's move. D now is, as it were, exerting full control over the outcomes. If next R uses, or has available, two moves, then the variety of the outcomes can be reduced to a half (but not lower). If R has three moves, it can be reduced by a third (but not lower); and so on (1956:207).

Ashby's law demonstrates graphically the relationship between unpredictable environmental variety and the variety of strategies available to the adaptive system. That relationship is such that in order to ensure success in the existential game, an adaptive system must have a sufficient variety of responses to match the variety of unpredictable environmental moves. If, for whatever reason, unpredictable environmental variety should exceed response variety, a "negative imbalance" would exist that would threaten the survival of the system by increasing the probability of the occurrence of an environmental move for which there was no appropriate adaptive response.

In the event of a sustained negative imbalance in the variety equation, an adaptive system must either alter its strategy, migrate, or perish. There are a number of ways that a system can alter its strategy to correct a negative variety imbalance. Perhaps the most obvious approach is to follow Ashby's dictum, "only variety can destroy variety" (1956:207), and increase or diversify the variety of tactics in its strategy.

Let us assume, for example, that after several years of above average precipitation, R elected to drop cultivation strategy B in order to increase yields in its
two more productive crops. In doing so, he would be gambling that precipitation would remain above 12 inches. If precipitation should drop below 12 inches, R could correct the negative imbalance by reinstating crop B. Diversification consists, then, of increasing the variety of R's strategies in order to increase the probability of success in the set of all strategies, as well as to lower the impact of individual strategy failure.

The strategy of diversification is perhaps best exemplified by the hunter-gatherer. The typical hunter-gatherer can name literally hundreds of edible plants, of which he regularly exploits only a handful of favorite comestibles. For example, Lee (1968:35) notes that although some 85 species of edible plants are known by the Bushmen, over 90 percent of their vegetable diet by weight is derived from only 23 species, and that only 17 of a total of 54 game species classified as "edible" by the Bushmen were exploited on a regular basis. The spectacular resilience of the hunter-gatherer existence system lies in the variety of plant and animal resources that are not exploited on a regular basis, but which represent potential food reserves which the hunter-gatherer can fall back on if his priority resources fail.

Another characteristic of hunter-gatherer societies is that they often exhibit a fluid organizational structure that allows them the flexibility to exploit that which they need to survive. A hunting and gathering strategy focuses on the exploitation of seasonally and spatially fluctuating resources. As a result, the movement of individuals or families between bands or local groups often constitutes the most effective means of quickly adjusting band size to fluctuating resources. The process of interband "flux" has been reported by several researchers (Turnbull 1968; Lee 1969), and in order to accommodate the process, the local band organization usually exhibits sufficient flexibility to fission and disperse its members when food resources are low, as well as to accommodate new members when local resources are high.

In hunter-gatherer and other broad-based, low-energy societies, unpredictable environmental variety is effectively "matched" by the variety of subsistence/survival options and flexible social relations that permit the rapid exploitation of those options. In short, by keeping a wide variety of survival strategies continually open, and by diversifying whenever an unpredictable environmental event threatens the priority resource base, the hunter-gatherer exercises a passive control over random environmental fluctuation.

Diversification is an extremely effective strategy for coping with a negative variety imbalance, but the ability of an adaptive system to diversify is limited by a number of conditions. First, and perhaps most important, are the variety of strategy options that are available to the system, which is a function of the variety and spacing of resources in the environment. And second, is the ability of the group of exploit that variety, which in turn is a function of primarily three things:

1. a group's knowledge of their environment.
2. a group's technical ability to extract and process resources.
3. a group's tactical ability to extract and process key resources.

Tactical ability is largely dependent, in turn, on both group mobility and intergroup relations, both of which are influenced by a wide variety of factors such as population density, organizational flexibility, geographic constraints, and so on.

The importance of the first constraint on diversification is illustrated by the Hopi agricultural system. The core zone of Hopi agriculture exhibits a wide variety of qualitatively different physiographic cultivation zones, permitting a highly diversified agricultural strategy (Hick 1942). The diversity of the Hopi environment permits the Hopi farmer to grow subsistence crops in nearly every conceivable environmental niche as insurance against localized crop failure, which is a common occurrence on the semi-arid Colorado Plateau. Such a diversity of strategies would clearly be impossible in a more uniform environment where the number of potential agricultural zones was limited by physiographic redundancy.

Examples of the second class of constraints on diversification are even more numerous. The constraints imposed on mobility by sedentism and population growth is a case in point. It has often been noted that horticulturalists faced with crop failure would do better to adopt a more practical pattern of semi-nomadic gathering (Dittert and Shoewetter 1963), but examples of long-term or radical shifts to hunting-and-gathering by agriculturalists are extremely rare and are usually correlated with such factors as a precipitous decline in population, population dispersal or the introduction of a technological aid that increases the efficiency of a generalized hunting and gathering regime (e.g., the introduction of the horse in sixteenth century North America).

The reasons for this should be clear. Food producers typically live in population densities that far exceed the maximal density for successful year-round foraging. Moreover, successful hunting-and-gathering depends to a large extent on the seasonal mobility of the population, and mobility is one of the many adaptive options that the food producer must often sacrifice if he is to reap what he sows and store what he reaps. In addition, when food production is intensified it is usually at the expense of subsistence diversity. Not only does the reliance on a finite set of crops simplify the subsistence base, but clearing land for crops and rechanneling runoff to supply water to crops tends to simplify the local ecosystem, reducing or eliminating wild plant and animal resources that represent potential variety in the set of subsistence options.
Because of these constraints, diversification as an adaptive response to a negative imbalance in the variety equation is gradually replaced by more active control mechanisms as population and the commitment to sedentary food production increases, and each of these new mechanisms results in a sociocultural growth and elaboration.

Mechanisms of Sociocultural Growth

When the hunter-gatherer is faced with subsistence stress because of some environmental event, he can fall back on a wide variety of secondary resources, and his social organization is flexible enough to allow him to adjust the distribution of his population so that these resources can be exploited most effectively and with a minimum of conflict. Diversification remains a viable strategy in low-density horticultural societies where diversity is maintained in cultivation strategies and broad-based hunting and collecting options are preserved. However, when food production is intensified it is usually at the expense of subsistence diversity for the sake of food quantity and predictability.

The loss of strategy variety that usually accompanies the intensification of food production has two primary and predictable effects on the variety matrix:

1. It increases the likelihood of a negative variety imbalance.
2. It reduces the availability and effectiveness of traditional diversification options.

A glance at the variety matrix illustrates why there is an increased likelihood of negative variety imbalances following a decrease in strategy variety. It is a simple function of the reciprocal nature of the cultural-environmental relationship. That is, a negative imbalance in the variety equation can result from either:

1. An increase in unpredictable environmental variety
2. A decrease in strategy variety.

The two are, in fact, functionally equivalent. For example, when R employs all three of its cultivation options, variability in precipitation is essentially irrelevant to subsistence strategy success. But when R reduces its strategies, variation in precipitation becomes highly relevant indeed. By reducing its strategy variety, R has, in effect, increased the effective variability of its environment. It is as if the environment itself had become more variable. Simply stated, a reduction in strategy variety may be functionally equivalent to an increase in unpredictable environmental variety, and it may have identical consequences and implications.

The effect of food production intensification on such things as population density, organizational flexibility, mobility, and resource abundance has already been discussed and its impact on diversification strategies should be clear. There are, however, other techniques for correcting a negative imbalance in the variety equation. Three of these are:

1. Control
2. Enhancement
3. Alleviation

(1) Control. Control is the inverse of diversification. Instead of increasing the variety of R's strategies, an attempt is made to decrease the variety of D by controlling a fraction of D's moves. In Table 1, let us suppose that society R has limited its cultivation strategies to A and C so that precipitation values below 12 inches are lethal to the subsistence system. In attempting to cope with low variations in precipitation, society R can choose to diversify its strategy by adding cultivation strategy B which produces high yields under conditions of low precipitation, or it can attempt to control low precipitation fluctuations by direct technological intervention. For example, society R might employ water-control structures to collect and concentrate runoff, and thus transform D's move to an artificially higher precipitation value.
Control is the hallmark of modern agribusiness. By striving for "maximum sustained yields," modern monocrop systems have so reduced their internal resilience as to be totally dependent on a variety of environmental controls designed to keep external parameters such as weather, insect pests, and competing wild plant species carefully in check. These fragile man-made ecosystems are maintained at stable production levels primarily by the modern farmer's ability to predict and control a wide range of potentially destructive environmental "moves."

Examples of technological control solutions to problems of system resilience are abundant in the archaeological record. In the arid and semi-arid Southwest the most critical environmental variable to successful agriculture is water. It is not surprising that the history and prehistory of the Southwest is replete with examples of man's attempts to control, by technological and other means, the fluctuation in this key variable.

The prehistoric development and elaboration of water-control technologies in the northern part of the Southwest was particularly intensive between approximately AD 1000 and 1300 (Woodbury 1961; Rohn 1963; Shoenwetter and Dittert 1968; Plog and Garrett 1972). A number of investigators have suggested that this trend was a consequence of climatic change and environmental deterioration following the onset of drought conditions on the Colorado Plateau. According to Shoenwetter and Dittert (1968:54), "Technological means of water control came into wide use as environmental stress on agriculture intensified." Others (Plog and Garrett 1972; Zubrow 1972) have suggested that the rapid evolution of water control technologies during the eleventh and twelfth centuries may have had little to do with climatic change. Instead, they argue that population growth and changes in prehistoric "carrying capacity" provided the necessary incentive for the development of water control technologies.

Whatever the case, the development of control technologies in the prehistoric Southwest (and presumably elsewhere) resulted from the realization that external variables were exceeding acceptable ranges of variability, and conscious efforts were made to bring these "runaway" variables back into line. Whether environmental variability was perceived as excessive due to a decrease in subsistence strategy variety resulting in a contraction of the domain of subsistence stability, because of real changes in the environmental regime, or for other reasons, is a question that can only be resolved on the basis of empirical data from each region.

It is often the case that various technological control strategies evolve in conjunction with socio-religious and socio-political institutions whose function it is to predict and control the occurrence of fluctuation in key environmental parameters. There is really no coincidence here. As subsistence options decrease following economic specialization, or as the variety of environmental "moves" increases as a result of changes in the environmental regime, and consequently, as the variation in certain environmental variables becomes critical to the survival of the system, accurate prediction of such things as "rainfall" and "number of frost-free days" may become essential. Clearly, prediction and ritual control are less critical to the hunter-gatherer who keeps his strategy flexible enough to cope with virtually any unexpected occurrence. It is when man limits his strategies to achieve "maximum sustained yields" and other restricted goals, that presumption of knowledge about future events and prediction of future occurrences becomes important.

Moreover, successful prediction and ritual control of potentially pathological environmental change requires sophisticated environmental mapping and information processing. Consequently, specialized roles and/or institutions may evolve to process and manipulate this new information and influence or direct economic strategies accordingly. It is not surprising that these same "predictive" institutions may evolve the latent function of redistributing excess food resources among the population. After all, if an institution is ultimately held accountable for stress resulting from its faulty predictions, it may either improve its predictions or devise ways to circumvent blame by alleviating the stress. This principal may even be seen operating in egalitarian societies where explicit redistribution systems are either not present or only weakly established. Thus, Richard Ford (1972) describes a "paradoxical" aspect of Puebloan calendric ritual observances:

_Puebloan prayers and related sacred acts are intended to control the spirits behind the effective environmental variables, the most difficult of all to conquer and the very ones that modern climatologists wish they could control. But these ceremonies have the ecologically important latent function of prying surplus from household units for use by others, a most difficult task in egalitarian societies (1972:14)._1

(2) Enhancement. Enhancement is an attempt to change subsistence strategy outcomes by altering the relationships between D's moves and R's strategies. For example, in Table 1 the relationship between cultivation strategy A and precipitation input 1 is such that the outcome is always 'c', or no yield. Enhancement poses the question how determinate is this relationship, and furthermore, by altering the techniques associated with Strategy A, would it be possible to transform the outcome to a 'b', or perhaps even an 'a'? A change in the technology or techniques of Strategy A, such as the use of more efficient tools or a more efficient structuring of the labor force, might so change the matrix relationships as to produce crop yields where before there were no yields.

Enhancement may involve a technological solution
to loss of system resilience. In this sense, it is similar to Control. However, instead of attempting to control D's moves by direct technological intervention, Enhancement attempts to make existing strategies more efficient. This may involve changes in techniques and tool kits or changes in storage and food preservation technologies. It may involve changes in the organization of labor and an increasing differentiation of subsistence-related activities in order to create a more efficient division of labor.

Enhancement of the technology of production may involve both quantitative and qualitative changes in subsistence tool kits. That is, existing tools may be modified to make them more efficient, and new tools may be added to enhance the efficiency of the tool complex. Consequently, an enhancement strategy may affect the willingness of the group to adopt new innovations, particularly labor-intensive innovations that promise to increase the efficiency of a particular food procurement strategy, but only at the cost of an increase in labor expenditure.

The efficiency of a procurement strategy can also be enhanced by restructuring the labor force in such a way as to increase the effectiveness of strategy implementation. A given strategy can be intensified, for example, by restructuring or reconstituting work groups in order to achieve a larger or more cooperative task group. Such restructuring may ultimately involve the differentiation of individual tasks within the larger group, which often results in a more efficient and effective deployment of individual skills within the task organization.

Archaeological examples of technological and task organizational enhancement are abundant. One of the most often cited examples of technological enhancement from the prehistoric Southwest is the evolution of the food grinding complex during the Basketmaker-Pueblo transition (Bartlett 1933; Woodbury 1954; Plog 1974). In general, the trend in metate and mano form through time was characterized by a gradual increase in the size of the utility or grinding surface, which has been interpreted as an increase in food preparation efficiency in response to the increasing demands of an agricultural subsistence base (Plog 1974:138). That is, less efficient grinding surfaces gave way to more efficient grinding surfaces as agriculture assumed an increasingly greater relative importance in the subsistence economy of the prehistoric pueblos.

Recently, Gillespie (1976) has suggested that this simple technological efficiency hypothesis is inadequate for explaining the evolution of the slab metate-mealing bin complex, and he suggests that we look beyond the simple changes in tool morphology to changes in the systematic context of the grinding activity. Gillespie suggests that the stationary and highly specialized nature of the slab metate-mealing bin complex suggests not only a change in the efficiency of food preparation, but also a change in the nature of the social unit performing the grinding task. He hypothesizes that the Pueblo I through metate grinding complex is indicative of food preparation at the household level, whereas the evolution of large communal grinding facilities and highly specialized, non-mobile mealing bin complexes during Pueblo II indicates that the task of food preparation had been promoted to a supra-household level (1976:134–135).

Clearly, these two hypotheses are not necessarily contradictory. The promotion of such activities as corn grinding and other kinds of food preparation to a supra-household level can be a very effective means of enhancing the efficiency of the local task group, as well as reaffirming and strengthening the interdependence of separate households within the larger co-resident unit. Eggan made essentially the same point in his discussion of early Pueblo II social organization:

> The presence and distribution of grinding bins is of great importance, since I believe that the greater efficiency of group grinding and preparation techniques is an important factor in the development and maintenance of the extended maternal household in the western Pueblos (Eggan 1950:129).

Alleviation. Alleviation involves the elaboration and extension of social organizational mechanisms that function to alleviate the consequences of subsistence system failure. That is, rather than attempt to change subsistence system relationships or control external variables impinging on those relationships, an alternative is to alleviate the effects of decreased resilience within the system via social intervention.

Mechanisms for alleviating social and economic stress are ubiquitous in complex stratified societies. In our own society, social welfare programs, progressive tax structures, government subsidies to high-risk economic ventures such as farming and geologic exploration, etc., are all examples of social mechanisms that function to alleviate stress without actually attempting to rectify the deviant variables that caused the stress.

In simpler status societies such as the chiefdom, alleviation is achieved by means of a centralized redistributive economy which functions to redistribute surplus goods and at the same time maintain and enhance the power and prestige of the chief or ruling lineage. In egalitarian societies, which by definition lack the hierarchical political institutions for instituting and administering stress-alleviation mechanisms, alleviation is managed by kinship and ritual-based reciprocal behavior. Primitive reciprocity can range from simple gift giving and food sharing in times of need, to less altruistic and more economically-motivated acts of direct exchange of goods for goods or services, to impersonal varieties of exchange or barter centered on "getting something for nothing with impunity" (Sahlins 1968:193–196).

With the possible exception of the latter variety of "negative reciprocity," primitive reciprocity functions in an egalitarian setting to equalize the distribution of scarce food resources and thus alleviate the economic and social impact of localized or disproportionately
distributed subsistence failure.

One of the best examples of functioning alleviation mechanisms in the Southwest is provided by Richard Ford’s study of eastern Pueblo ritual-subistence ecology (Ford 1972). Ford points out that the unpredictable and spatially variable nature of environmental variability in the semi-arid Southwest often results in differential access to key subsistence resources, even within the same community. According to Ford, “The consequences of such an environment are manifest; one farmer can lose his entire crop while another can have a bumper crop; the next year the reverse may be true” (1972:6). It is the result of such environmental variability and resulting differential access to critical resources within the same community that, according to Ford, prompts the creation and elaboration of various social and ritual mechanisms designed to intercede in order to redistribute critical resources within the same community.

One such mechanism involves the notion of extending the etiquette of balanced reciprocity to the gods by assembling large quantities of food “offerings” during religious ceremonies to thank the gods for their benevolence. Since this food is subsequently redistributed to ritual participants or to the entire village, reciprocity with the gods amounts to a kind of “impersonal redistribution” of food surplus, and can function to alleviate stress in the community due to the frequent localized crop failures. To support his argument Ford points out that the annual occurrence of calendric ceremonies involving large quantities of food redistribution tend to coincide with the periods of greatest food scarcity in the village.

Food redistribution characterizes other aspects of Pueblo ceremonial life as well, including critical rites or rituals that correspond with changes in the individuals’ physiological, psychological, and social state of being, such as birth, death, illness, initiation ceremonies, and so on (Ford 1972). Each of these ritual events involves exchanges of food between households of the Pueblo, either as a form of “balanced reciprocity,” in which individuals who perform a service for another individual during the rite are reimbursed for their efforts with food, or a “generalized reciprocity,” in which excess food is simply given away during the course of the ceremonies.

In short, Ford demonstrates that the highly precarious subsistence base of the eastern Pueblos is made more resilient by both random (critical rites) and time-dependent (calendric ceremonies), ritual events that have the latent function of redistributing excess food to individuals and families in need. Ford also points out that these mechanisms may be supplemented during times of extreme stress by feedback-controlled alleviation mechanisms such as “gleaning,” cooperative planting and harvesting of the caciques’ fields, and in more extreme circumstances, community migration and warfare (Ford 1972:15–17).

To summarize, a sociocultural system may rapidly alter its strategies and structure to cope with the consequences of a shrinking domain of stability. I have discussed three mechanisms that ultimately serve to broaden the domain of subsistence stability and restore a measure of system resilience. Each of these mechanisms results in sociocultural growth and elaboration. Control and Enhancement imply technological growth as well as growth in the information processing and organizational subsystems of the social system. Alleviation mechanisms result primarily in the growth of the political and organizational components of the society.

It is important to note that these three mechanisms do not necessarily evolve or function in mutual exclusion. In fact, they generally co-occur and often tend to reinforce one another. For example, the development of environmental control strategies often requires an enhancement of the techniques of production as well as the elaboration of social institutions that function to reallocate labor and redistribute scarce resources.

It can also be shown that any one of these mechanisms may have the long-term effect of decreasing subsistence strategy variety (and in some cases, conceivably increasing the variety of environmental moves), so that positive feedback may evolve that exacerbates the very problems that selected for the mechanisms in the first place. That is, attempts to enhance the efficiency of one or several strategies may require increased expenditures of time and energy that might otherwise have been expended in other, perhaps more marginal, subsistence activities. Consequently, attempts to expand the domain of subsistence stability by enhancing a reduced set of subsistence strategies may result in a further contraction of the stable domain, requiring further enhancement, and so on and on.

Similarly, the intensification of certain food production strategies and various attempts to control the fluctuation of environmental parameters by technological means may have unforeseen and deleterious effects on the local ecosystem. We do not need to be reminded of the often drastic consequences of man’s various attempts to simplify and control his local and regional ecosystems. The long-term consequences of such control measures is often the introduction of new pathological variables into the subsistence equation. Thus, man’s attempts to control or reduce the number of environmental moves may have precisely the opposite of the desired effect, for through his efforts he may introduce new variables for which his subsistence system may be totally unprepared to cope, new variables that require further technological control if the stable system is to survive.

( Editor’s note: No list of references for this paper was received from the author. Likewise, two figures referred to in the draft were not supplied to us, and therefore references to the missing figures have been
Questions and Comments

Question: Do you know of anyone who is doing research on Anasazi food storage technology? In Third World countries even today, twenty to thirty percent of what is stored is lost, which is really a significant figure. I’ve not been able to find much of anything on it.

John Ware: All I’ve read are student papers, and I’ve heard seminar papers; I’ve seen very little published. I could come up with some references, but I’d have to think about it.

Comment: There’s not much on insect pests either.

Ware: I suspect that insect pests are much less of a problem in a diversified low-energy economy where you’ve got a diverse variety of crops. Insect pests are primarily a problem in modern high energy monocrop systems.

Comment: I wonder how many insect parts were missing in float samples. This would be a good thing to look for.

Comment: In a recent paper, I can’t recall the reference, they found insect parts preserved in sulphur, or fragments preserved in salt.

George Gumerman: I can wholeheartedly agree with your comments about the recent studies showing a concurrence of culture change with climate change. We have disavowed any sort of environmental determinism, and yet I think the implications in many of those articles are there that we have taken an environmentally deterministic stance. So we need this kind of paper to keep us on the straight and narrow.
PALEOENVIRONMENTAL CHANGES AND ANASAZI BIOCULTURAL ADAPTATIONS

Paul R. Nickens

Introduction

The Anasazi communities who once occupied much of the Colorado Plateau must be judged as having achieved successes and, on occasion, failures. Successful adaptation to what must be considered in general as a harsh and sometimes capricious environmental setting required flexibility and capacity for initiating changes in cultural patterns. Data continue to accumulate from geoclimatic/bioclimatic indicators and archaeological information suggesting that the Anasazi were greatly affected by recurring environmental fluctuations which brought about many cultural and demographic changes. Much has been written regarding adaptive adjustments in cultural patterns, especially in the context of subsistence-settlement systems. It should be noted, however, that full analysis of Anasazi adaptive responses to periods of climatic fluctuation requires input from a biological as well as from a cultural perspective since an interrelationship between biological, cultural, and environmental variables would have affected the adaptedness or maladaptedness of the prehistoric Anasazi tradition at any given time. One critical aspect of this interrelationship is the overall health status of the human population.

This paper explores the connections between the Anasazi health status and their environmental settings with special emphasis on one aspect of the interrelationship: the potential ramifications for Anasazi health status brought about by perturbations in past climatic regimes, particularly periods trending towards drought. Concomitant changes in Anasazi health status, and other biological changes which probably took place during marked and lengthy periods of climatic stress, have generally been overlooked in past analyses of Anasazi cultural dynamics. This deficiency is especially apparent when one looks for statements concerning the role deteriorating health status of a given community may have played in the decision-making processes for adaptive patterns during stressful periods. True, some cogent but broad treatments are available which discuss Anasazi health status and the environment (e.g., Kunitz 1970; Kunitz and Euler 1972; Woodbury 1965), but the bulk of human biological data are found as descriptive, and often obscure, appendices to archaeological site reports. It can be said, therefore, that few comprehensive attempts have been made to fully utilize the potential data for overall health status found in the many Anasazi skeletal series and to incorporate this information into discussions of the prehistoric cultural context. Considering the amount of Anasazi skeletal material, and the high level of development in
Southwestern archaeological methods and theoretical perspectives, it is surprising that we do not know more about Anasazi health status and the role it played in past cultural patterns.

Regrettably—and with apologies—this paper does not attempt an all-inclusive treatment of these topics, nor does it provide concrete results in the way of a case-specific analysis. Rather, the intent is to briefly discuss the concept of Anasazi health status and its relationship to environmental factors, and how it can be placed within a context of cultural adaptation.

More to the point of this session of the Anasazi Symposium, the aim is to examine the ways in which a better understanding of this important aspect of Anasazi life can contribute to a more complete awareness of adaptive modes employed by Anasazi. In this case we are concerned with potential changes in the health status which might have been brought about and/or made more important within the communities by periods of recurrent environmental stress.

**Anasazi Biocultural Adaptation**

The idea that observations on health and disease are of some importance to an ecological perspective on Anasazi adaptations is not a new one (e.g. Colton 1936), but, as previously noted, explicit, comprehensive discussions on the relevance of such observations to Anasazi cultural dynamics are generally lacking. Important contributions in this field are being made in anthropological studies of contemporary communities (cf. Alland 1970, Bennett et al. 1975, and Montgomery 1973, for summaries) and a biocultural perspective on prehistoric populations is gaining popularity (e.g. Blakely 1977; Dennell 1979; Santley and Rose 1979; Saul 1972; Wing and Brown 1979).

In the past, anthropologists have been rather well informed about long term evolutionary relationships of changing disease patterns and human history (Armelagos and McArdle 1975). However, an understanding of the significance of health and disease in short term events, such as climatic stress periods, is considerably less well developed. According to Montgomery (1973), a more explicit view of health and disease and the effects on human populations requires analysis of the following aspects. First, a culturally and ecologically relevant definition of health and disease is required (cf. Armelagos and McArdle 1975:1-3). Here, we may simply note that health and disease can be seen as varying states of an individual's, or even a community's, dynamic relations with an environment. Another aspect is the emphasis on variations of health and disease within particular segments of local populations. This perspective may be especially important in times of stress since it may be expected that population segments will be differentially affected during such periods. Third, it is important to delineate the influential environmental features of local habitats and ecosystems which may directly affect the health status of a population. A fourth concern is the changes in social and environmental contexts which may be related systematically to changes in patterns of health. Finally, consideration should be given to the implications of health and disease patterns for related systems and subsystems in the local population and local ecosystem.

Following the work for others (Blakely 1977), the linkage of sociocultural, environmental, and biological variables falls under the term "biocultural adaptations," which focuses on interacting biological and cultural relationships specific to the survival of particular human communities in particular ecosystem contexts. The goals of analyses exploring this complex interrelationship center on 1) delineation of human adaptation and stability in terms of the specific sociocultural elements that buffer or link individuals and populations to climatic, nutritional, and disease environments; and 2) the biocultural interactions that play primary roles in the regulation of demographic variables and their subsequent effects on economy, technology, and resources of the human population (Bennett et al. 1975:166-167).

The study and understanding of the biocultural adaptive aspects of a human population are difficult tasks to accomplish for contemporary groups, and especially so when dealing with archaeological populations such as the Anasazi. Analysis must include review of a large number of variables, internal and external to the population under study. Bennett, Osborne, and Miller (1975:166) suggest, for example, that answers to the following types of questions may be pursued:

1. What are the relationships between environmental variation and the behavioral, developmental, physiological, and genetic adaptation of individuals and populations?
2. What is the effect of environmental variation and social homeostasis, especially in terms of fitness, fertility, and morbidity?
3. What are the relationships between environmental variation and the growth and destiny of populations?
4. What are the bioenergetic relationships between the environment and human population in terms of nutrition?

Clearly, as students of the Anasazi tradition, we would like to have answers to these kinds of questions and others of biocultural importance since analysis of the finer variation within the biological systems of the Anasazi should give us additional ways to measure
change in the overall cultural system. It becomes a question, then, of how much can we realistically expect to learn and how do we go about gathering these data? The answer to the first part of the question is that we stand to greatly increase our knowledge of the Anasazi through a biocultural perspective, but at this point we can not really say how much since the interrelationships have not been heretofore analyzed in an explicit manner. As to the second aspect, there are in biological anthropology a growing number of methodological approaches which can help us gain this perspective. These include more accurate diagnosis of pathological conditions appearing in the skeletal series and the etiology of these conditions, the use of radiographic techniques to identify stress markers, and continuing development of trace element analysis of bone for paleonutritional implications. When we add the potential of such techniques to the array of archaeological benefits we have for the Southwest (e.g. good chronological control, paleoclimatic floral materials, and ethnographic information), it seems the data base is ripe for meaningful analysis of Anasazi biocultural adaptations.

Anasazi Biocultural Adaptations and Climatic Changes: A Preliminary Statement

We know that the Anasazi were afflicted by a wide range of health problems, but none so severe or deleterious that the general survival of individual communities or populations was in question. No data are available, for example, to indicate widespread epidemics or endemic health problems which may have played a major role in significant cultural changes. Nonetheless, certain conditions were extant which we believe, either through known cases or by inference, may have had a profound effect on the communities. Without a doubt, some of these would have increased in negative importance during times of stress, particularly those synergistically related to the nutritional situation. By way of an example of the interrelationship between biocultural adaptive variables and the environmental setting, we can briefly review the potential effects that periods of climatic stress probably had on the health status of the Anasazi communities by examining the interplay between diet, nutrition, and periods of climatic deterioration.

The fact that the Anasazi tradition, which occupied much of the Colorado Plateau between ca. A.D. 400 and 1500, had to cope with recurrent periods of climatic changes has long been known. Recently, however, syntheses of the paleoclimatic data and analyses of responses on the part of particular communities or regions to climatic changes have resulted in a new awareness of Anasazi-environmental relationships. Euler and his co-authors (1979) have posited that many Anasazi cultural changes were either directly or indirectly related to fluctuations in regional climatic patterns. Importantly, we are coming to realize that neither the severity of climatic changes nor the adaptive responses on the part of affected human populations occurred in exactly the same manner throughout the Colorado Plateau. Thus, while significant changes in the climate can be identified for large areas, local environments and local human populations show differential variations and responses on the part of the Anasazi.

A critical point here is that even though the perseverance of the Anasazi for several centuries reflects a generally successful adaptation, there were ups and downs. It is fair to say that virtually all Anasazi communities were affected to one extent or another by climatic changes throughout their history on the Colorado Plateau. To combat changes in the climate over the centuries, the Anasazi made use of several adaptive responses: shifting of settlements to more favorable locations, intensification of food sources, trade, and development of agricultural methods to increase productivity.

One effect of climatic stress on the communities over which the Anasazi would have had little direct control is a decline in health status which must have accompanied a dwindling of food resources. We will never be able to fully determine the extent to which a decline in the health status affected a given community, but it may be hypothesized that the effect was probably greater than we have previously suspected, particularly for certain segments of the population.

In general terms, changes in the environment may cause changes in the biological systems of a human population in several ways. In order of increasing magnitude and risk to the population, these responses include:

1. changes in individual behavior
2. changes in individual physiology
3. change in individual physiological acclimatization
4. changes in the death rate of individuals within a population
5. selective fecundity, fertility, and mortality within a population
6. deep genetic changes that affect anatomy, innate behavior, and other aspects of the population as a whole (Slobodkin and Rapoport 1974).

Viewing these possibilities as including a spectrum of Anasazi responses to periods of worsening climatic conditions, it is suspected that the first ones, resulting in minor individual reactions, probably would not have led to significant changes in the cultural pattern and, at the other end, times of climatic stress were too brief to have had lasting genetic effects on the
Anasazi population. It is, therefore, the middle-range biological responses, those related to morbidity, fertility, fecundity, and mortality, which are most likely to have become more prevalent during a stress period. Of critical importance in identifying possible changes in these health factors is the fact that nutritional stress, either malnutrition or under-nutrition, greatly increases the potential for deleterious effects occurring within a population.

We do not have time to completely address the interrelationships between adequate nutrition and reproductive performance, disease risk, and mortality levels, but it can be noted that a great deal of synergism occurs between these factors (see Santley et al. 1979:187–192 for a good succinct discussion). Significantly, it is the general reproductive performance of the population and the youngest members of the society which are most severely afflicted in times of stress, since poor nutrition can affect both fertility and fecundity of the females as well as create serious problems for those born into the society during stress periods. A review of the literature has shown that a woman’s nutritional status can have an effect on her chance of conceiving, upon the outcome of the pregnancy, and upon infant survival (Santley et al. 1979:187–188). Further, it is clear from a number of inquiries that adequate nutrition and protection from exposure to infection are requisites for reducing the risks of illness during the particularly vulnerable first two years of life.

Among the Anasazi, there is abundant evidence that the first two or three years of life were all important to an individual’s chances for survival to adulthood. It has been noted (Reed 1967) that infant mortality in the prehistoric Southwest may have approached 50 percent. Although this figure remains unsubstantiated to my knowledge, it is not inconceivable and certainly in line with infant mortality rates in Third World countries today where nutritional stress is prevalent. In any event, we do not know precisely what the infant mortality rate was for the Anasazi, but if we can assume for the present that the average may have been nearly one-half of those born, it is also probable that this figure increased during periods of climatic deterioration and concomitant nutritional stress. Aside from the probability of a high infant mortality rate, there is abundant evidence in Anasazi paleopathological literature of severe anemia (El-Najjar et al. 1976) and other non-specific stresses (Clarke 1978) occurring in Anasazi infant skeletons during the first three years of life. Additionally, Stephen Kunitz (1970; Kunitz and Euler 1972) has discussed the difficulties of progressing through the weaning phase and the effects they have on the chance for survivorship.

The net result of these conditions is that a youthful Anasazi may have only had a 50/50 chance of survival during normal climatic periods, and even less in times of environmental and nutritional stress. It may be argued at this point that nutritional stress does not necessarily correlate with climatic deterioration. This may not be the case in all situations; however, during extended periods of drought conditions, the years A.D. 1150–1195 for example, it has been shown that not only is the potential for domestic crops lessened, but wild floral and faunal resources are probably available at a reduced scale as well (Harlan and Dennis 1975; Nickens 1981). As a consequence, there seems to be little that can be done to increase nutritional intake during an extended and widespread episode of climatic stress. Indeed, we see many areas in the northern Southwest were abandoned during the latter part of the 1100s.

The importance of a situation such as this for analyzing Anasazi cultural dynamics is unanswered at this time. It may be that severe stress to the young Anasazi had a bi-directional effect. On the one hand, lower reproduction levels and a high infant mortality rate through the age of three would have resulted in fewer Anasazi reaching the reproductive/productive age and becoming contributors to the maintenance and continuance of the community. Once past the stressful early years, individuals apparently stood an excellent chance of attaining adulthood with relatively little difficulty (Palkovich 1980). Conversely, despite the distinct possibility for reproductive problems and high infant morbidity and mortality rates, it may be that the result was a smaller but more fit adult population. Some studies (Lovejoy et al. 1977; Meindl and Swedlund 1977) have indicated that high infant mortality rates may in fact result in increased survivorship during the subsequent adult years since those individuals who survive the high disease risk early years are biologically suited to cope with health difficulties in the later years of life.

Concluding Remarks

In this paper we have reviewed the general basis for analysis of Anasazi biocultural adaptations and have alluded to the fact that such a perspective should aid us in understanding Anasazi cultural changes, particularly responses to periods of climatic stress. I believe we can postulate without difficulty at this point that periods of climatic stress would be paralleled by concurrent periods of nutritional stress. During such times, a general decline in the overall health status of the population could be expected within certain segments, especially the youngest age class, suffering the worst effects. In the archaeological record, periods of climatic stress accompanied by observable changes in demographic patterns should be reflected in the dietary and human skeletal data. There are many possibilities for identifying the expected changes in health status, each of which must be examined in conjunction with cultural and environmental variables.
We should expect, for example, increases in nutritionally related skeletal pathologies such as evidence for anemias and other generalized stress indicators. Better data are needed to be able to address the role of diet and nutrition to the overall health status of the Anasazi and what happened to this important component of the adaptive situation during stress periods. Here, trace element analysis for nutritional implications and more intensive analyses of such things as coprolites and Anasazi foodstuffs would add immeasurably to our understanding of Anasazi adaptive modes. However, the role(s) that biocultural adaptive considerations played in changing Anasazi cultural patterns will remain unanswered until we start taking a finer look at the interrelationship between environmental, cultural, and biological variables in the Anasazi archaeological record.

Questions and Comments

George Gumerman: This has long been a neglected aspect of Southwestern archaeology. If any of you are interested in this particular subject, I can recommend for you a publication. It is called “Aspects of Southwestern Paleoepidemiology” by Kunitz and Euler. It was published by Prescott College Press back in “ought-six” or something like that, and I think the only way you can get a copy of it is by writing to either Kunitz or Euler. I think it lays the groundwork for some of the things that Paul Nickens was talking about. I feel that this line of inquiry into human adaptation could benefit from the kinds of studies that are being done in the Pacific, especially Melanesia where John Tyrrell and other students have devised some techniques they call biogeographical techniques, that go along this line. I think these would be quite productive in Southwestern society.
Introduction

This paper will examine an example of an unique and abrupt shift in Anasazi settlement patterns which has been recorded at several widely separate locations on the Colorado Plateau. According to Euler et al. (1979:1090), some Pueblo II populations left the lowlands and canyon bottoms where they had practiced floodwater farming and moved to upland areas after AD 1000. Examples of such settlement relocations, thought to have endured for only one to two centuries, have been documented for the Rainbow and Defiance Plateaus as well as northern Black Mesa in the western region of the Anasazi culture area (Euler et al., 1979:Figure 1). The same canyon bottom to upland relocation has been recorded on the northeastern perimeter of the Anasazi culture area at Chimney Rock Mesa, southern Colorado (Eddy 1975, 1977). Because these diverse Anasazi populations moved up the elevational gradient in a synchronized manner, it is not unreasonable to suppose that they were responding in an adaptive way to region-wide climatic factors, the nature of which is not fully understood.

Our objective in this paper will be to employ Chimney Rock as a case study in order to briefly review why this lowland-to-upland settlement relocation might have taken place. However, of more importance will be an examination of how the settlement shift was maintained at Chimney Rock once prehistoric populations had been implaced on the elevated mesa and ridge tops. Particularly, the limiting factor of potable water supply will be investigated as this critical factor affected upland adaptation in areas away from perennial streams and rivers.

Cultural Background

The prehistoric archaeology of Chimney Rock is located in the San Juan National Forest, Archuleta County, where it forms an archaeological district which has been withdrawn from commercial leasing beginning in 1957 (Figure 1b). This 6.12 square mile preserve lies in the upper San Juan River basin between the Piedra River and two of its tributaries—Stollsteimer and Devils Creeks. Intensive site survey conducted between 1970 and 1971 revealed 91 prehistoric Anasazi sites dated by ceramics and tree-rings between AD 925 and 1125. The Chimney Rock sites were made up of 217 architectural structures (residences and kivas) and 27 transient camp/workshop locations. Three of the architectural sites (5AA83, 86 and 88) were partially excavated to provide outdoor museum displays for the planned United States Forest Service interpretive development. A mound at another site (5AA92) was cleared so as to salvage information before its destruction by a new
Figure 1. Maps of the Chimney Rock Archaeological Area. a, Map of the seven named site groups, isolated archaeological sites, and numbered ravines; b, Map showing the location of the Chimney Rock Archaeological Area as of 1957 (reproduced from Eddy 1977: Figures 1 and 4).
The settlements of Chimney Rock have been classified into seven named site groups as well as 15 isolated sites (Figure 1a). The clustering of these site groups was originally identified intuitively by Eddy (1977:Figure 4) and later verified objectively by Tucker (1981:Figure 8) using Nearest Neighbor analysis and a computer generated Z-coordinate contour map procedure.

The archaeological data recovered in this manner provided the necessary information to define a new cultural unit—the Chimney Rock Phase. This indigenous cultural pattern contrasts with one distinctly different site, the Chimney Rock Pueblo (5AA83) which is thought to be evidence of an intrusive Chaco colony appearing in AD 1076 according to stratigraphic and tree-ring data.

Much of the diversity of the Chimney Rock Phase settlement can be summarized in terms of two distinctive patterns. Mesa-top settlements were more dispersed, contained fewer people, practiced seasonal movements for farming and big game hunting, hauled water, and infrequently used riparian resources. By way of contrast, the Piedra River settlements located on the high terraces above the river, tended to be organized more compactly around plazas and with more use of Great Kivas; they contained more people, did not have to practice seasonal movements for farming, did not have to haul water any great distance from the river, and had more convenient access to riparian resources.

Environmental Background

Chimney Rock Mesa is a steeply dipping cuesta which rises from 2,000 m elevation to an altitude of 2,303 m. The landform is divided into two portions, an upper and a lower mesa interconnected by a narrow causeway. It is upon the upper mesa, a small, triangular rock platform, that the Chimney Rock Pueblo was constructed, while the bulk of the indigenous archaeology of the Chimney Rock Phase is to be found on the lower mesa. Two freestanding pinnacles of rock form the well known chimneys. These dramatic spires are located on the northeast end of the upper mesa where they tower to 2,395 m elevation overlooking the Chacoan colony of 5AA83.

In addition to archaeological survey and excavations whose purpose was to study prehistoric settlement, land use, and population size, a number of subcontracted environmental studies were conducted by specialists. These include investigations into pollen, charred plant parts, and animal bone. Study of fossil pollen extracted from the soils found within the excavated prehistoric sites revealed to Buge and Schoenwetter (in Eddy 1977: Appendix B) that "...in general, all of the fossil samples indicate conditions to have been drier during the Chimney Rock phase..." by comparison to the present. Further, there is a general trend in which the drier pollen samples fall late in the time series, suggesting maximum aridity around AD 1100. These interpretations, which are based on the ratio of tree-to-nontree pollen percentages, give good support for the why question asked at the beginning of this article. It would follow that the Chimney Rock people were seeking higher elevations away from the drier axial center of the San Juan Basin with relatively more moisture for slope runoff and dry farming purposes. However, this conclusion is at odds with those of Euler et al. (1979:1096) who finds that: "The interval AD 950 to 1150 is one of the best documented periods of increased effective moisture on the Colorado Plateau." Again, they argue "...that annual precipitation was substantially higher in AD 950 than it is now." Indeed this may have been true for some localities on the Plateau; however, the Chimney Rock data strongly suggests a local anomaly near the base of the San Juan Mountains with climatic conditions quite reversed.

Other environmental studies, based on plant parts and animal bones, led Robinson, Harris, Minnis, and Ford to conclude that there was ample winter time precipitation, principally in the form of snow pack (Eddy 1977:Appendices A and C). In conclusion, the auxiliary environmental studies suggest that the total annual precipitation during the Chimney Rock Phase was lower than the present, but that a larger proportion fell during the winter when it could be stored as soil moisture (Eddy 1977:66). Unlike today, the mesa-top was apparently forest covered with Ponderosa pine, and some high altitude mountain animals had their range depressed to the elevation of Chimney Rock.

Regional Dendro-Climatic Data

In an effort to resolve the issue between the Chimney Rock climatic interpretations based on pollen made by Buge and Schoenwetter which is in seeming opposition to the climatic interpretations of Euler et al. (1979:1096), I turned to the tree-ring evidence for climatic change developed by Robinson and Dean (1969) for the Four Corners region of the Colorado Plateau. By good fortune, this analysis covers the two centuries between AD 1000 and 1200, as graphed on Figure 2, and thus bears on the question of upland occupancy and final abandonment of this portion of the Upper San Juan Basin. The dendro-climatic data is composed of tree growth means and standard deviations compiled from a network of 23 stations, each of which is a small climatically homogeneous area. Although Chimney Rock is not one of these stations, due to its proximity lying as it does near and just to the northeast of Gobernador, New Mexico (Paleo-climatic Station 18) and Mesa Verde, Colorado (Station 12), it seems quite safe to extrapolate contour values to Chimney Rock. The network data was next transposed by Robinson and Dean to decade isochronic maps. Data was plot-
Upland Anasazi Settlement Adaptations . . .

Figure 2. Graph of tree-ring departure values, AD 1000–1200.

The maps are contoured with isopleths representing equal departure values of +2, -2, +4, and -4. Areas with positive or negative values equal to or exceeding 4 are also shaded to emphasize the significant departures from normal (X) (Robinson and Dean 1969:3-7).

For the Four Corners region as a whole, Robinson and Dean (1969:7) note high positive values during the decades AD 1110–1129, while low negative departure values fall into three sets, AD1030–1049, 1090–1109, and 1140–1189 as shown by cross-hatched blocks on Figure 2. Modeling of the relationships between climate and tree growth suggests that low precipitation generates high temperatures, low soil moisture, and a narrow tree-ring (Robinson and Dean 1969:Figure 1). Therefore, decades of low negative departure values can be equated with low precipitation, while high values are wet years.

When the common variance for the nearby tree-ring stations is extrapolated to Chimney Rock, it can be seen that seven decades were wet, three normal, and ten were dry (Figure 2). Further, the three wet and one dry regional intervals consistently agree with the local wet-dry peaks and troughs of the extrapolated Chimney Rock polygon. From this synchronized patterning it follows that when the entire Four Corners Anasazi were stressed by subnormal rainfall, then so was Chimney Rock. Further, these centuries were, on the whole, characterized by subnormal rainfall and thus stressful for farming. In addition, they were erratic with wild swings from dry to wet and return leading to uncertainties in farming strategies.

Therefore, these data strongly support the pollen interpretations of Buge and Schoenwetter rather than the climatic reconstruction of Euler et al.

Unfortunately tree-ring data is not available to treat the problem of why the upland Chimney Rock settlements were founded in the first place, since this event
is thought to have taken place in the 10th century, an interval of time which preceeds the data plotted by Robinson and Dean. However, pertinent data is available to deal with the issue of the effect of climate on upland occupation. Figure 2 shows a tree-dated building spurt for four High Mesa site excavations (5AA83, 86, 88, and 92), and these have construction dates (C, B or r suffix) ranging between AD 1076 and 1093 (Dean 1975:82-85). Most but not all of these dates correlate well with a three decade wet interval falling between AD 1060 and 1090. It can be concluded that ample precipitation provided ample crop yield and therefore release time from subsistence pursuits to engage in building activities. It is of interest that roofing timbers for 5AA83 and 88 were cut during the summer during June and July, according to Robinson (Eddy 1977:44). This unusual pattern of summer harvest differs from the usual Anasazi custom of winter-time felling which avoided the summer agriculture schedule. Thus, at Chimney Rock wet years produced such an assured crop of corn and beans that agrarian laborers were able to take off in the very middle of the agricultural season to fell and stockpile construction timbers including Ponderosa pine, some Douglas-fir, some real fir, a little juniper, and aspen (Eddy 1977:65). Thus, upland adaptation is in part based on a correlation between wet years and architectural activity. But what about the effect of subnormal rainfall revealed by the low departure values (Figure 2)?

Cold air drainage and shading is a final temperature factor affecting local human adaptation. Heavy cold air subsides at night to fill the bottoms of narrow canyons which bisect the Colorado Plateau where it laps up against the base of the San Juan Mountains. In order to avoid this cold air layer prehistoric settlements at Chimney Rock were built on upland terrain well above this chilled air mass. Such a vertical distribution, favoring the uplands and higher terrace (T2-T5) building sites, is graphed on Figure 3 which shows the elevational spread of Chimney Rock sites, a vivid documentation of settlement adaptations.

The precipitation factors to which the Chimney Rock farmers adapted include a winter-dominant rainfall pattern, a low total annual rainfall, and highly unpredictable year-to-year rainfall. The winter-time frontal storm pattern leading to heavy snow pack would have provided certain advantages to upland occupation. The most important of these is the available moisture for house construction to be obtained from

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**Figure 3.** Histogram of sites and structures plotted against elevation in meters (modified from Eddy 1977: Figure 9).
Upland Anasazi Settlement Adaptations...

lingering snow banks present on north facing slopes. Construction of the thick-walled masonry houses entailed large quantities of mud mortar requiring water during the puddling stage. The haulage of this water by back-packing from canyon bottom streams would have been prohibitive work. Further, this same snow bank source would supply water for drinking and cooking during the fall, winter, and spring leaving only the summer months for potable water haulage and storage, a matter to be considered at greater length in a moment.

Further, winter dominant precipitation would favor high retention of soil moisture of advantage in early spring planting and germination of the maize crop. Farming using runoff at the mouths of ravines would also be favored, as would dry farming on terraces and even on thin upland mesa-top soils.

But less total annual rainfall would likely mean summer drought. To combat this limitation, late plantings could have been made on north facing slopes with maximum soil moisture due to shading. And finally, to combat year-to-year variability in precipitation and temperature, a characteristic of the unstable southwestern environments, a mixed strategy of farm field sites would provide coverage over many risky options.

Water Transport Study

In order to examine the hypothesis that potable water was hauled from perennial streams to elevated domestic residence, I graphed the frequency of water jars, straight-line distance to nearest permanent water, and elevation of residential site (Eddy 1977:Figure 10). From this crude analysis, conducted on data from only six archaeological sites, the predicted relationships held. A trend could be detected in which the frequency of water jars rose as did the distance to perennial water (Figure 4). Thus, the main adaptive pattern sketched above was independently verified by this more objective and empirical test. However, due to the small amount of data and lack of statistical measure in this graphical procedure, I will now broaden the scope of the water transport thesis. This more refined study will be conducted according to the "Method of Hypothesis Testing."

During the latter part of the Chimney Rock Phase, decades of deficient rainfall are recorded between AD 1000–1020, 1030–60, and 1090–1100. Although these intervals ranged between one and three decades of local drought at a time, and two were regionally severe, it is obvious that human occupation at Chimney Rock endured. Surprisingly, the termination of the Chimney Rock Phase took place not during a dry period but instead during a regional wet interval dated between AD 1110 and 1129. At Chimney Rock this wet interval consists of a significantly high departure of more than +4 followed by a normal (X) decade. However, the succeeding four decades of rainfall were subnormal, lying between AD 1130 and 1170, culminating in "... an interval of extremely dry conditions prevailing in the Four Corners region" (Robinson and Dean 1969:7). So, in fact the Chimney Rock farmer, who certainly was a good comparative judge of year-to-year rainfall and crop productivity, witnessed a decline in rainfall values beginning in AD 1130 and continuing through AD 1170. My dating of the Chimney Rock Phase abandonment, a small part of the regional evacuation of the entire San Juan Basin which was terminated by the Great Drought of AD 1276–1299 (Antevs 1955), may in fact be off by a decade or so. It is probably safer to say that abandonment of this portion of the San Juan Basin began around AD 1100 and ended about AD 1150. Continued forty years of drought likely exceeded the farming capacity of these prehistoric Anasazi despite the advantages of an upland settlement shift.

Prehistoric Adaptation

Having suggested that the prehistoric Chimney Rock peoples of the 11th century were moving into the uplands to avoid drought and optimize soil moisture for farming purposes, let us now examine the higher elevation environmental factors to which they had to adapt. Given their peripheral position on the edge of the Anasazi culture area and the marginal position for corn agriculture, it seems reasonable that they were stressed by new and constraining environmental variables which offered challenges to be met by cultural responses. Among the most stressful environmental parameters were precipitation and temperature variations. Such climatic factors are often of an unpredictable nature requiring massive technical response not just to the mean values but, more significantly, to the seasonal and annual extremes.

Response to extremes is most likely in two sectors of technology: agricultural practices and architectural design. Agricultural technology had to contend with a short growing season, severe winter cold, and cold air drainage and shading. To meet these challenges, the prehistoric farmer must have selected field sites and seed corn that would overcome killing frosts and midsummer drying of the soil. An appropriate response would be for early spring planting on southeast-facing field sites to maximize solar warming while still capitalizing on snow-melt soil moisture. Support for this strategy is to be found in many field-side camps which favor a southeast exposure. Minnis and Ford tested the hypothesis of field stress and found that, unlike their expectation, the recovered corn and beans
were quite uniform in their size measurements, indicating near optimum growth conditions, a measure of a successful adaptation (Eddy 1977: Appendix C).

Severe winter temperature is a second factor to which the Chimney Rock occupants must have adapted. Modern temperature records from nearby Pagosa Springs indicate that the fall, winter, and spring months all have minimum temperatures below freezing. Therefore, only well constructed and comfortably heated housing would keep these farmers protected. To do so, well insulated masonry surface rooms were built with extra-thick walls (greater than one meter). They were outfitted with interior fireplaces for warming the room, and ventilated with tunnel and shaft for fresh air circulation. Many of the domestic rooms, both circular and rectangular-shaped, were massed to increase the total insulation effect (Eddy 1977:70).

Method of Hypothesis Testing

The Method of Hypothesis Testing is a deductive research strategy made up of propositions, hypotheses, test implications, and test statistics with region of acceptance or rejection specified (Hill 1970). This deductive design is advocated by Braithwaite (1968) so that general propositions are systematically rewritten as specific numbered hypotheses and tests. At each step in the deductive progression, the provisional explanations more closely approximate archaeological reality.

In this analysis, bivariate correlation of variables will be made using the Pearson R correlation coefficient. Acceptance or rejection of a significant correlation will be based on a probability of 0.05 or less. This significance level is arbitrarily chosen but it is one generally accepted in social science research.

Proposition

The following study is drawn from a generalized proposition which states that in arid lands, water is the single most limiting determinant of residential settlement location. It follows that settlement in mesic environments with ample rainfall and surface water supplies will not be restricted in its distribution over the landscape. In contrast, permanent settlement in arid lands will be restricted to perennial water courses and other assured sources of water such as lakes, ponds, and/or springs. However, this limitation can be partially overcome in certain circumstances through the use of technology. Southwestern examples are water control features such as irrigation ditches, reservoirs, check-dams, walk-in wells, and deep shaft wells (Woodbury 1961). But willingness to haul water from

![Figure 4. Graph showing trends of vessel shape classes by site elevation and distance from nearest potable water source (modified from Eddy 1977:Figure 10).](image-url)
source to dwelling is another viable means of overcoming the limitation of surface water supplies. This option seems to have been favored at Chimney Rock where there are no known water control features of Pueblo II age. At Chimney Rock, the challenge of hauling potable water during the summer for the highest site group communities must have been considerable. The most elevated community, the High Mesa site group, is between 182 and 303 m above the nearest perennial stream. Historically, it is known that modern Pueblo Indian communities such as Acoma or Hopi, which are situated on high mesa tops, hauled water for kitchen use. Women transported domestic water in jars by balancing the vessels on their heads and steadying the container with one hand. However, at Chimney Rock, the horizontal distances of up to 1,970 m and vertical climb of up to 303 m suggest that some other container, such as a light-weight pitched basket or leather bag, may have been used to reduce the rate of breakage and weight of the water transport container. Negative evidence for this thesis is the lack of many broken water jar sherds to be found as isolated finds between streams and archaeological sites. It seems more likely that the large, fragile, plain gray pottery storage jars were simply left in place at residential sites while water was hauled in light-weight, flexible water-proofed bags outfitted with a tump line. The jars, then, became the stationary storage cisterns. However, it would follow that the quantity of broken jar sherds is a very good measure of the increasing need for potable water storage during the summer drought as one moves farther and farther away from the canyon bottom perennial streams.

### Hypotheses

The proposition having to do with the limiting role of water and its effect on arid land settlement is rewritten here as a series of numbered hypotheses. By definition, a hypothesis is a provisional explanation to be considered a valid conclusion only if it can be accepted after empirical testing against some body of data. In this Chimney Rock study, the hypothesis will involve predicted relationships between three variables: water jars, distance to water, and site elevation. Each hypothesis is framed in the “if . . . , then . . . ” format so that the preceding “if . . .” part sets the conditions for the subsequent prediction which is the “then. . .” part of the hypothesis. In turn, the test implications follow from the “then” prediction so that if the indicated facts are present in the archaeological data, then the hypothesis must be accepted. But by the same token, if the test implications do not appear in the data base, then the hypothesis must be rejected. Although not true of all hypothesis testing, in this study testing of the hypothesis will involve numerical variables so that statistical measurements are feasible. Statisticians use a hypothesis testing design in which the null case is set in opposition to the alternative hypothesis (Lapin 1975: Chapter 8). To accept the alternative case (Hₐ), it is first necessary to disprove the null hypothesis (H₀). The alternative hypothesis may be written in as many statements as is appropriate, usually differentiated by subscripted numbers in the form: H₁, H₂, . . . , Hₙ. The null hypothesis, the target one tries to disprove, is a negative statement of relationship. Null, meaning no, predicts that there will be no significant difference between and among the measured variables, in this case water jars, water distance, and site elevation. The Ho prediction, then, is that numerical measurements among the critical variables will show no significant relationships. That is, the test statistics will show probability values greater than 0.05 and therefore any correlation could be explained as a chance event, not a culturally significant one. By the same token, the alternative hypotheses, to be accepted, must yield a test statistic with probability values of 0.05 or less (p = 0.05).

The Chimney Rock hypotheses are as follows:

**H₀**: If water is not the most critical determinant of settlement location, then there will be no difference in the frequency of water jars, site elevation, and distance from site to nearest perennial stream.  

**H₁**: If water is the most critical determinant of settlement location, then, the frequency of water jars will increase as does site elevation.  

**H₂**: If water is the most critical determinant of settlement location, then the frequency of water jars will increase as does the distance to the nearest perennial stream.  

#### Test Implications:

- **T₀**: There will be no relationship in the frequency of water jars, site elevation, and distance to nearest perennial water.  
- **T₁**: As the frequency of water jars increases, so will the elevation of sites on which those jars sherds occur.  
- **T₂**: As the frequency of water jars increases, so will the distance to permanent water for those sites on which the jar sherds occur.  

#### Critical Variables:

The three measures of water transport and storage during the summer drought are water jars, site elevation, and distance to nearest perennial stream. These variables were quantified in the following manner:

- The first variable is called water jars (WATJARS). This measures is the percentage frequency of plain gray jar sherds thought to have been employed for storage and service of drinking water. The Chimney Rock ceramic collections are divisible into three form classes: wide-mouth corrugated jars, narrow-necked plain gray jars, and painted bowls (Eddy 1977:25). The wide-mouthed jars are interpreted as cooking vessels because of the shape and soot film often found adhering to the textured surface. The narrow-necked jars are interpreted as service vessels because of their narrow necks and the presence of soot film on the necked portion.  
- The second variable is site elevation (SIELVA). This measures is the percentage of water jars found at sites between and among the measured variables, in this case water jars, water distance, and site elevation. The Ho prediction, then, is that numerical measurements among the critical variables will show no significant relationships. That is, the test statistics will show probability values greater than 0.05 and therefore any correlation could be explained as a chance event, not a culturally significant one. By the same token, the alternative hypotheses, to be accepted, must yield a test statistic with probability values of 0.05 or less (p = 0.05).

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Eddy jars were likely used for storage, transport, and pouring of water due to the constricted neck shape and lack of soot encrustation. And finally, the painted bowls, based on their shape, small size, and painted decoration, are thought to have been employed in food service. The critical measure, then, is the changing ratio of narrow-necked jars to those for cooking and food service. The actual Water Jar percentage is based on a count of plain gray sherds divided by the total pot sherd count (n) for that site. To insure statistical reliability, only those sites with an N of 50 or more were utilized. This rule yielded a sample of 18 useful sites. Counts and percentage calculations were made from the laboratory notes by Roy Reaves of Interagency Archeological Services. The range of jar sherd percentages is 50.1 percent with minimum value of 18.5 per cent and a maximum of 68.6 per cent (Figure 5).

The next critical variable to be considered is elevation of each site in the useful sample (ELEV). No attempt was made to calculate the vertical height of the site above the nearest point on a permanent stream; rather the raw elevation measured in meters above mean sea level was taken from the USGS topographic map for statistical analysis (Figure 1a). The range of elevation values for the 18 sampled sites is 297 m. extending from a low of 1,958 m. to a high of 2,225 m. elevation. This range is generally representative of the total inventory of Chimney Rock sites and structures shown on Figure 3. The bimodal pattern of structures and/or rooms shown on Figure 3 is reproduced on the scattergram computer plot reproduced here as Figure 5. This uneven distribution of sites is accounted for by the fact that the largest population aggregates were found at the lowest and highest elevations while the steep slopes and stony soil of the inter-

![Figure 5: Scattergram graph showing percentage of water jars (Y-axis) regressed against elevation of site (X-axis). The trend of the points is shown by the estimated regression line summarized by the regression equation.](image-url)
mediate elevations were avoided by prehistoric settlements.

The final measure is the variable called distance (DISTANCE). This is a straight-line measure in meters from each site in the useful sample to the nearest point on a perennial stream. Three nearby streams flow all or for a good part of the year today. These are the Piedra River, Devil’s Creek, and Stollsteimer Creek (Figure 1a). Intensive pedestrian survey of the Chimney Rock Archaeological Area did not reveal any major seeps, springs, ponds, or other natural tanks which would have supplied water sufficient to support any upland site or site group community. Therefore, use of permanent stream waters during the dry summer months was a necessity. The 18 useful sites in our study sample yielded values from 91 m to 1,970 m straight-line distance from site to nearest perennial water (Figure 6).

**Computer Study and Test Statistics**

Analysis of the data was carried out using the CDC Cyber 172 computer housed at the University of Colorado Computing Center, Boulder, Colorado. The data was coded on center punch cards for statistical manipulation by Version 8, Statistical Package for the Social Sciences (SPSS), a set of canned programs written by Nie et al. (1975).

The particular SPSS subprogram used to analyze these data is called SCATTERGRAM. This subprogram constructs a graph of data points consisting of two paired variables such as WATJARS with DISTANCE or with ELEV (Figures 6 and 7). Statistics output for each scattergram plot are: simple linear regression including slope of the regression line (B), the intercept with the vertical axis (A), the Pearson product-moment correlation statistic (R), R-square, the statistical significance of Pearson’s R, and the standard error of the estimate (Table 1).

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Figure 6: Scattergram graph showing percentage of water jars (Y-axis) regressed against distance of site from nearest perennial stream (X-axis). The trend of the points is shown by the estimated regression line summarized by the regression equation.
Water Transport Results

Results of these two scattergram analyses are illustrated on Figures 5 and 6 while the correlation statistics are shown on Table 1. The graphs are summarized by regression equation lines, both of which indicate a positive correlation in which an increase in either horizontal distance or vertical elevation is matched by a corresponding increase in percentage of water jars. From these results, the null hypotheses of no relationship can clearly be rejected. Further support for the acceptance of both alternative hypotheses is provided by the highly significant Pearson R correlation statistics (Table 1). Again these bivariate statistics indicate a positive correlation between the three variables, with probability measures well below the 0.05 level of significance. The R-squared statistics shown on Table 1 serve as a measure of variance explained, 47 per cent for the first pair of variables and 53 per cent for the second pair.

Based on these empirical findings, it seems clear that upland settlements of Chimney Rock Mesa were maintained during the dry summer by hauling water for domestic needs over considerable distances. Further, these empirical tests lend considerable support for the general model of upland adaptation which has been constructed by more subjective and qualitative reasoning.

Questions and Comments

Question: Is there any indication that water jars might be related to increased population, aside from volume?

Frank Eddy: That’s why I threw in the graph on numbers of sites by elevation. In fact, there is slightly more mass to that histogram on Figure 2 down close to the river than higher up, so I would say probably not, that in a crude sense I have controlled for that variable. But these are counts of water jars on an individual site and then the paired measurements of distance to the stream and elevation of that site. I had enough data on 18 out of 91 sites, so that’s my sample that I am using here in my scattergram analysis.

Jim Judge: Each dot on the graph, on the scattergram, is a site, is that right?

Eddy: Yes, represents a pair of measurements at one site location, so that is a sample of 18 sites. I would have used more if we had had more data.

Judge: The impression is that all of the sites are the same. Are they all of equal size and function?

Eddy: They are all residential sites.

Judge: Are they of equal size?

Eddy: No, that’s not true. And we were working in a day before much fancy statistical collection techniques were in vogue so that we were doing grab sample collection of data too, which is another factor that skews, but I would imagine that you are cancelling out your error there, pretty much.

### Table 1. Correlation Statistics for Three Water Transport Variables

<table>
<thead>
<tr>
<th>Statistics for Water Jar Frequencies and Site Elevation:</th>
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<tbody>
<tr>
<td>Correlation (R)</td>
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<td>Std Err of Est</td>
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<th>Statistics for Water Jar Frequencies and Distance to Nearest Perennial Stream:</th>
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<tr>
<td>Correlation (R)</td>
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<td>Std Err of Est</td>
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Question: On the histogram, Figure 3, you have this correlation of the structures with elevation. Did you also do any correlation with maps of geologic features, in other words the availability of good building sites, or test for the flat mesa?

Eddy: That's what I say, it's a bias. If you smooth out that histogram curve, it would be bimodal and so the upper mode is the mesa top, which is inclined but more level, and the lower mode is on the Piedra River terraces which again are level locations. In between you have a dip in the number of sites, and that's affected by the fact that you have steep slopes and stony soils, a lack of good building sites.

George Gumerman: Speaking for all the authors, all the "et al's" as well as the "Eulers", in that Science article, we do appreciate this kind of input to show us where there are variations we weren't aware of. In this light, we are later this month having an advanced seminar at the School of American Research to refine our regional sequences and to look at it more carefully in terms of how phases, how culture boundaries, were determined by individual archeologists so we can more effectively equate culture change with perhaps some sort of climatic variability, and something that we did not do in the Science article is to look at the relationships, not just showing a concordance of perhaps climatic change and culture change, but to attempt to explain the relationship in a more thorough way.

If I can sum up the papers this morning, and many yesterday, I think, to steal a phrase from Cordell and Plog, these papers have indicated that we have made great bounds in escaping the confines of normative theory. (That phrase always makes me think of a break-out from the big house!) But I think what we have seen here is an escape from these confines. However, I think we've got a little ways to go in looking at the incredible heterogeneity of the environment. While we do talk about non-normative behavior, I think, in spite of lip service and statements to the contrary, we tend to see the environment in a normative fashion and that our next strides might be taken in looking at the environment much more closely than we have instead of just talking about variability in the environment. Perhaps we can do a better job in measuring that variability, in looking at cultural responses to that variability.
ANASAZI CULTURE: SUCCESS AND FAILURE

Friday, October 2, 1981
Afternoon Session I
Chair: Paul R. Nickens
YOU JUST CAN’T KEEP THEM DOWN ON THE FARM

Gregory L. Seward

Introduction

A basic problem in understanding the Anasazi has been the lack of a logical reconstruction of their subsistence economies. All too often, archaeologists have either talked past one another while debating the subject or have accepted the previously interpreted data without question. Several Southwestern archaeologists have attempted to define Anasazi subsistence strategies by applying sweeping generalizations to the entire culture. One generalization that has gone uncontested by most has been the theory that Anasazi seasonal movements evolved into a sedentary lifestyle, with permanent sites ultimately being occupied year around.

It has often been assumed that a large, sedentary settlement was necessary for intensive agriculture and “farm” maintenance. Other interpreters felt that sedentism was a “necessary” result of growing agricultural practices. There is no disputing that more individuals may be required to step-up agricultural production, yet a permanent year around population is not necessary to achieve this. Excavation and subsequent pollen/macrofloral analyses have shown that a harvest of cultigens could be produced by the inhabitants of seasonally occupied sites. Seasonally inhabited sites were often repeatedly occupied during a particular season or seasons. The extensive cultural deposits at many sites attest to this. The Anasazi were apparently cognizant of the most efficient use of both manpower and resources. The intensity with which a locale was utilized may have resulted in many sites appearing to have been sedentary. This utilization includes, of course, the exploitation of plant and animal populations within the site vicinity as part of a seasonal cycle. Only by defining the limits of such an exploitive range or cycle can the archaeologist understand the potential maximum resource utilization within that range (Seward 1980). Jennings (1978) states that there exists “a break even point or distance beyond which a hunting and gathering group would not go in view of the time consumed for problematical gain.” Additionally, and as will be discussed later, the act of cultivating or farming encourages an invasion of exploitable wild species and helps concentrate these resources into a smaller area. In the following discussion I wish to point out several problems and assumptions that have clouded the issue of Anasazi seasonality versus sedentarism.

Discussion

Several misconceptions have persisted since archaeologists began attempts at recognizing “regional cultural patterns” of the Anasazi. I believe the one valid overall concept that can be ap-
plied to the Anasazi is a set of similar adaptive abilities in response to diversified sub-regional environments. It has been commonly held that through the course of their existence the Anasazi gradually evolved from an economy based on seasonally changing hunting-and-gathering practices to a primary agricultural dependency based on sedentary techniques.

The archaeological literature contains examples of the unquestioning way in which archaeologists have accepted the idea of a sedentary Anasazi life style.

For example:

Beyond the obvious fact that here a group of sedentary farmers lived in permanent houses for six hundred years, what did we learn? (Hayes and Lancaster 1975:182).

A sedentary lifeway—at least as compared to the preceding Archaic hunting and gathering pattern—was most likely necessitated. (Berry 1980:67).

... even though they were sedentary peoples, they frequently built and occupied new houses, often in new locations. (Rohn 1977:295).

The upland camps were probably used for hunting and collecting expeditions away from home (home implying sedentary residence in this case) (Eddy 1966:379).

Others have alluded to year-round site occupation (Swannack 1969; Rohn 1971), giving the impression that because the Anasazi were farming and living in large habitation units, their sedentism must be an obvious fact. It appears that many archaeologists have been conditioned to interpret large sites this way. They are convinced of the intrinsic nature of the buried deposits even before their shovel has been placed into the ground. As anthropologists, we must not allow this preconditioning to influence an interpretation during any phase of site analysis, be it survey, excavation or subsequent armchair synthesis.

Hayes and Lancaster (1975:182), belatedly aware of this, in fact contradict their earlier statement about the site's obvious sedentary nature by stating "...it pays us to remind ourselves occasionally that things dug up do not in themselves provide the description of a whole culture or a way of life." How, then, do they "obviously" know that the site was occupied by a sedentary group?

This leads us into another misconception. That is, the idea that site size is indicative of the level of sedentism of its inhabitants. In other words, the larger the site, the more sedentary the people are thought to have been. Hand in hand with this, it is also assumed that a large site must indicate a large population. This idea, too frequently accepted without substantial data, lies in direct conflict with the fact that very few of the large sites contain rooms that were all contemporaneously occupied. Even those of us who are trying to escape the confines of normative thought state: "For example, until prehistoric societies were locked into a sedentary pattern by the labor investment involved in the construction of large settlements, the shifts between sedentary and non-sedentary life-styles were probably common" (Cordell and Plog 1979:410).

It is hard to find solid evidence in the archaeological record for the oscillation of an Anasazi group between a sedentary and non-sedentary existence. What a confusing and unproductive subsistence system that would have been!

If such a great amount of labor was required for the construction of a larger site (here assumed to have been completed all at once), when did the Anasazi find time for hunting-and-gathering, let alone for the farming which "necessitated" this monumental project? Pollen analyses have shown that the proportion of cultigens to present wild species is rarely greater than 1:1. Obviously, the Anasazi found some time to sneak away for other "subsistence procurement activities."

Many archaeologists feel that even the basic, open air pueblo unit of the Anasazi must have been sedentary due to the amount of apparent labor involved in construction of that site. I feel that if an area was to be included within the general seasonal cycle, to be exploited as little as 2 or 3 times during a 5 to 8 year period, then it is quite reasonable that a "permanent" structure would be built for reuse. Even the refurbishment of existing structures would be more time and labor efficient than the construction of new structures each time the group entered the same resource area.

In addition, the reoccupation of existing structures by later and probably different groups is well documented on a number of Anasazi sites. Unfortunately, this reoccupation of a site locale is often seen as simply a step in the gradual process of the eventual sedentary occupation of that area. The repeated use of a resource area is quite different from occupying it year-around. That the area was deemed favorable by the Anasazi for a specific time during the year is all that can be verified.

Ford (1981) and Gasser (n.d.) both point out that the act of cultivation (i.e. disturbance of the ground surface) encourages the invasion of wild species and several weeds into even the smallest farm plots. The archaeo-botanical data indicate that wild species formed an essential part of the prehistoric diet, though many are considered "weeds" in the botanical sense of the word (Gasser n.d.). Again, citing Ford (1981), small scale farming often results in a concentration of weedy plant species which might otherwise be scattered, less abundant and in sufficient quantity and close proximity for even a seasonably mobile population to gather. Cleome (bee plant) and the Cheno-Ams are excellent examples of invading species frequently utilized by the Anasazi. This is especially true during Pueblo III times when the utilization ratio of corn to bee plant reverses itself.
habitat produced by the admixture of cultigens and concentrated wild species provided forage for “animal taxa in greater numbers than could be supported under natural conditions” (Ford 1981). The Anasazi, as a group that had been subsisting on natural resources since their origin, had an intimate knowledge of basic ecology. Not only would they recognize basic ecological progressions, as just mentioned, but also the point at which the exploitation of an area would over-tax the ability of the natural resource base to replenish itself.

Many times, the level of sedentism of a site’s inhabitants is based on the amount of faunal and floral remains, especially cultigens, that are recovered. The abundance of corn remains found on later P III sites versus the amount found on earlier BM III sites is due to the fact that through time there was an increasing utilization of the total corn plant. Cutler (1966), Ford (1981) and others point out on later sites that in addition to the kernels that served as food, corn cobs were used in construction, the cobs and stalks used for fuel and the tassels used for bedding. I am unconvinced that the presence of corn plant remains is a factor of preservation. Rather, it is a factor of disposal and utilization. This change in corn plant utilization produced more archaeological evidence of Zea mays and has been interpreted as an increasing dependence on corn. This, in turn, is then used to support sedentism among the Anasazi. In addition, there are numerous problems in interpreting corn remains, including the fact that these deposits give no indication of the corn’s productivity. The same is applicable to cucurbit remains. It is often difficult to positively identify domesticated squash remains from those of hybrids, wild buffalo gourd or other wild cucurbits due to its preservation characteristics and methods in which it was deposited. These factors contribute to what is often a distorted profile of domestic plant use.

Finally, most pollen and macrofloral analyses seem to demonstrate that it is illogical to view the triad of corn, beans and squash as a primary resource/Anasazi subsistence base which “necessitated” a change to sedentary settlement. This triad may be seen as an important source of food during certain times of the year but never the only one.

There are two problems which continually plague archaeologists in their interpretation of a seasonal versus sedentary Anasazi culture. The first, of course, is ethnographic analogy. Though extremely useful in discovering possible resource utilization patterns unfamiliar to settlers of European descent, any further conclusions reached using ethnographic analogy are askew from the onset. As numerous researchers have pointed out, and as I believe also, the very fact that the modern Pueblo groups are sedentary should disqualify them as a base from which to reconstruct Anasazi culture. Similarities between the modern Pueblos and the Anasazi are mainly the result of similar adaptations to similar environments.

The second problem is our own ethnocentric interpretation of the archaeological evidence. We feel that the Anasazi were consciously aspiring to a level of sedentism. When archaeologists find evidence of domesticated plants or of agriculture they assume that it signals a maximum achievement or “high point” in evolving Anasazi subsistence economies. Cliff dwellings, large villages, and above all, agriculture must be evidence of sedentary life. I’m afraid that at times we bestow our values upon the Anasazi simply because the observable components of their “civilization” at certain points in time most resemble those that make up our own culture. Can we really assume that all cultures that could be sedentary would have chosen to be sedentary? As Netting (1977:9) has so appropriately stated, “it is our tendency to pigeonhole human groups in some evolutionary framework.”

I have obviously skirted the problems with the culture history of the Anasazi after A.D. 1225, and for good reason. By this time, many areas were already abandoned while others were in a state of chaos. In virtually every region of the Anasazi sphere a trend towards intensification and diversification of the exploitation of wild species was occurring. The pollen and macrofloral record often portrays a near complete elimination of the cultigen triad from the Anasazi subsistence base prior to abandonment of occupied areas. Also during Pueblo III, nearly all of the cliff dwellings are seen as being permanent, sedentary habitations. Many are often considered to have a possible defensive orientation. Again, this is stated as an “obvious fact” and has not been substantiated with new data.

I tend to agree with most (Jennings 1978; Berry 1980; Powell et al 1980; 1980; Ford 1981; Gasser n.d.; etc.) who feel that several key environmental and social factors created a stress situation to varying degrees within the entire Anasazi culture. I will also agree that this eventually resulted in a complete breakdown of Anasazi society and the abandonment of their homeland, as it were. However, the evolution of this stress related societal breakdown, I feel, will never be explained to everybody’s liking. Thus, I will not attempt it here.

Conclusions

In the preceding paper, I have tried to bring out some of the older misconceptions and false assumptions that are, unfortunately, still around and being used as the foundation for current interpretation. It has been much too long since Southwestern archaeologists have congregated, like this present body, to revamp old ideas and theories with new data. The Anasazi culture was characterized by an adaptation to
a variety of micro-environments within their culture area. In essence, the Anasazi were efficient subsistence economists who, throughout their history, utilized two concurrent subsistence strategies (that is, hunting-and-gathering and agriculture) while maintaining a seasonally oriented life style. Further, intensification of agriculture does not negate hunting-and-gathering; it supplements hunting-and-gathering.

The main impetus of this paper has been to bring to light the unquestioning acceptance of sedentism as a result of agriculture and the weak foundation it is usually built on. The fact that the idea was developed and accepted years ago before many of our current analytical methods were in use calls for a reevaluation of the theory. The size of a site, the reoccurrent use of an area or the intensification of agriculture can no longer be used as the basis on which sedentism can be defined within the Anasazi culture. I, like many others, used to believe this (Seward 1980).

The Anasazi were agricultural hunters-and-gatherers, not agriculturalists who, when given time from their farm chores, exploited wild species. Future research in the area should be able to address these questions and either supply solid evidence of sedentary habitation or verify the existing lack of data to support the idea of sedentism and lead to new interpretation of agricultural remains.

Questions and Comments

Curt Schaafsma: I was talking to Fred Eggan and he indicates a fairly strong theme contained in contemporary Hopi culture in the linkage back to the wild plant resources that are either used as a normal supplement or as a buffer food supply in times of stress. So there are lots of clues to this whole thing in the contemporary or ethnographic Pueblo world. I think you are still on the right track there.

Greg Seward: Granted, but a hunting gathering group or a sedentary group, if they are in an area long enough, they are going to become cognizent of the available exploitable species, I can’t think of a site where the Anasazi totally exploited everything a la Jennings, you know, with this starving nomad type thing in the Great Basin. I feel that they were very conscious of what was around and knew what they could use in case certain things like the cycle of the pinyon nuts or whatever. . . .

Schaafsam: All I am saying is that the ethnographic pueblos also are aware of these things, and we can learn a lot more about this simply by paying attention to this dimension of their culture.

Seward: Granted, but we have to really be cautious when we start drawing analogies because one is seasonal and one is sedentary. And that’s a very different socio-economic. . . .

Al Schroeder: Getting back to these analogies, the Spaniards tell us that the Jemez were wandering and hunting and gathering all over the place before they got back to their pueblos in the early 1600’s.

Seward: That’s right, and their life style was changed because of the Spanish.

Schroeder: No, it was because of the famine that preceded the 1620’s; the Spaniards had nothing to do with it. I got two questions: Along these lines you’re speaking, now, what about the percentage of crops versus wild foods in these pueblos that have been excavated; have you worked with anything of that nature? The reason I’m asking you this question—the Pimas, for example, who were pretty sedentary in ways we usually think of them, if the Gila failed and they couldn’t get water into their ditches, they would spend five years out from the fields just gathering and hunting and living that way. Now, have you got any data to support your statements regarding the percentage of crops versus the wild food products?

Seward: Well, my wife’s a palynologist, and she gave me many, many studies, and I could not see where the triad was that much greater, if any, than wild species, the total number of wild species, and in fact, in some cases prickly pear was used more than beans.

Schroeder: Well, again its because Casteder and Bell and others who have worked with the historic groups have found that in almost every instance there was hardly any case where there was more than fifty percent of the crops supporting them, that the wild products were often of much greater percentage than the regular crops.

Seward: Right.

Schroeder: In other words, what you are getting at in your talk, as I see it, is maybe these Anasazi were like the Mojave, they plant, they go off and hunt and gather and so on, and come back. . . .

Seward: No, I maintain that the Anasazi could produce a cultivated crop and permanently occupy a site seasonally, not year round but seasonally, and still hunt and gather. Then when they harvested these crops and they went into their fall and winter ranges, which is something hardly anyone ever talks about, the winter sites—it’s always, “They’re here to grow crops and to exploit species from spring to fall.” while there is very little documentation of sites that have been called or considered winter sites.
Seward: Well if that’s what you want to call a permanently seasonally occupied site, a home base, yes, I can agree with that.

Adrienne Anderson: Are you suggesting the entire population that is mobile like this in a seasonal way, or perhaps that there are select groups within the Anasazi territory that are seasonally more mobile and where we do have the big urban centers?

Seward: Yes, but I think that’s deceiving because, Number 1, I think that our population numbers, our estimates, are exaggerated for the Anasazi per site, and I do feel that the whole site was essentially abandoned when that period of exploitation was over.

Charlie Steen: Sometimes it works the other way, the manner in which you are speaking. Bandelier in 1880 or 1881, writing in May, said of Cochiti, the village is virtually abandoned; the people have gone to their little summer fields which are at higher elevations where there would be more rainfall. But this was a breakup in the family groups for summer farming, and then they would return to the main village in the fall.

Seward: So what you are saying is that the whole village is spreading out and then they are re-aggregating?

Steen: In May, the village suddenly became a hundred times larger, and then contracted again in the fall.

Seward: I have to be careful, but I can see that happening in a lot of the larger Anasazi sites where several groups, clans, whatever you want to call them, families, are coming together, aggregating for a certain amount of time during the year, and then, possibly during the winter, they are splitting up into their own little groups and going to other smaller sites, and then the cycle repeating, I don’t know, yearly or ...

Schaafsma: To go back to the ethnographic thing again, you may have this one backwards in the context of the ethnographic example which would be to set up a philosophy to be projected—not to be projected straight back but to be setting up a model.

Now, to go to that ethnographic model, we’re going to see people holing up in the big villages in the winter time, in other words, people dispersing whether it be Acoma, Taos, Jemez, Zuni, wherever; people are dispersed in the growing season and in the fall or winter in the modern ethnographic context. So that’s a model to go back to. In the past you will maybe see people, like at Far View, aggregating in the wintertime and dispersing to 20 or 30 small sites. . .

Seward: Right, but the problem with that would be that every large site would be assumed to be an aggregation point.

Schaafsma: Which is a possibility.

Seward: Well, that’s true. And going back to that thing, there are probably 30,000 sites in San Juan county, and it is incomprehensible to me to think they would be permanently occupied.

(Editor’s Note: At this point there was some interference in the tape recorder, and two questions could not be heard on the tape due to the background noise.)

Schaafsma: Granted that evidence needs to be compiled to support the idea of sedentism, if that is indeed the proposal. Similarly, evidence has to be compiled in order to support the idea of a seasonal occupation. Do I understand you correctly to suggest that one of those bits of evidence comes from pollen diagrams, that you can get an accurate assessment of the proportions of utilization of plants like maize and beeweed, like cleome, through pollen diagrams? Is that what you are saying?

Seward: And also coprolite analyses. Coprolite analyses, of course, are more valid, but I think that it does give a good indication of what’s being exploited . . . or the macrofloral specimens in coprolites. But what I am saying, basically, is that I am asking the profession to prove to the rest of us that the Anasazi could have been, or were, sedentary. If the information is valid and empirical, then I’ll change my mind; but at present I’m not convinced that the Anasazi were ever sedentary.

Question: Do you have any evidence against it?

Seward: Well, I try to present problems and, well, that’s kind of a loaded question. Just the analyses of macrofloral pollen, the reoccupation of sites, like especially on White Mesa near Blanding, Utah, which I participated in a couple of years ago. We found a lot of reoccupation of P-I structures by P-III people. And so that at least signifies a reoccupation of the area. There is just nothing there during the winter for them to exploit. The deer go down into the canyons. It’s miserable out there, and that’s highly subjective and I’d rather not . . .

Question: Where did they go?

Seward: Well, that’s the point I made. No one has really documented a seasonally winter-occupied site.

Schaafsma: Sometimes it works the other way, the manner in which you are speaking. Bandelier in 1880 or 1881, writing in May, said of Cochiti, the village is virtually abandoned; the people have gone to their little summer fields which are at higher elevations where there would be more rainfall. But this was a breakup in the family groups for summer farming, and then they would return to the main village in the fall.
Adrienne Anderson: Talking of storage features in large sites for storing the produce of the summer, they would have to be there during the winter to use them.

Anderson: That's the kind of paper that the Symposium was set up to produce.
THE KAYENTA ANASAZI A.D. 1270: PRELUDE TO A MIGRATION

Alexander J. Lindsay, Jr.
and
Jeffery S. Dean

Introduction

Anthropologists and historians in the late 1880’s developed an intense interest in the cultural history of the prehistoric and protohistoric pueblo cultures of northern Arizona and New Mexico. These investigators often perceived the emergence of new traits, the disappearance of established traits, and changes in settlement sizes and distributions as the results of migrations. Ethnographic accounts of legendary migrations were sometimes proffered to bolster these theses. Through time, the migrations hypothesis became popular, but only as one of many explanations of culture change and variation. In the 1950’s the migration concept came under scrupulous examination in the Seminars in Archaeology: 1955 (Wauchope 1956), and later Migrations in New World Culture History (Thompson 1958).

Today, the migration concept in archaeology is used more carefully than in previous times but not without controversy. Nonetheless, migration is a viable explanatory device for the archaeologist (See Euler and others 1979). Its application requires a quantity of data and imposes a special burden on the research effort. The state of flux in migration research (Bennett and Gade 1979) by geographers and sociologists on the cutting edge of theory development and data collection puts added constraints on archaeologists in this area of investigation. In this paper, we suggest that one prerequisite to postulating a migration is to know in depth the cultural tradition at the origin and its various systems in the premigratory state.

At the end of the 13th century, the Kayenta Anasazi abandoned their place or origin, north of Black Mesa, and migrated to the Tusayan region on Antelope Mesa and the Jeddito Wash area, to the Hopi Mesas, and probably southward to the Mogollon Rim country and beyond. This paper examines briefly the Kayenta at a premigration state and offers some clues for determining the Tsegi phase phenomenon at postmigration destinations.

Suggested destinations achieved by some migrants from northeastern Arizona, including Kayenta migrants estimated to number between 3,000 and 5,000 persons, have been proposed for Point to Pines by Haury (1958) and the San Pedro Valley at the Reeve Ruin (Di Peso 1958) by Lindsay (1969). Other possible locations are on Anderson Mesa at Kinnikinnick Pueblo (Conner 1943) southeast of Flagstaff, and the Safford Valley (Brown 1973; Wasley 1962). These four areas are all a considerable distance from the Kayenta homeland in northeastern Arizona, and one to several weeks is the estimated travel time for a small group to reach these locations.

The long range goal in assessing the nature of the premigration Tsegi phase Kayenta at A.D. 1270 is two-
The Kayenta Anasazi at AD 1250

fold. One is related to our research in Long House Valley near Kayenta, Arizona, which had a continuous Kayenta Anasazi settlement from A.D. 1 to 1300, and which includes several large Tsegi phase pueblos. The second purpose of this assessment is to provide a premigration model of the Kayenta as background for testing Haury’s (1958) hypothesis of a prehistoric migration from northeastern Arizona to Point of Pines in the late 13th century.

Information for this paper is derived mostly from three major works on the Tsegi phase: Anderson’s (1969) detailed report on technology; Dean’s (1969) study of two major cliff sites in Tsegi Canyon, Betatakin and Kiet Siel; and Lindsay’s (1969) synthesis of the Tsegi phase.

Quite briefly, we wish to comment on some of the terms used in this report.

The **Kayenta tradition** is a regional manifestation of the Anasazi tradition that is mostly signified by its architectural and ceramic traditions; both evolved with a distinctive character between roughly A.D. 300-400 and 1300. The Kayenta are located in northeastern Arizona and southeastern Utah in a geographically definable area of expanding and contracting limits of settlement and use.

The **Tsegi phase** is the terminal unit of the Kayenta sequence (Colton 1939). This cultural manifestation represents a 50 year era of maximum cultural complexity for the Kayenta and the apogee of the Kayenta tradition. The phase dates between A.D. 1240 and 1300. A.D. 1290 is a closer approximation of the time when most villages were abandoned, while 1300 designates most accurately the time of depopulation of the region by its former Kayenta inhabitants.

Throughout the Tsegi phase Kayenta settlement occupied a roughly rectangular area, delineated by the San Juan and Colorado rivers on the north, the Colorado River and Navajo Canyon at the west, and the contiguous Red Lake and Klettha Valleys and the Kaibito Plateau adjoining the northern scarp of Black Mesa on the south. The eastern border is the western margin of Monument Valley. This is an area of 2,400 square miles, reaching roughly 60 miles east to west and 40 miles north to south. The area can be traversed in two to four days walking-trotting time.

Contemporaneous traditions occupying adjoining areas are the Chinle-Canyon de Chelly and Mesa Verde traditions to the east, Cibola to the southeast, Winslow and Tusayan traditions to the south, and the Sinagua to the southwest.

The **Nuclear Kayenta Region** noted above has two recognizable physiographic provinces, each comprising similar rock units and similar sedimentational histories. The north and east portions of the region are distinctive for their spectacular platform-mesa-plateau uplands rising high above deeply entrenched, usually defile-like canyons, all tributaries of the San Juan and Colorado rivers. The lowlands within these canyon systems are generally below 4,400 ft. The canyonlands are resource areas that provided tool stone, long growing seasons, and permanent water sources near or adjacent to deep alluvium. Abundant and varied rock types for making chipped stone tools are available in the extensive Pleistocene gravel beds deposited along the San Juan and Colorado rivers, and also in the conglomeritic rock units of the upland mesas. There is ample evidence to document the extensive use of these lowland resources. The uplands, which comprise most of the areas of permanent human occupation, are between 5,000 and 6,500 ft. South of the canyon and mesa country are broad alluvium filled upland valleys trending generally east-west. Much of this region is covered by eolian sand and is dominated by stands of pinyon and juniper broken by sagebrush parks, with black-brush on arid knolls and hills. Resources in the highlands include timber for construction, firewood, building and tool stone, and many plants and plant products for fabrication and subsistence. Some resources are ubiquitous and at hand or a few kilometers away, and others are localized, which may have required and inspired craft specializations (See Ambler, this volume). Although many canyons have permanent streams, water is scarce throughout the region. In the uplands, precipitation is sufficient to maintain fairly dense stands of mesic vegetation.

Temperature and moisture are adequate for growing maize and varieties of squash, gourds, and beans. Cotton appears also to have been grown in the canyons in prehistoric times (Adams, Lindsay, and Turner 1961). The eastern side of the region appears to have been more favorable for agriculture in both past and present.

The beginning of the Tsegi phase at A.D. 1240-50 represents a time of noticeable changes in the Kayenta cultural configuration. Paleoenvironmental evidence (Euler and others 1979) shows that the Kayenta were under the pressures caused by a climate deteriorating toward drier and warmer conditions and by dissection of the alluvial flood plains, circumstances of considerable consequence to the high desert farmers. Neighbors to the east and south appear also to have been coping with similar environmental stresses (Rohn 1977, Guerman 1969, and Schroeder 1977). Some areas in the Kayenta region had been abandoned many decades prior to A.D. 1250, including those occupied by the Virgin-Western Kayenta (Aikens 1966), Cohonina (Schwartz 1966), and Chaco (Vivian 1970).

In attempting to adjust to the changing environmental regimen and concomitant deterioration of horticultural potential, the Kayenta evolved a new settlement pattern and social organization. Population aggregation, caused in part by an in-migration of Kayenta from small settlements beyond and in the nuclear area, contributed to these changes in settlement patterns and postulated social organization.
Settlement Pattern

Tsegi phase sites occur in various forms including isolated granaries, soil and water control features, burial locations, field tending camps, and villages of various sizes and complexities. The localized village is of concern here; probably 100 of these are known, and perhaps 30 of these have been excavated sufficiently to reveal patterning. Villages range from more than 100 rooms to 10 or 15 rooms. Field houses of one and two-rooms are common; often these are reoccupied houses of an earlier age. Open sites are considerably more numerous than diff-situated villages, the latter occurring mostly in Tsegi and Navajo canyons and the former occurring throughout the region.

Tsegi phase villages are often, but not always, front-oriented (Reed 1956) and usually situated in prominent positions that are easy to locate and observe from a distance. Some cliff-locale sites are exceptions and often are of slight to moderate difficulty of access. The characteristic village position is at the margin of valleys or canyon floors. Villages commonly are built in positions elevated above the surrounding terrain apparently to maximize intersite visibility (Lindsay 1969). Settlements are near arable land, mostly alluvium and stable dune sands. Settlements that lack natural water sources usually have skillfully built simple water catchment systems to provide a nearby supply of domestic water. Weathering pits, tanks, and plunge pools also provided intermittent supplies of water.

Tsegi phase village architectural technology sometimes includes loopholes, blind doorways, and artificially restricted access routes. These features make the sites defensible, although there is little evidence that the villages are primarily defensive in character.

None of the known villages have in them, or adjoining, any features that are presently recognized as structures specifically to promote or require intercommunity cooperation. Villages are probably autonomous; however, the social linkages and communication networks among the villages of a locality (for example: Long House Valley, Rainbow Plateau, Tsegi Canyon) appear to be strongly developed.

Community Pattern

Each Tsegi phase village has its own characteristic form, often influenced by the nature of the building site. Nevertheless, the villages have much in common as exemplified in the habitation and ceremonial architecture. Dean (1969:23–36) and Lindsay and others (1968:14–9) provide specific definitions for most of the Tsegi phase architectural units and site features.

The domestic structure basic to all permanently or semipermanently occupied Kayenta communities is the single story living room. The room is usually rectangular, often nearly square, and usually between 3 and 4 meters on the longest dimension, which is generally at right angles to the doorway. The entrybox complex (Lindsay and others 1968:7) is the signal attribute of Tsegi phase living rooms, although other features include a low door threshold and relatively high doorway, interior plastered walls, leveled and plastered floors, and occasional jacio walls. The entrybox complex consists of a slab-lined hearth connected to the doorway by an open top, box-like device of high slabs that act as deflectors. This feature controls entry to the room and air circulation.

Granary and storage rooms are built contiguously to the living room, and with the presence of more than one living room these units are built on as appendages to form a linear or cellular-like, multi-room unit. These rooms, especially the granary, have several distinctive characteristics. The granary, a specialized room for food storage, is distinguished by better quality exterior masonry, by the absence of smoke blackening, by the lack of floor features, and by distinctive entryways, both roof and wall forms. The entries are designated to be closed and sealed from the outside. The doorway is the commoner form and it has a grooved or clay-molded lintel, threshold, and jams fashioned to accept a tight fitting stone slab door held in place by a wooden rod passed through loops set in the masonry at either side of the door.

The primary room association unit is the room cluster, which includes one or more living rooms, storage rooms, and granaries. These units are adjoined to an open courtyard that usually has more floor space than any one of the rooms adjoining it. Grinding rooms fitted with one to three metate or mealng bin units sometimes stand alone or are incorporated into room clusters. When grinding rooms are absent from a site, mealng bins are fitted into storage and living rooms.

The pit house structure continues into the Tsegi phase as an alternate room style; its exact function is not known. In general shape the pithouse follows the style of masonry rooms, and some have the attributes of masonry living rooms and granaries (Hobler 1974).

Tsegi phase kivas assume two basic forms: circular (keyhole) and rectangular. Shapes vary somewhat as do their construction and degree of subterraneity. A kiva may be attached to a room cluster, several clusters, or a courtyard complex. Granaries and annexes (Dean 1969:32) are sometimes associated with kivas. In some instances a kiva occupies a location near, but separate from, the domestic units. The circular kiva is the favored form in this era but rectangular kivas are found throughout the region. Both forms may occur in the same village (Lindsay and others 1968). To the best of our knowledge, the rectangular kiva form is associated only with the Tsegi phase. Some small sites lack an identifiable kiva.

Floor features of the two kiva forms are the same and
The Kayenta Anasazi at AD 1250

Material Culture

A dozen pottery types are represented, five of which occur at Point of Pines. This distinction may be of paramount importance in analyzing the alleged "Kayenta" migration ceramics, specifically similar to St. Johns Polychrome.

The implications of these architectural data are for a society of simple social organization characterized by strong, probably matrilocal, households. Differing degrees of social integration appear to be developing rapidly in the Tsegi phase, a more complex level of social-cultural integration than in any previous era. The society is fluid and mobile with provisions for assimilating and aggregating unrelated social units. Localized clans and the Kachina cult are not evident in the Tsegi phase period.

Social Organization

The Kayenta ceramic tradition at Point of Pines is of coil and scraped manufacture, being made of local materials probably available to most potters. The range of variation in vessel forms and styles of decoration is restricted. One might expect more diversity given the range of wares. Some pottery appears to have been traded from manufacturing centers; indeed, there may have been potter specialists (See Ambler, this volume). Most pottery is thought to have been made and used at each village.

White and orange wares are decorated with geometric designs drawn with carbon paint. Both classes have similar hemispherical bowl forms. White and orange ware jars differ considerably in both shape, size, and capacity. Gray ware vessels have indented and plain corrugated and rough surface finishes. The plain gray pottery, Kiet Siel Gray, is essentially a Tsegi phase type, and its limited forms and shapes are not shared with the corrugated ceramics. The larger size grayware jars are smaller than their orange and white ware counterparts. Studies have not been made to determine the character of these jar size and capacity differences. Tusayan Black-on-white decoration style is distinctive for its bold negative design layout and strict geometric theme and diaper layouts. Scrolls, frets, spirals, hachured and bold framing lines, corbeling, and serration are favored elements. Although this pottery design style is most distinctive and is seldom confused with earlier materials, it shares many elements and layout features with the ceramic traditions of the Mesa Verde and Cibola areas, and possibly Hohokam.

The designs on orange ware are signified by bold red lines framed with narrow black lines that often emphasize panels of parallel lines, hatching, and corbeling arrangements. Orange peel and offset quarter layouts are common. Similar designs are executed on red-slipped vessels. Some orange and red ware decorations are outlined with white framing lines to create the distinctive four and three color Kayenta and Kiet Siel polychromes.

Two ceramic forms of earlier Kayenta tradition manufacture belong also in the Tsegi phase ceramic complex. Colanders occur in both white and orange painted wares, usually in the form of squat seed jars. Undecorated shallow plates also occur, usually in grayware, sometimes with the underside corrugated or less frequently basket impressed. Often a row of perforations follows around the circumference of the plate, giving the form the name "perforated plates." The use and function of both vessel forms is unknown.

In addition to the diversity in the technology of the ceramic tradition, there is evidence from Anderson (1969) that use of the three styles of grayware changed rapidly during the Tsegi phase, implying the Kayentans' acceptance of change in a time of increasing economic stress.

The Kayenta Anasazi at AD 1250 . . .

Social Organization

The strong patterning of Tsegi phase architectural forms implies equally strong organizational patterning. The ubiquitous room cluster can be interpreted as the domiciliary household unit for the nuclear families and the matrilocal extended families (Lindsay 1969). Lineages may have formed by this era, and some arguments can be made for the development of nonlocalized clans (Dena 1970).

At the level of community pattern, considerable form variation occurs throughout the region among villages of comparable sizes. Several villages are aggregations of courtyard complexes, family household units, and are distinctly of an unplanned village style (Chang 1958). Fewer villages show a planned plaza layout. There is no one area or locality where the similar forms cluster. Less well known are two unexcavated planned-style villages with an open square layout.

The implications of these architectural data are for a society of simple social organization characterized by strong, probably matrilocal, households. Differing degrees of social integration appear to be developing rapidly in the Tsegi phase, a more complex level of social-cultural integration than in any previous era. The society is fluid and mobile with provisions for assimilating and aggregating unrelated social units. Localized clans and the Kachina cult are not evident in the Tsegi phase period.

Material Culture

Pottery: A comment about Tsegi phase ceramics is required because of its distinctive character at this time and its antecedents. The Kayenta ceramic tradition of this era is closely shared with that of the Tusayan tradition to the south. The significant difference occurs in the Tusayan ceramics with the application to some orange ware bowls of a whiteline decoration in the style of the White Mountain red wares, specifically similar to St. Johns Polychrome. This distinction may be of paramount importance in analyzing the alleged "Kayenta" migration ceramics that occur at Point of Pines.

During the last half of the 13th century, the Kayenta made three wares: white, orange, and gray. A dozen pottery types are represented, five of which are sensitive time markers. All have antecedents in earlier Kayenta ceramic forms and design styles.

Kayenta pottery is of coil and scraped manufacture, being made of local materials probably available to most potters. The range of variation in vessel forms and styles of decoration is restricted. One might expect more diversity given the range of wares. Some pottery appears to have been traded from manufacturing centers; indeed, there may have been potter specialists (See Ambler, this volume). Most pottery is thought to have been made and used at each village.

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In addition to the diversity in the technology of the ceramic tradition, there is evidence from Anderson (1969) that use of the three styles of grayware changed rapidly during the Tsegi phase, implying the Kayentans' acceptance of change in a time of increasing economic stress.
Tools and Equipment

The Kayenta tool inventory is simple and basic. This simplicity makes it distinctive. The relatively modest range of tool types displays little diversity of shape. Stone, bone, and horn-antler tools show little variety, and tools in general, especially percussive items, often are worn. The few textiles known are worn and mended. Perhaps the poverty and lack of variation in the Tsegi tool kit indicates both the conservative nature of tool technology and an unwillingness to create new things in anticipation of an impending departure.

A technology that the Kayenta probably acquired from neighboring groups is that for water and soil control. The use of terraces, dams, ditches, and catchments appears to have been adopted by the late 1100s, at a time of increasing water stress, to capture transient water flows (Lindsay 1970). During the Tsegi phase the Kayentans used a few simple architectural devices in subtle ways, apparently as needed but not in excess of need. The point for consideration is their capacity to borrow or invent soil and water control measures and apply them to their domestic and horticultural requirements.

Subsistence and Agriculture

As noted previously, the Kayenta farmed to produce a portion of their subsistence. By analogy with Navajo and Hopi who grow similar products under like conditions, the Kayenta performed well. In clearing land for farming and other exploitive land practices the Kayenta may have been their own worst enemy, for erosion of farming areas began as early as A.D. 1150 and was well advanced by 1300.

The wide knowledge and use of both domesticated and wild plants and animals demonstrates an intimacy with the natural environment (Clark 1966). Numerous food resources are available in the highlands, and others occur in the lowland canyon areas. In the lowland areas temperatures are higher, a longer growing season prevails, and permanent water exists, which may have permitted the cultivation of more than one crop per growing season. The Kayenta grew food locally and collected plants and animals at more distant locations from their villages, apparently with enough success to allow a new social system to evolve and certain technologies to develop.

Trade

Within the Kayenta region there are indications of internal trade for resources of the region and also for finished products from outside the region. To date, no published studies have been done to identify the character of the trade system. However, the uniformity of Tsegi phase pottery up to the time of the region's abandonment argues for an effective regional communication network. The localization of certain minerals, clays, and rock types in the region requires contacts for acquisition and distribution of both materials and products.

Kayenta pottery has been traded outside the region since A.D. 900. During Tsegi phase times, substantial numbers of Kayenta vessels occur at Mesa Verde, particularly in the Sinagua area, southward into the Verde Valley, and southeasterly into the Cibola region. Mesa Verde ceramics occur in Kayenta sites with decreasing frequencies from east to west, with almost no representation on Cummings Mesa on the far west. The *tcamahia*, a celt-like hornstone tool imported from the Four Corners area, is found at most villages. Turquoise and shell are rare. Ornamental forms are common and probably were made from raw material quarried on Black Mesa. In view of the paucity of trade goods it is surprisingly that macaw skeletal and feather remains occur in some sites (Hargrave 1970). The implication of these materials is for trade with populations several hundred miles distant to the east, southeast, and southwest. The Kayenta imported durable goods of stone and pottery, a few luxury-ornamental items, an exotic bird, and perhaps also foodstuffs. Kayenta pottery is the most obvious item exported to other groups.

Summary

By the close of the 13th century, the Kayenta had abandoned their homeland. Some evidence indicates that a few migrants anticipated returning to their villages after a brief, local displacement. On the whole, the final abandonment pattern of the Kayenta area is one of directionally biased, interregional migration.

A large population left the Kayenta region, and some evidence indicates that a sizeable number of Kayenta found new homes in the Tusayan area. Yet some groups may have continued southward at least as far as the San Pedro Valley east of Tucson. The Kayenta of A.D. 1290 seem to have been capable of coping with long migrations to less stressful environments.
The title of the paper is misleading because the subject matter pertains as much to the early Rio Grande culture as to that of the San Juan. The two areas were allied, but not as closely as many students would have us believe.

Along the upper Rio Grande, a small stable population developed from an Archaic base. At the present time, it appears that these folk of several linguistic groups stayed in position for at least 2,000 years with only minor shifts. As the culture of the Rio Grande peoples developed, some aspects of general Southwestern puebloan evolved: brush shelters became pit houses, which in turn became surface house blocks, a maize-bean agricultural economy was adopted, ceramic containers were made, and there were similarities in religious structures with those of other parts of the southwest. The stimuli for these developments seem to have moved up the Rio Grande from that large cultural area we term Mogollon.

During the early 1930’s, H.P. Mera made a series of archaeological surveys through much of New Mexico and adjacent portions of surrounding states. During this survey Mera noted hundreds of ruins and made small surface collections of pottery from them. No excavations or other stratigraphic studies were made by him. The products of the surveys were several papers in which Mera interpreted the data he collected. One of the papers, published in 1935, was entitled “Ceramic Clues to the Prehistory of North Central New Mexico.”

In that paper, Mera noted that much pre-Columbian pottery of the upper Rio Grande bore strong stylistic resemblances to the ceramics of other portions of the northern Southwest. The similarities were in types and layouts of design during the Pueblo I, II, and III periods, and the districts towards which the relationships were indicated were Chaco Canyon, Cibola (Zuni area), and the San Juan (Mesa Verde area).

For the most part, the influences from Chaco and Cibola were held to be in the Pueblo I and II periods; that from Mesa Verde was of the later time.

The years in which Mera worked were those in which the first surge of dendrochronological studies occurred. It was quickly noticed that during the 13th century the Mesa Verde area population of the San Juan shrank constantly and, near the end of the period, disappeared. At the same time, the population of the upper Rio Grande increased rather dramatically, and the Indians spread out from the river bottoms of the main stream and a few tributaries to the highlands on both sides of the river and even to the piedmont of the east flank of the Rocky Mountains.

Mera pointed out that Rio Grande pottery of the 13th century bore strong stylistic affinities with
ceramics of Cibola and, particularly, with Mesa Verde. He suggested that the 13th century increase for the Rio Grande population might be due to an influx of refugees from the devastated San Juan.

What seems, in retrospect at least, a flood of papers ensued which attributed the increase in Rio Grande population to migrations from the west. Reed (1950), Wendorf and Reed (1955), Riley (1952), and others wrote of influxes of “foreign” peoples to swell the ranks of those in the Rio Grande, and there were implications that the newcomers from the west spoke Keres or Tanoan languages and overwhelmed and engulfed a nameless and weak riverine culture. The argument continues to the present day.

This explanation of the cultural development of the Rio Grande must be wrong. Most of the support for the movement of Cibola or San Juan peoples to the Rio Grande is based on the Mera analysis of the situation, and not on excavations. The ceramic craft is an art form (Steen, 1977) which can easily and quickly be passed across country by a sort of osmosis from one group to another or can be carried by traders. During the first three puebloan periods across the northern portion of the southwest, the designs on painted pottery are remarkably similar. The vessels have minor regional differences in design and also show variations due to local materials, but otherwise one can spot a Pueblo I sherd, for instance, just about as far as one can see it—no matter where the locality. More dependable than pottery as pointers for cultural development are construction methods and the nature of ceremonial rooms—things which do not change as readily as styles in ceramics.

Along the upper Rio Grande there seems to have been a steady evolution of construction methods and types of houses and of ceremonial rooms with no apparent influence from other districts.

From the time the Rio Grande puebloans abandoned the pit house (11th century) until well after A.D. 1300, wall construction was almost entirely of adobe. Walls were built in courses which were about 20 cm. thick and 40 cm. high. Each course was apparently allowed to dry before the next was added. Masonry walls were not common until (a guess date) about A.D. 1325, and the quality of the masonry was poor. This date has been arrived at by excavations on the northern part of the Pajarito Plateau, and it is well after the period of any proposed 13th century population movements.

Ceremonial rooms were, in proto-Tewa country at least, little different from the dwelling chambers: They are difficult or impossible to distinguish before excavation. Surface rooms built on the eastern side of house blocks are distinguished by a slightly larger size than dwellings, a somewhat curved eastern wall, and an alignment of three features: a vent at the base of the east wall, a small deflector, and a larger than normal firepit.

This is not much like the kivas of the San Juan or the Cibola regions. The question of kivas in the Rio Grande is one yet to be decided. Keres, and possibly Tiwa, adopted the kiva during the 13th century, but the Tewa seem not to have done so until nearly the middle of the 14th.

Then, what did happen along the upper Rio Grande?

The puebloan complex of culture developed from an Archaic base fed by stimuli from the cultural mass we term Mogollon. This began at an early stage, at least two thousand years ago, and continued through the pre-Columbian puebloan periods. Throughout this time there were trade relationships between the Rio Grande and the Cibola and San Juan areas, but they appear not to have been as strong as the ties which bound the people of the upper river with those of the middle Rio Grande and those of the mountainous areas of the Mogollon region.

From the vicinity of present day Socorro to Taos, the Rio Grande puebloans experienced pretty much the same development as that of their neighbors to the west, but with a considerable lag (possibly as much as 100 years). Sometime during the Anasazi Pueblo I period the style of painted ceramic decoration passed into the Rio Grande valley, and this relationship continued through the next two cultural periods. That was the only puebloan trait which can be said to have passed from west to east.

Pit houses continued to be used on the Rio Grande until the 11th century. Then a form of adobe construction—a type of pressed earth—became the standard for building houses. Whereas further west by this time semi-subterranean kivas were standardized and distinctive, in the Rio Grande the ceremonial room was built on the surface and incorporated in the house block.

There is still the question of the rapid increase in Rio Grande population during the 13th century. One of the difficulties in judging what happened is that our view of that, or of any pre-literate period, is foreshortened. What appears to us to have happened suddenly probably took several generations to transpire. At any rate, sometime during the second half of the 13th century we find numerous small settlements of puebloan peoples living and farming on the highlands on both sides of the Rio Grande. Somehow these folk learned that on the mesas and in the higher valleys rainfall was somewhat more abundant and dependable than along the main stem of the river.

An increase of population must have been involved in this development. Schwartz (1977), in describing results of the Arroyo Hondo excavations, mentioned a “population explosion”. Dickson (1979), writing of the same research project described rapidly expanding population. Neither of these men saw any addition to the indigenous population from outside the Rio Grande Basin. Nor do I.
The new settlement pattern of villages along the river plus many in the highlands persisted for nearly 300 years until, at about the time Europeans arrived in the southwest, there was a re-grouping in which the people again became pretty well centralized at lower elevations and along the main water courses. So much for the Rio Grande. Now, what do I believe happened on the San Juan?

The Anasazi, the pre-Columbian puebloan people who lived in the San Juan/Colorado River drainages, had a culture which also developed from an Archaic hunting-gathering base just as their Rio Grande neighbors. This base was one which developed in the Colorado-Great Basin region, and we must keep in mind two important considerations:

1.) The Anasazi, along with other Southwestern groups never achieved the status of true agriculturists. That is, a surplus of cultivated food probably was rarely grown. Half or more of their vegetal food was obtained from uncultivated plants. The most accomplished Indian farmers of the Southwest were the Pima, of whom Russell stated (1908) that more than half their vegetal food was from uncultivated sources.

2.) There is no archaeological evidence for movement of any 'foreign' groups into the Anasazi area, so I believe it is safe to argue that the culture we term Anasazi evolved from the Great Basin-Colorado River Archaic and, even during the centuries of village life, retained much of the knowledge and the practices of the early period.

We are prone to forget that the stimuli which fired the Anasazi development came from the south through the Mogollon area. The Anasazi took the bit in their collective teeth and ran away so that they developed a sub-culture with so many distinctive features that at least two generations of southwesterners believed that the entire pre-Columbian development began in the San Juan, and that all else was peripheral and/or second rate.

At any rate, here we have the well known Basketmaker-Pueblo development and in the 13th century during the Pueblo II period, after a short-lived climax, everything fell apart for our primitive horticulturists. The 13th century must have been a miserable time on the San Juan. The entire century, according to the tree ring record, was a dry one with a drought climax during the last quarter of the period. In addition, the development of large pueblos with no sanitation led to much disease. A third major contribution to the miseries of the century was the extensive arroyo cutting which prevailed over the northern part of the Southwest during the time.

Jett (1964) discussed these and other difficulties and reached a conclusion that no one factor could be blamed for all the troubles which beset the Anasazi.

The population of the San Juan cannot have been a large one to start with. Here, as on the Rio Grande, there were many small and large settlements and vil-
lages and, again, our trouble is that we can seldom tell which rooms or settlements were occupied at any given time. At Tse-ta’a, in Canyon de Chelly (Steen, 1966), there were three separate room blocks of the Pueblo III period. Each was constructed with a different type of masonry—one of them was built in the Mesa Verde style and will appear again later in this essay. It is possible that all three were occupied at the same time, but that is not likely. Each of these house blocks would appear to have been used by an extended family or household. The same pattern of living units can be distinguished at large pueblos, comprised of a number of household units packed into close quarters. Again, how many of these were in use simultaneously?

All this is to argue for a small Anasazi population as the 13th century began. As those years passed, I would say that the number of Anasazi constantly shrank until at the end of the century few people remained. The exodus from the upper San Juan probably began before the advent of the 1276-1299 drought and the movement was principally toward the west and southwest. At this time, near the Colorado River and west of the Chuska Mountains, a number of small settlements (household units) were established on lands which already had been abandoned by others. The 'others' would have been earlier groups who had already drifted, in their shrunken numbers, toward a focal region. The ‘Mesa Verde’ ruin at Tse-ta’a and as many as ten other similar sites within Canyon de Chelly, belong to this movement.

The focal region of this movement was the Hopi country, and this leads me to believe that the Anasazi of the upper San Juan (Mesa Verde) area spoke a Shoshonean language. They were on the edge of the great block of Shoshonean peoples and their ties with the Anasazi of the Colorado River and the Black Mesa seem to have been strong and of long duration.

One isolated piece of evidence should not be forgotten. In 1891, Nordenskiold (1893) excavated a desiccated body of the Pueblo III period at Step House. The body was that of a woman with an undeformed skull, and the lady's costume is of particular interest. She was clad with a leather skullcap-type hat with a peculiar leather fringe, a leather dress and a pair of moccasins. On this evidence alone one cannot argue that the lady belonged to one of the Shoshonean hunting groups, but it seems likely. In at least this one instance it appears that there were friendly relations between the Mesa Verdeans and some nearby hunting tribe.

Now, in conclusion, I wish to do a little more guessing. I doubt that all the people of the Mesa Verde region drifted off towards the horizon to seek new lands. Some must have stayed behind, discouraged, to give up the whole idea of making a living by planting crops. These people I see reverting to a hunting-gathering type existence from which their forefathers had evolved. They would have been quite
familiar with such a life style from neighboring
groups to the north and northwest.
This suggestion should not be a surprise to those
familiar with the prehistory of the country to the
west. The rather reluctant pre-Columbian farming
populations of central Utah and southern Nevada,
which disappeared at about this time did not just
dissolve into thin air, nor did they migrate to some as
yet undiscovered Nirvana. They probably said,
collectively, “To hell with the uncertain new-fangled
life.” (they had been at it for a scant 200 years) and
returned to the good old ways. They too were
Shoshoni speakers.

Questions and Comments

Curt Schaafsma: When Olson and I were working
with Dick, here, on the Picuris thing, we were
concerned about architectural continuity in the Rio
Grande Valley, and I sense a quite familiar
architectural continuity back to Alan McNut’s
pithouses down around Santa Ana where you come
up with the circular 4-post roof support system and
east orientation on the basic ground plans, which
basically were similar to what Stew dug over at
Tonque Arroyo, which was some years later. In other
words, the architectural continuity, particularly in the
kivas, we really tracked back to about 500 A.D. with
Alan McNut’s sites down in Santa Ana. A reasonable
argument is that the architectural continuum in the
Rio Grande Valley doesn’t start in the 1000’s, and
actually goes on back about 500 years.

Charlie Steen: Alright. In this paper I was thinking in
terms of wall construction and I sort of ignored the
preceding pithouses.

Schaafsma: Right, and the pithouse-kiva continuum
really has immensely deep roots in that valley.
Another comment on that: at Picuris there is a kiva
that we demonstrated was built around 1250 that was
still in use in 1955.

Steen: Don’t say that; that shoots me down! A 1250
kiva? Go ahead.

Schaafsma: The kiva-pithouse continuum in the Rio
Grande Valley is distinctive and very long lived. One
of the key features is the east orientation, no pilaster,
no bench, a typical 4-post roof support.

Steen: That’s right. The east was the important
direction.

Schaafsma: Right, since about 500 A.D.

Al Schroeder: Charlie, out of curiosity, do any of the
Hopi traditions seem to link up in this direction?

Steen: Yes, there’s one, at least one, which has to do
with a village with some spruce trees. This has been
made much of in spite of the fact that the spruce trees
at Spruce Tree House are firs. They have at least a
legend referring to this general area, and its pretty
hard to separate the true story now from modern
additions. They say now, the Mesa Verde.

Schroeder: After they made the trip around the Four
Corners.

Steen: That’s right.

Joe Ben Wheat: Charlie, didn’t Danson find, in his
survey down in the Datil area—wasn’t a lot of that
late stuff Mesa Verde?

Steen: I was surprised, I wasn’t distressed at the time.
I did some work at Gila Cliff Dwellings in 1942 and I
got some real honest-to-god Mesa Verde Black-on-
white there. But I think this is in the same category as
finding St. Johns Polychrome in eastern Oklahoma.
This is a beautiful pottery, very well made, and I
think it was quite widely distributed by what, traders.

Wheat: Most of his houses and settlements in the late
phase in the Datil area tended to be Mesa Verde-ish,
to say the least.

Ted Frisbie: Emma Lou Davis dug one of the sites
near Magdalena in the late 50’s or early 60’s, and she
had pretty much Mesa Verde style pottery and
architecture. She also worked at Helicopter Mesa, of
course, which was north of old Highway 66, now
Highway 40.

Steen: What Emma Lou Davis got was one of these
little units, a little household unit which I think
represented one extended family unit high-tailing it
across the country. I think many conceive of this
migration as a village or a community picking up and
leaving, going off on a long trek which looked like
the push-cart people going into the Salt Lake Basin. It
couldn’t have been that. Tiny, very small, groups of
people, and I believe mostly going west, in that
direction.

Comment: Charlie, I might say that during the
stabilization done by Morris at Cliff Palace, I have
been told (I never saw the sherds, but I’ve been told)
that a St. Johns Polychrome sherd was found out in
front.

Steen: It should have been. That’s perfectly good.
St. Johns is found everywhere. It’s a beautiful
pottery. This does not pertain to the subject directly,
but I mentioned St. Johns Polychrome in eastern
Oklahoma because it’s back here in my mind. I have
wondered for a long time, why pottery? This is a
fragile substance, or artifact; why carry it for
hundreds of miles, really, when it’s so breakable. The
late Carl Schmidt at the University of Oklahoma dug
a Washita focus site of A.D. 1200 to 1300, thirty or
forty miles south of Norman (this would have been in
Despite numerous idealistic definitions, the archaeologists’ concept “site” has been most frequently used to denote a unit for field investigation selected arbitrarily and subjectively. While it would be useful for a “site” to consistently represent the remains of some social unit such as a community (e.g. Willey and Phillips 1958) the long-standing pragmatic usage of archaeologists should simply be accepted. In practice, a “site” equals any unit of archaeological remains in the ground selected for investigation and identified with a distinctive label.

Interpretation of socio-political organization can be achieved through other concepts pertaining to spatial relationships. The enclosed space in which a socially and economically sustaining household or equivalent group resides may be called a dwelling or house or apartment or suite (Rohn 1965:65–66), depending on the specific nature of its architecture. A settlement (cf. Chang 1967) consists of a group of dwellings situated close enough together to allow active interrelationships among their inhabitants, who would then represent a community. In many cases, settlements contain a variety of non-residential spatial units such as avenues of travel, plazas, ceremonial constructions, markets, and specialized work areas.

While we can readily distinguish between villages and large urban settlements, such as Teotihuacan and Cahokia, at what point or stage in its growth and development does an ordinary settlement become an urban settlement? Numerous criteria—size, existence of occupational specialization, markets, public buildings, relationships with a hinterland, governmental regulation of cooperative behavior, and combinations of these—have all been advanced as indicators of an urban condition at one time or another. Perhaps the distinction is more flexible if the criteria remain variable, even somewhat amorphous. Certainly the need for careful interpretation of each possible criterion dominates the process by which any settlement is delineated and thence characterized. I shall outline briefly the delineation of several large thirteenth century Pueblo settlements in the Four Corners of the American Southwest, and speculate about several of their urban qualities.

Since none of these settlements has been extensively excavated as a settlement, each must be examined from its surface indications. Particular emphasis was assigned to relative density of habitations and accessibility both within the settlement and to the outside. Contemporaneity was evaluated from visible architectural details, spatial congruence, and surface pottery fragments. To illustrate this approach let us turn first to the well-preserved cliff ruins of the Mesa Verde where room counts are possible without excavation.

Thirteenth century Pueblo houses on the Mesa Verde were usually constructed of stone masonry in shelters and beneath ledges of vertical sandstone cliffs.
Budding Urban Settlements... lining steep, narrow canyons. The same cliffs that contained these shelters also obstructed traffic flow between shelters and to the flat mesa top where the best farm lands were located. Trails had to be carefully selected and often improved by clearing rocks or cutting small steps.

The 33 separately designated habitation sites located in Cliff and Fewkes Canyons must all share only 8 to 10 such trails, yet all are easily accessible to one another along the base of the cliffs (Rohn 1977). They contain some 530 to 545 living and storage rooms plus 60 specially built ceremonial structures or kivas. Drawing parallels with ethnographic census figures from historically occupied pueblos, we can estimate a population of 600 to 800 persons inhabited the prehistoric Cliff-Fewkes Canyon Settlement. Half of them lived in the two largest sites, and nine-tenths in 14 of the 33 sites.

Two large non-residential structures, located among the habitation sites, may have been built for and used by members of this community. One resembles a great kiva (Fewkes 1916a; Cassidy 1960), the other a tri-wall structure (Fewkes 1916b). Strong cases have been made for a ceremonial/religious function for both by their excavators (Rohn 1977).

Both artificial and natural sources of water were developed. A few springs and seeps were readily available in the canyons at the base of the cliffs, but hardly yielded an adequate supply for 600-800 people. I believe this group increased their water supply by construction of a complex water collection and transportation system that covered a distance of roughly seven miles (Rohn 1963). Such a system combined a need for considerable labor with centralized planning and direction. The proximity of both specialized ceremonial structures to the two primary water outlets, one natural and one imported, suggests religious connection for the overall direction.

Aztec Ruins in northern New Mexico provides a second case. Only portions of one large rubble mound have been excavated (Morris 1919). The complete settlement included more than 60 kivas and probably housed between 700 and 1000 people (Corbett 1963). A great kiva (Morris 1921) had been built in one plaza, while two tri-wall structures (Vivian 1959) stood apart from the house units. No water system has yet been described for the Aztec Ruin settlement, but oral references to “old” ditches in the nearby valley are suggestive.

In the Montezuma Valley, northwest of the Mesa Verde, lie several quite large settlements of twelfth and thirteenth century vintage. Only lengthy excavation can reveal their full extent, and even then parts of these settlements have already been destroyed by erosion, road building, and vandalism. However, it is possible to gain a crude estimate of their size by counting the visible kiva depressions. Conservative estimates based upon excavated sites on the Mesa Verde and on historic Pueblo census would suggest some 10 to 12 rooms inhabited by 12 to 15 people were associated with the average kiva (Rohn 1971 and 1977). If nothing else, the settlements can be compared with one another and with the two previously described ones by relative numbers of kivas discernable on the ground surface.

Table 1 summarizes the size data for all of the Pueblo settlements discussed in this paper. The greatest number of kiva depressions was recorded at the Yellow Jacket Ruin (also referred to as Suhuaro) together with at least eight towers and one great kiva measuring 62 feet in diameter. Yucca House, Toltec Ruin, and Lowry Ruin (a small segment and great kiva excavated by Martin—1936) each have 80 or more discernable kiva depressions, and each has either a great kiva or a tri-wall structure. The Wilson Ruin has at least 70 kiva depressions and what appears to be a tri-wall structure.

Other settlements of similar size, but not yet evaluated by these criteria, exist in this region as well as numerous smaller settlements of contemporary age. Actually, some Pueblo III settlements in the Montezuma Valley appear to be quite small. I believe that most, if not all, of these smaller settlements were outliers of the larger centers where the great kivas and tri-wall structures were located, that the larger settlements functioned as ceremonial centers for occasional ritual observances, perhaps once every year or two years. I would doubt the presence of a concomitant political interrelationship in the sense of the Meso-american ceremonial center organization where trade and tribute formed integral parts of the system. Instead, I would envision a pattern not unlike that seen among the present-day Pueblos where the Snake Ceremony is performed at Walpi and Mishongnovi or at Shungopavi and Hotevilla, yet is attended by Hopi from all ten villages. Similarly, the Corn Dance at Santo Domingo attracts people from all the Rio Grande Pueblos even though many of them perform a similar ceremony on a smaller scale. Further illustration of this principle has been detailed by Leap (1970), especially for Isleta and Taos.

Furthermore, I would estimate that the vast majority of all Pueblo III inhabitants of the Montezuma Valley lived in one of the larger settlements. Nor would I be surprised to find several smaller settlements serving as “ceremonial centers” for a cluster of contemporary groups. In fact, our preliminary data from the Mesa Verde clearly suggest it. Yet, if only seven large “ceremonial center” settlements existed in the Montezuma Valley—besides the five listed in Table I, we may add the Goodman Point Ruins (with a great kiva) and Cannonball Ruins (Morley 1908) at least—together with their outlying settlements, they conservatively must have housed 10,000 to 15,000 people. Adding the occupants of the Mesa Verde (estimated at around 4,000 by me in 1960) and the Mancos Valley, the figure would closely approximate the present-day population in the same area.
For comparative purposes, Table 1 includes data for two other settlements. The Pueblo Bonito figures (Judd 1964) assume that site in the Chaco Canyon in New Mexico, south of the San Juan River, to be a complete settlement. Of course, the nearby sites of Pueblo del Arroyo (Judd 1959) and Chetro Ketl could actually have belonged to the same settlement, although the latter had its own great kiva while the former had a tri-walled structure.

Better documentation is available for the Far View Settlement on the Mesa Verde (Rohn 1977: 280-283). This settlement seems to have been an early Pueblo III precursor of the late Pueblo III Cliff-Fewkes Canyon Settlement. The kiva-tower structure probably was the functional equivalent of the tri-wall structure, since great kivas existed at least as early as Pueblo I. This situation offers our only time depth for the moment, although some surface pottery suggests the large Montezuma Valley settlements began developing in Pueblo II as early as the eleventh century. Future definition of settlements for all the prehistoric Pueblo stages should clarify the general picture.

Can the large Montezuma Valley settlements properly be termed “urban”? I think not. Yet, they seem to have been on the verge of becoming urban, were only another few ingredients added. And apparently the necessary ingredients were never added, for the twentieth century Pueblos of New Mexico and Arizona possess essentially the same characteristics as their thirteenth century forebearers. Even their size has stabilized (Stubbs 1950).

For Peru, Rowe (1963) has suggested a figure of 2000 inhabitants as a demarcation between a small urban settlement or pueblo and a large one or city. He defines an urban settlement as an “area of human habitation in which many dwellings are grouped closely together . . . to leave insufficient space between them for subsistence farming” (p. 3). How “many” dwellings is not specified. Still, the 700 year persistence in size in Pueblo settlements suggests his size factor has validity. Perhaps other factors strongly affect true urban development, for the rapid growth of cities in Asia, Latin American, and Africa demonstrates seemingly unlimited growth potential even without advanced technology (Adams 1960).

In the sense that they are deemed the property of the whole populace, public buildings seem to be found in all cities. They may be government buildings, religious structures, markets, transportation hubs, water or other utility distribution devices, waste disposal systems, and many others. The large Northern San Juan settlements all contain unusually large ceremonial buildings—the great kivas and the tri-wall structures. Where we have investigated closely, they also appear to have developed suitable water holding and management facilities, some quite elaborate (Rohn 1963).

The presence of smaller contemporary settlements around the large centers may indicate an interrelated hinterland population linked to the large settlements through ceremonial needs. I have already suggested this pattern for the Mesa Verde (Rohn 1977), but only

<table>
<thead>
<tr>
<th>Settlement</th>
<th>Kivas</th>
<th>Est. Rooms</th>
<th>Est. Population</th>
<th>Specialized Ceremonial Structures</th>
</tr>
</thead>
<tbody>
<tr>
<td>Yellow Jacket Ruin</td>
<td>103</td>
<td>700-800</td>
<td>1200-1500</td>
<td>Great kiva</td>
</tr>
<tr>
<td>Yucca House</td>
<td>84</td>
<td>1000-1200</td>
<td>1000-1200</td>
<td>Great kiva</td>
</tr>
<tr>
<td>Toltec Ruin</td>
<td>80+</td>
<td>1000-1200</td>
<td>Tri-wall Structure</td>
<td></td>
</tr>
<tr>
<td>Lowry Ruin</td>
<td>80+</td>
<td>1000-1200</td>
<td>Great kiva</td>
<td></td>
</tr>
<tr>
<td>Wilson Ruin</td>
<td>10-74</td>
<td>850-1100</td>
<td>Tri-wall Structure (?)</td>
<td></td>
</tr>
<tr>
<td>Aztec Ruin</td>
<td>60+</td>
<td>700-1000</td>
<td>700-1000</td>
<td>Great kiva</td>
</tr>
<tr>
<td>Cliff-Fewkes Canyon</td>
<td>60</td>
<td>530-545</td>
<td>600-800</td>
<td>Tri-wall structure</td>
</tr>
<tr>
<td>Pueblo Bonito</td>
<td>35</td>
<td>651</td>
<td>1000-1100</td>
<td>3 Great kivas</td>
</tr>
<tr>
<td>Far View Settlement</td>
<td>32+</td>
<td>300-375</td>
<td>400-500</td>
<td>Kiva-tower</td>
</tr>
</tbody>
</table>

Table 1 A Summary of the Large Pueblo III Settlements in the Northern San Juan Region of the American Southwest.
by detailed investigations through intensive survey and some excavations can the idea be tested.

Perhaps the missing ingredients are the most important for judging "urbanness". So far as we know now, the thirteenth century Pueblos had no occupational specialties. This is not to say that some craftsmen did not make more pottery or stone axes than they needed and trade these items to others. However, we have yet no evidence of any household devoting full time to any occupational specialty, not even religious leadership. Apparently, every household farmed and manufactured many or most of their own needs, bartering for other items with any surpluses.

In like manner, we have no evidence of regular marketing through designated marketplaces or of any centralized political control. Even the ceremonial interactions of various settlements seem to have carried no theocratic implications. Monumental art or architecture does not occur in Puebloan settlements. The great kivas and tri-wall structures were constructed with the same materials and techniques used on domestic buildings.

On the technological level, the most glaring absences were systems for waste removal and transportation. In all likelihood, well-traveled trails, such as the ones mapped at Chaco Canyon, linked the various settlements. Still, people and goods traveled on human feet. Colton (1960) has suggested the failure to develop adequate waste removal places a limitation on the size of Pueblo settlements. However, this same impediment seems not to have hampered growth in many contemporary cities.

By the thirteenth century, some Pueblo settlements in the Northern San Juan Region had reached the threshold of true urban size. They practiced a food producing economy, effectively managed water and soil resources, and possessed a stone-bone-wood-ceramic-textile technology. Some apparently functioned as ceremonial centers. Yet, they did not cross the threshold in the past 700 years. We may argue that several necessary ingredients such as an adequate technology, occupational specialization, or centralized political authority were missing. We might suggest that Pueblo concepts of social organization and government prohibited further development. Perhaps the extensive relocation of the Pueblos from the San Juan drainage to their present locations after the thirteenth century, followed by the impact of Europeanization starting in 1540, shocked Pueblo culture too severely.

Whatever the determining factors—and I prefer the first one listed above—the Southwest's Pueblo Indians never developed large urban settlements or cities. However, their experiments with large settlements may have helped in our general understanding of the beginnings of urban life during prehistory.

Questions and Comments

Question: In your investigations out in Montezuma County or here on the Mesa Verde, have you seen any evidence of road systems more like the Chaco network of trails?

Art Rohn: I've never seen direct evidence of road systems in the northern San Juan, but I would be inclined to suspect they have largely been destroyed by the intensive agricultural activity in this particular region. I frankly believe they probably existed, but I have not seen direct evidence.

Charlie Steen: Al Lancaster has said that yes, there formerly were. There was at least one long road which went from Goodman Point south.

Rohn: I would trust Al's word on that, that he has seen traces of a road from Goodman Point heading southward. I might add, the Goodman Point Ruins are in direct line of sight with the Yellowjacket Ruins. The distance from there to the Toltec Ruins, while there is not a direct line of site, is quite short, and there is only a small erosional drainage divide between the Toltec Ruins and the Yucca House to the south, so that intercommunication among these large settlements would be quite easy. Perhaps we have not looked in the right places for traces of these road systems. I'm inclined to think we would find some if we did an intensive search.

Al Schroeder: Just out of curiosity, Art, how long have you been working on this . . . ?

Rohn: When I said counting kiva depressions in the ground was not easy, I wasn't kidding. I actually began this work in the early 1970's, and presented this same information to a meeting of the American Anthropological Association in 1971, but unfortunately none of you were there and people apparently didn't hear it.

Schroeder: I've got another question. A number of years ago, I talked the Mesa Verde people into a tea garden party out at Yucca House. I kept telling them all of those depressions were tea gardens. As you may know, the park service didn't like the idea of putting a shovel into the site, but on the basis of that, Al Lancaster, Al Hayes and myself went out there and put in a test. Much to our surprise we found adobe walls, that is, coursed adobe walls under the masonry walls. Has that been found in any of these other big sites that you know of?

Rohn: Coursed adobe I can't specifically answer for. Mud walls, yes. It is quite common in the Yellowjacket area. I think Joe Ben Wheat can confirm this—to find masonry caps sometimes on top of an earthen structure, or masonry filled in where the earth would not hold up to provide its own support, or sometimes the earthen bank gave way and a masonry repair job was completed. I would not be surprised by that, however. We did not find, neces-
sarily, masonry lining of kivas or structures all the way up. We do not always find horizontal coursed masonry. Sometimes there are large upright stones forming the bases of the walls, whatFewkes called the so-called "megalicith" construction.

Schroeder: Well, in this case what had happened, we just followed a masonry wall down to see how far down it went and what might have been underneath, and directly on top, underneath the masonry wall, was this adobe or coursed adobe wall. I just wondered if this was very common up here.

Rohn: How far down did it go?

Schroeder: Yes, in fact it had a big timber at the bottom, and we hacked off a good chunk with a shovel. We sent it up here to Mesa Verde, which I understand sent it on down to the tree-ring lab, and that's the last I ever heard of it!

Jim Judge: Art, in these southwestern Colorado sites, do you see any more dominant than others, assuming that they were contemporaneous?

Rohn: I can't describe dominance on the basis of only the surface appearances, but I do know that one very fascinating point was brought to my mind by an outsider who looked at a number of these sites with me. I just happened to be showing them off one day, and this was an outsider who is not used to the Southwest; he has worked mostly in Mesoamerica. He stood upon the sites, looked around, and said, "Why, they've got everything here they need. There is water, there's farmland, there's a lovely view, the climate is well, it's so unlike other major population concentrations, or at least other highly reported places such as Chaco Canyon and the Maya land where there are anomalies, where there does not appear to be an adequate supply of one of these basic resources." I do not see any dominance yet. Of course, as I have suggested by the subtitle of the paper, and as I know Joe Ben would immediately agree, we look to the hub of the universe, at the formation of the Yellowjacket Canyon, the Yellowjacket Ruin, itself.

Susan Collins: Given this model of thousands of people living together, clustered in permanent large communities, and all of them being at least part-time farmers, have you given any thought to the question of land propriety, ownership as we see it, in the archeological evidence?

Rohn: There is a lot of work that needs to be done and that would be a good place to start. I don't know how else to answer that. These sites, I think, have been avoided for many, many years and generations because of the incredible chore their investigation would involve. For Joe Ben to spend sixteen years just across the canyon, working on a small suburb of the main Yellowjacket Ruin in order to try to understand the chronology of this area, for me to have spent I forget how many seasons, but not that many, a little further west trying to delineate some of the earlier settlements, both of us, I know, have looked at that large settlement and said, "We can't live long enough to do the job right!" And we would have to start with, I think, a detailed investigation of at least a sample of one of these large settlements so that we would have a comparative unit of material, whether it be ceramic pastes, or design styles, or details of architecture, or other tools, samples of macaw feathers, or whatever we might find, who knows . . . by the way a copper bell has been excavated from the vicinity of the Goodman Point Ruins, probably the furthest northward extent of a feature such as that; it was excavated by Clifford Chappe and reported by Hayes and Chappel some years ago . . . that once we have that kind of comparative unit we might be able to link outlying smaller settlements to the larger core ones and perhaps then be able to draw some boundaries. I think it is a job that needs to be done. It points out that we've got several generations of work ahead of us at least.

Curt Schaafsma: As you recall, Hayes pretty well showed that the period of 950-1000 was the population apogee on Wetherill Mesa as far as site density is concerned. When I worked on Lewis Mesa and Johnson Mesa in 1967, the same kind of density occurred over there. We did a survey between Dolores and Mancos and got the same kind of density of that time period over there. I began to look around the literature and began to see that that really was the territory of a lot of people in the region. I just wondered how that source of thought looks to you.

Rohn: Alden did suggest for Wetherill Mesa a population maximum during Pueblo II in what he called the Ackmen and Mancos phases. His data, however, upon which that was based paralleled very closely the Chapin Mesa data. We both estimated room counts, even from rubble mounds, based upon size and comparisons with excavated sites; we both estimated room counts for the different stages of occupation on both Wetherill and Chapin Mesa. We discovered that there was a constant increase in numbers of rooms estimated as one moved through time, reaching a peak in the 13th century. But Alden reasoned that many of the late sites might have indeed concealed the remains of Pueblo II sites, and hence drew his conclusion that the population maximum struck during Pueblo II. But his data actually showed a peak of room count estimates in late Pueblo III. This is the picture that also appears on Chapin Mesa. I cannot give you any more precise data than that, but I've come to believe that
perhaps the population curve should be drawn to show a constant increase to the 13th century with then a very sharp drop. Since our data, the estimated room counts are the only thing we have (we have no actual census figures or anything of the sort), I’m inclined to agree with you, that I think this process was under way from Basketmaker III right on through. The large Pueblo I settlements that the Dolores Project is encountering in the Dolores Valley and on the Dolores Divide, the rim between the Dolores Valley and the Yellowjacket drainages, these are huge things, they even make Site 33 and Site 13 look like relatively small places. These large settlements were beginning that same practice. I think by Pueblo II we have a similar sort of a pattern. We must, however, get away from the preconceived idea that a unit pueblo equals a village. Pueblo II villages consisted of clusters of unit pueblos that were within a stone’s throw of one another. If you could throw a stone and break your neighbor’s window, you are in the same settlement with them, in the same community. We need to reevaluate our Pueblo II settlements, and then I think we’ll begin to see the beginnings of this thing had a very deep root.

Comment: I’d like to point out in the Red Mesa Valley and in the southern Chaco slope, which is maybe 80 miles long and 25 to 30 miles wide, that exact same pattern exists in P-II. A central public structure, a Great Kiva, and a cluster of domiciliary buildings around it in a radius of about one-half mile to a mile, and then a void between that community and the next one. The average spacing is about six miles from center to center.

Rohn: I think we’ve heard similar figures elsewhere of that kind of spacing.

Joe Ben Wheat: The thing I wanted to find out, you were talking about copper; we are still trying to chase down a find that was made in the early 1900’s north of Lewis: a Pueblo III small jar that was found which contained around 40 solid copper balls. They were given by the man who found it to a cousin back in Ohio because they had copper back there and he was much interested in it. We are still trying to find out which relative has it or where it went to. It’s simply one more copper find from this area.

Rohn: I think we will find more of it when we get into investigations of this sort, there is no question about it.

Question: I was wondering about the significance of the relationship between your circular Great Kiva, the “sports model” Great Kiva as we have in Fire Temple and Long House, and the tri-wall. A few like say in Cliff Canyon, Fewkes Canyon, the circular Great Kiva, and we have one in Morfield Canyon, do you see in the areas you are working anything that shows any significance in the relationship or lack of relationship between these structures?

Rohn: Since the only rectangular Great Kivas, or the name that we adopted in Long House, I remember, some years back, the “Grand Kiva,” since the only ones of these rectangular structures that have been found have been found in rock shelters, overhang situations, I wonder if these might be adaptations to the cliff-side setting or situation where it would be more difficult to get the necessary accoutrements of a round structure. That’s one possibility; there may be I don’t know how many others. The odd shape of Sun Temple, as a modified tri-wall which I have suggested, is a lot harder to explain. They could have built a full tri-wall on the spot of Sun Temple, and I cannot answer that one. At this point I have absolutely no response. Other tri-walls in the northern San Juan zone, except for the Hubbard Ruin at Aztec, are as yet unexcavated, so we don’t know the range of variation of these tri-wall structures. It’s terrible we have to keep looking southward in order to understand the tri-walls. We have tri-walls at so many places in the northern San Juan country, and yet we really don’t know what they consist of. I find, George, I really can’t respond to that because our information is so skimpy, but again I think it points to a badly needed kind of investigative research.

Judge: We seem to have little on Morefield Canyon, and that is one problem, perhaps.

Rohn: The Great Kiva at Morefield Canyon was excavated by Bob Lister some years back, and I believe you excavated a Pueblo I version of a Great Kiva there also. There is a Pueblo I version of a Great Kiva probably on Grass Mesa up in the Dolores Valley. How many others exist, heaven only knows. But, unfortunately, from Morefield Canyon we do not have the delineation of the Great Kiva with its settlements, so we don’t have a large picture of just how extensive that was. I really don’t have answers for these at the present time.

Schroeder: Art, if you will recall back in 1969, I was playing around with the business of possible association between ballcourts and Great Kivas. At the time, I pointed out there were two settlement patterns up north, one of which was the Great Kiva with the population units scattered around different villages, small villages, and the Great Kiva more or less associated. But at the same time, on either side of this string of Great Kivas from Forestdale on up to the Lowry country, you had a different settlement pattern, without Great Kivas but with a little kiva. And then on the east side, no kivas. Do you conceive of these pueblos that you have been describing here, with Great Kivas, as a population unit with scattered villages around it the same way?

Rohn: Yes, I do. As a matter of fact, Al, when you published that article I thought you were way ahead of your time. And once again, for young people looking for research topics, pick up from where Al left off. I think there are ideas in that particular paper that need investigation.
THE ANASAZI AND THEIR EXTERNAL RELATIONS

Saturday, October 3, 1981
Morning Session
Chair: James Judge
CHACOAN ARCHITECTURE IN CONTINENTAL CONTEXT

Stephen H. Lekson

This report is also inventoried as Contribution No. 35 of the Chaco Center, National Park Service and the University of New Mexico.

Introduction

One aspect of Chaco that has inspired a great deal of argument in the past is the possible Mesoamerican source (or sources) for its architecture (pro: Ferdon 1955, Kelley 1966, DiPeso 1974a, Kelley and Kelly 1975, Hayes 1981, among others; con: Vivian 1959, Martin and Plog 1973, among others). The subject is old (some of us might say old and weary) but it remains controversial; even the most detached and scientific researcher can bristle and snap if caught off guard by a member of the opposition.

For reasons which, if listed, archaeologists would find tediously familiar, laymen would find incomprehensible, and both would find unnecessarily boring, North American archaeology has become centered on the study of relatively small areas or regions. In the Southwest, this has lead to a certain amount of myopic provincialism, particularly in the Anasazi area. Only recently have state-of-the-art practitioners of Anasazi archaeology rediscovered that each of their research domains is, in fact, located on the planet Earth, undeniably north of, and incontestably close to, Mexico. It is a fact some archaeologists have found hard to live with.

Other archaeologists, particularly those working in Mexico, had never suppressed this geographic relationship. But there is an interesting difference in approach between archaeologists who work in Mexico, where history in various guises informs the archaeology from at least 900 on, and those who have had to do without analess and codices in the illiterate Southwest. Mexicanists speak of specific events, particular rulers, change wrought through political mechanisms—in short, interpretations bound to the history of Central Mexico. Their Southwestern counterparts deal in adaptations, subsistence strategies and populations—in short, interpretations couched in terms of anonymous evolutionary processes. (There are, of course, notable exceptions to my stereotypes in each camp.) The two positions need not be inimical: history can be addressed processurally as readily as prehistory, and a processural archaeology that cannot incorporate political mechanisms as vehicles of change is unreal. But the two approaches have polarized, for reasons that have more to do with the sociology of academic disciplines and the Treaty of Guadalupe-Hidalgo than with any reasonable assessment of the archaeological record.

The Southwest, in its later archaeological development, is in some sense an extension of the Mesoamerican cultural area. The reader who cannot accept that statement may as well stop reading. At issue, I suppose, is the nature of that extension: are we talking about an archaeological abstraction, a line on a map encompassing similar systems of adaptation, settlement pattern and material culture, or is this extension a demonstrable historical event?
There are few Anasazi archaeologists who would label themselves isolationists; almost all accept the ultimate Mesoamerican origin of many aspects of Anasazi culture. But in practice, later developments within the Southwest are treated as essentially local phenomena. Not a few would consider it terribly infra dig to discuss Mesoamerican influence as anything but an odd quirk in the history of Southwestern archaeology.

Not so the Mexicanists. But the most vocal conjecture of the Mexicanist camp tend to argue historical correlation by simply correlating events in Central Mexico with events in the Southwest. Temporal correlation, by itself, is not explanation. This is the standard criticism that Southwesternists level at Mexicanists: political expansion in Central Mexico and increasing complexity in the Southwest may be contemporaneous, but what of it? And the standard response: Pochteca, the archaeologically obscure (if not invisible) traveling salesmen of an expanding Mesoamerica, exploited the indigenous Southwest and coincidentally upgraded the Anasazi area from PII to PIII. This argument (see Kelley and Kelley 1975) fails to satisfy the Southwesternist who has spent the last few decades documenting local developments in canyon after valley after mesa.

What is lacking is an explanatory theoretical approach that encompasses a continental scale. By identifying this problem, it may be possible to address extra-regional questions while avoiding any compromising identification with either Southwestern isolation or Mexican intervention.

My presentation follows the historical development of the question, at least as it applies to Chaco. The earliest studies (e.g. Ferdon 1955) noted details or traits of Chacoan building that seemed Mesoamerican. Various lists of traits have been either denied and enlarged by subsequent authors (the latest in the series being Lister’s 1978 trait list, and McGuire’s 1981 refutation of the same). The first part of this paper will discuss traits and details which have been attributed to Mesoamerica. The second part of this paper will discuss Casas Grandes and Chaco. Excavations at Casas Grandes (a site in northern Chihuahua, Mexico) have raised many specific questions regarding Chacoan building and Mesoamerica. The third and final section of this paper will approach the problem of continental context from a slightly different perspective. In this section, I will evaluate architectural developments within a larger area of North America, and attempt to relate those developments to processes of continental scale.

**Mexican Traits**

Of the many lists of Mesoamerican traits at Chaco, I will follow that of Lister (1978); it is the most recent, it is probably the most inclusive (some might say, most credulous), and it emanated from the Chaco Center. McGuire (1981) has published an excellent criticism of this list, but on several important points, my conclusions differ from his.

"Rubble-core" or "core-and-veneer" masonry:

These terms have been used to describe a bewildering variety of wall types, having in common only multiple wythes (Lekson in prep.). When compared with walls in areas outside the Anasazi Southwest, "core-and-veneer" has been stretched occasionally to the point of uselessness (Ferdon 1955; DiPeso 1974a: 211; Schroeder 1981:54).

Any wall with coursed masonry also seems to be a candidate; but coursing has very little to do with the interior structure of a wall (which, I assume, defines "core-and-veneer"), nor is coursing that unusual a building technique anywhere bedded stone is available.

"Core-and-veneer" walls at Chaco developed out of the massive compound walls of the early 900's as a building technology capable of producing multiple stories with widely-spaced cross-walls, or in the case of exterior walls, no lateral bracing at all. The development of veneer styles was at first functional, and later largely stylistic; the relative simplicity of even the most elaborate of these styles argues for local development.

The architecture of northern Mexico is not noted for its use of load-bearing masonry walls. Comparisons usually begin as far south as La Quemada, the walls of which do, in fact, resemble some Chacoan masonry (Marquina 1964, Kelley personal communication). Both building technologies employed roughly shaped tabular sandstone. No building at La Quemada was multi-storied, but some of the walls were quite tall, reaching a height of 4m without cross-walls or other buttressing. The base width of these walls was up to 2.4m (Batres 1971). The great width demonstrates the solution to a structural problem similar to that at Chaco (great height without lateral buttressing). I suspect that the use of similar materials and comparable structural requirements produced walls of generally similar appearance but utilized in the two very different contexts.

There is no way to estimate the number of Post-Classic masonry technologies south of La Quemada to which Chaco might be compared. Surely, the number is very high, and it would be remarkable if some of these did not resemble, in either interior structure or surface appearance, the relatively crude efforts of the Chacoan builders. To the best of my knowledge, none really do. To some extent, this may reflect the widespread use in Central Mexico of lime plaster over masonry (which might eliminate requirements for minimal exposure of mud mortar), or the use of different materials (widespread use in Central Mexico of volcanics and other extrusives), or different structural
requirements (single story load-bearing walls, or simple veneers over filled mounds). But this may also indicate lapses in the best of my knowledge. Unfortunately, there exists no systematic technical analysis of Mesoamerican construction technologies, and the National Park Service was for some reason hesitant to fund my acquisition of this knowledge with a Grand Tour of Mexican archaeological sites.

Tower Kivas:

Tower Kivas (cylindrical structures of more than one story) are relatively late at Chaco, probably dating to the early 1100’s. In the earliest example (Kiva N at Chetro Ketl), the second story was probably built about 25 years after the first (Lekson, McKenna and Dean 1981), but in other Tower Kivas (e.g. the three and four story Tower Kivas at Kin Klizhin and Kin Ya’a) the structures appear to have been planned and built as units.

Tower Kivas lack any clear analogues in Mesoamerica (Pollock 1936). The multi-storied round building most often compared to Tower Kivas is the Caracol at Chichen Itza, in Yucatan, Mexico. Beyond the facts that both are multi-storied and both are circular, there are very few points of resemblance between the Caracol and a Tower Kiva. Tower Kivas were built in square or rectangular enclosures as part of a larger architectural unit; the Caracol is free-standing on a huge platform mound (see McGuire 1981 for detailed comparison of the Caracol to Chacoan forms).

Kiva N at Chetro Ketl, with the upper story added later to an earlier lower story, demonstrates likely development of the form in Chaco. Taken together with existing traditions for circular room form and multi-storied building, this evidence of development suggests that the Tower Kiva was an Anasazi form.

Tri-wall structures:

Tri-walled and bi-walled circular structures (Vivian 1959) are probably a specialized development of the Great Kiva form. After 1050, most, if not all, Great Kivas had peripheral rooms (rectangular surface rooms around the circumference of the large circular Great Kiva). Bi- and tri-walled structures may be the architectural correlates of increasing importance of the functions of peripheral rooms and decreasing importance of the function of the circular chamber, particularly in the case of isolated Great Kivas. Given a plausible, if unproven, scheme for the development of these forms in the Anasazi area, and a lack of references to Mesoamerica, the tri-walled structure need not be derived from outside the Southwest.

Great Kivas:

Kelley and Kelley (1975) suggested that Great Kivas were the actual base of operations for, and the creations of, pochteca. This is the most explicit postulated association of Mesoamericans with Southwestern architecture in the literature, and thus assumes somewhat more importance in this discussion than simpler arguments of influence. Early (7th cent.) Great Kivas were equated with circular complexes of the Rio Bolanos area in Jalisco, Mexico; these Rio Bolanos complexes were also felt to be pochteca centers. This seems to me to be wishful thinking, at least architecturally, since the Rio Bolanos complexes and Great Kivas have nothing in common save a circular plan. The Rio Bolanos complexes seem to be variation on the Mexican patio or courtyard domestic complex (Weigand 1978), with low substructure mounds surrounding a patio with a central mound or altar, but substituting a circular patio for the more common rectangular shape. The Great Kiva, on the other hand, is a very large, roofed, circular chamber. Great Kivas appear to be an example of a much wider phenomenon in pit-house or earth-lodge architectures of Western North America. Many villages in this broad area had a single structure, notably larger than the average domestic unit, that served in various ways as a community house. Certainly this is true in the Anasazi tradition. Kelly and Kelly (1975) supported their argument with a presentation of limited temporal and spatial distribution of Great Kivas, but their enumeration of Great Kivas only scratches the surface of this widespread building type (see Marshall et al. 1979). The suggestion of a Mexican origin for the Great Kiva does an injustice to one of the most characteristic Anasazi building types.

Intra-mural platforms:

“Intra-mural platform” is an awkward description of a construction noted by Ferdon (1955) in Room 3 of the Talus Unit. Ferdon felt this construction was a Mesoamerican temple platform built into a room block; this reconstruction has been criticized by Vivian (1959). Room 3 is currently seen as the base of a masonry ramp, or the substructure for a more elaborate wooden ramp, which led to the end of a roadway on top of the cliff immediately behind the Talus Unit. Other masonry ramps are known at Chaco.

T-shaped doors:

T-shaped doors, like Great Kivas, are a hallmark of Anasazi building (Love 1979). T-shaped doors are known from northern Chihuahua, but the context there is much more closely related to Anasazi building traditions than to Central Mexican. (See the discussion of Casas Grandes, below.)

Colonnades:

In particular, the colonnade, or “gallery”, at Chetro Ketl (dating after 1105) has been singled out as suspiciously “foreign” (Ferdon 1955, and all subsequent trait-listers). A similar, but smaller colonnade at Bc 51 (dating after 1040), and possibly as late or later than the Chetro Ketl colonnade, (Truell, personal communication) is also mentioned.

Free-standing square columns also appear as roof supports in square rooms (at Pueblo Bonito and Aztec Ruin, both in the first third of the 12th century) and as posts in the post-and-beam roof structures of Great
Chaco Architecture . . .

Kivas (Pueblo Bonito, Chetro Ketl, Salmon Ruin, and Aztec Ruin, all dating to the late 11th and early 12th centuries).

Vivian (1959: 84) argued that the square column was a logical development of the pier pilaster in circular rooms, which may date as early as 1040 in smaller sites (Truell, personal communication). Vivian’s argument seems forced to me, but antecedents in the Anasazi building tradition are actually of little importance, for it is the use of the square column in a colonnade or gallery, rather than the columns themselves, that is so blatantly Mesoamerican. Vivian also argued that the use of the colonnade at Chetro Ketl was significantly different from the use of the colonnade in Mexico. At Chetro Ketl, he felt the colonnade fronted a domestic structure, while Mexican examples fronted religious (or at least public) buildings. If this is true, it might be possible to consider the colonnade a masonry form of the older Anasazi ramada. At first blush, this seems reasonable: wood—the usual material for ramadas—was never plentiful in Chaco.

But it is not at all clear that the rooms behind the colonnade were “domestic”, nor was there any evidence of features and activities normally associated with ramadas in the colonnade (Lekson, McKenna and Dean 1981). Moreover, the columns of the Chetro Ketl colonnade were not built from floor or plaza level; they rested on a low (0.75m tall) stub wall. Although some earlier ramadas have very low walls running around the outside of the posts these generally did not impede movement from ramada to plaza; those in the colonnade definitely did. For these reasons, it is unlikely that the colonnade represents a ramada built in stone.

The low wall which raises the openings between columns to about the height of the sill of a raised door offers another line of argument for Chacoan development of this form. On each floor along the rear wall of Chetro Ketl were a series of evenly spaced raised-sill doors which, when viewed from the exterior, might have suggested the form of a colonnade. I offer this suggestion for the die-hard isolationists; I do not believe it myself, since the spacing of the doors is eight or more times the width of the columns, and the openings between columns are twice the width of the openings of the doors. An alternate, but related, possibility involves the multiple doors in the 1050’s plaza-facing wall of Chetro Ketl (Lekson, McKenna and Dean 1981). If these doors were open simultaneously (a rather unlikely occurrence) they, too, might have suggested the colonnade form. I do not believe this, either, since the doors in each room were probably sequential, and not contemporary.

The colonnade or gallery is such a characteristic element of contemporary Mesoamerican building, and the arguments for local Chacoan development are so labored and unlikely, that I feel the colonnade strongly suggests a Mesoamerican form, out of context, at Chaco.

Colonnades are a conspicuous form at Casas Grandes, but (as discussed below) they are later there than at Chaco. The nearest contemporary occurrence, and the most likely source for the derivation of the form, is either La Quemada or the Schroeder site near Durango, Mexico (Kelley 1971). Reyman (1971: 262) notes “an exact parallel to the Chetro Ketl Gallery” at Ixtlan del Rio, a roughly contemporaneous site in Nayarit, Mexico. The limited illustrations in Pina Chan (1969) do not seem to support this assessment.

The colonnades at La Quemada and Schroeder are not identical to the one at Chetro Ketl: the columns are round rather than square, which is probably of little significance, and the columns begin on the floor or plaza level rather than being carried on a low stub wall. This point is of some significance since a colonnade reduced to or below the level of that stub wall would offer little evidence of its original form. There may have been colonnades similar in detail to that at Chetro Ketl at more reduced sites closer than La Quemada or Schroeder; for that matter, there may have been more colonnades at sites in the Southwest.

Rectangular mounds:

The two rectangular mounds in front of Pueblo Bonito (Judd 1964, Lekson in prep.) are obvious architectural anomalies at Chaco Canyon, yet they have seldom been appreciated for what they are (notable exceptions: DiPeso 1974, Lister 1978, Windes 1981). They are usually referred to as trash mounds. Because this term is familiar, most Southwestern archaeologists are not as alarmed at the Bonito mounds as they might be over something more outre, like a colonnade.

The Bonito mounds are certainly unique in the 11th and 12th century Anasazi area. In fact, the “trash mounds” at Pueblo Alto, Penasco Blanco, and Chetro Ketl (which may have included two mounds identically placed to those at Bonito [Windes 1981]) are all rather unusual, but the Bonito examples with rectangular masonry facings, steps, flat surfaces, and internal architecture (Judd 1964, Figs. 23 and 24) are in a class apart. They most closely resemble the rectangular platform mounds of the Classic Period Hohokam (e.g. Doyel 1974) which probably post-date large scale building at Chaco. The Hohokam mounds will be discussed in the next section.

Altars in courtyards:

Reyman (1971) notes altar-like structures in Chacoan plazas as a Mesoamerican trait. In this he refers to the patio or central plaza altar of central and northern Mesoamerican domestic building. These altars were small masonry box-like structures set in the center of the domestic plaza, and are known from early Classic times in Central Mexico. In the period 900–1150, they were in use from Tula (Healan 1974) north to the Chalchihuites area of Durango (Kelley 1971).
There are three structures described as “shrines” in plazas of Chacoan sites. The first of these is a subterranean circular chamber, (Room 190 at Pueblo Bonito) with a flagstone floor, about 2m in diameter and 0.7m deep. This was located near the center of the east plaza of Pueblo Bonito (Judd 1964:175–176). The second “shrine” is Reyman’s interpretation of some partially exposed and very fragmentary walls in the east plaza of Pueblo Bonito, which he described as a “rectangular masonry structure, very similar to the firebox/altars found in Great Kivas” (Reyman 1971:281). The third “shrine” was at Pueblo Alto. This was a bracket-shaped low masonry wall with a possible central repository (Windes in prep.).

The Bonito shrine, a small subterranean circular room, may represent a form of sipapu (found in modern Pueblo plazas), while the Alto shrine resembles the isolated structures of the shrine complex described by Hayes and Windes (1975), which seem to be elements of the more extensive signaling system described by Drager (1976) and Hayes (1981). The Reyman shrine, in the east Pueblo Bonito plaza, is too fragmentary to assess. None appear very similar to the patio altars described above. While there may be a rough parallel with the Mexican patio altar placement, direct Mesoamerican derivation of these “shrines” is not indicated.

Discussion

In Contribution Number 14 of the Chaco Center, Lister listed the traits discussed above and (as noted at the beginning of this section) felt that they indicated Mesoamerican influence at Chaco. The mechanisms of that influence were not specified: “Was it accomplished by a pochteca-like group? ... Or was it accomplished by the transfer of ideas and commodities from group to group along an extensive but informal trade route? There is evidence or lack of evidence to both support and discount these explanations” (Lister 1978: 240).

In the first of the N.P.S. Chaco Canyon Studies (Hayes et al. 1981), Hayes, using many of the same traits, was more definite in his conclusions:

> ... the aggregation of traits having no apparent root in previous Anasazi tradition is so great, and they appeared on the scene so rapidly, that it is difficult to see them as anything but “hard” diffusion through the agency of a group of determined political entrepreneurs, some of whom must have been physically present and in residence. ... there is no place to look for the source except ultimately in Mexico (Hayes 1981: 62, 63).

Lister and Hayes represent one view that has been offered by the Chaco Center. In subsequent Chaco Center publications the approach has changed substantially: “While recognizing influence from Mexico as a possibility, recent research has focused on alternatives which emphasize the emergence of the Bonito Phase as a more local phenomenon developing ‘independently’ within the geographical region now known as the San Juan Basin” (Judge et al. 1981: 65–66).

I see only two architectural forms at Chaco that I feel are alien to the local Anasazi tradition; these are the colonnade and the rectangular mound. Although we can demonstrate the possibility of local development for many of these traits, the assessment of either local development or external origin must remain a matter of opinion rather than a matter of fact. In the standard argument suggested local development eliminates the validity of a suggested external source. But this argument follows a logical convention of dubious validity: that is the acceptance of the simpler over the more complex explanation. Presumably, local development is “simpler”—requires less explanation—than the diffusion of an external form. The criterion of simplicity (“Occam’s razor”) allows the archaeologist to deny external connections whenever internal continuity may be argued. There are many arguments that may be made against this; for example available materials and local climate may combine to suggest continuity in an architecture that is spurious (many details of Chacoan building were repeated in Richard Wetherill’s trading post and ranch house). It should be remembered that Occam’s Razor is only a logical convention and it may or may not be applicable to prehistoric human behavior; in selecting the simpler explanation, we may in fact be selecting a facile model to represent complex reality.

Chaco, Casas Grandes and the Classic Hohokam

From the earliest days of Southwestern archaeology, researchers recognized that Casas Grandes, a large adobe ruin in Chihuahua, Mexico, was of extraordinary importance. But scientific excavations took place only as late as 1958–1961 in a project under the direction of Charles DiPeso. Preliminary reports of that work made it clear that the excavations at Casas Grandes would be of great interest, in part because of the occurrence of numerous platform mounds, ball courts, colonnades and other features suggesting strong connections to Mesoamerica. When the massive final report appeared (DiPeso 1974c) these expectations were more than confirmed. The site has major implications for the culture history of the entire Southwest, but in DiPeso’s and many others’ views Casas Grandes was particularly significant for the understanding of Chaco Canyon. Twenty years after the fieldwork at Casas Grandes, and almost a decade after the publication of the report, the relationship of Chaco and Casas Grandes is still a matter of some interest.
DiPeso’s report created more than a little argument; most of the controversy centered on the dating of Casas Grandes. As DiPeso interpreted the ruin, the most spectacular developments at Casas Grandes were in part contemporaneous with the peak of the Chacoan system. Many archaeologists found this dating difficult to accept, and instead have adopted a dating of Casas Grandes that post-dates the large scale building at Chaco Canyon. I have discussed the dating of Casas Grandes elsewhere (see Appendix, this paper). DiPeso defined three phases for Casas Grandes, each with specific architectural correlates. While his phases were defined on the basis of more than just architecture, it is architecture that is of interest here, and at the risk of misrepresenting the archaeology of Casas Grandes, I will use DiPeso’s phase names, with reference only to architecture, but with my dating.

<table>
<thead>
<tr>
<th>Phase</th>
<th>Dates</th>
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</thead>
<tbody>
<tr>
<td>Buena Fe Phase</td>
<td>1130–1150 to 1300</td>
</tr>
<tr>
<td>Paquime Phase</td>
<td>1300 to 1400(?)</td>
</tr>
<tr>
<td>Diablo Phase</td>
<td>early 1400’s(?)</td>
</tr>
</tbody>
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The architecture of the Buena Fe Phase consisted of “20 or more ranch-style house clusters” (DiPeso 1974c: 372)—clusters consisting of rows of mainly single story rooms, one or two rooms deep, within or around rectangular compounds delimited by massive adobe walls. Paquime Phase building created a huge, terraced, multi-storied Pueblo-style building with several plazas and courtyards, and a variety of peripheral mounds, ball courts, etc. This was Casas Grandes at its peak. The following Diablo Phase was a period of sharp decline, with little new building but much subdivision and modification of the existing structure. The Diablo Phase will not be considered here.

The progression from “ranch-style” compounds to massively walled, multi-storied building at Casas Grandes is paralleled in the Classic Period Hohokam of Southern Arizona. There is considerable variation within the Hohokam area. For comparisons, I will limit discussion to the central area of the Hohokam, in the Gila River Basin (Doyel 1976). In this area, two phases have been defined (Haury 1976) that are temporally and to some extent architecturally equivalent to the Buena Fe and Paquime Phases at Casas Grandes:

- Soho Phase 1150 to 1300 = Buena Phase?
- Civano Phase 1300 to 1450 = Paquime Phase?

Soho Phase architecture is characterized by scattered rectangular compounds of single story adobe rooms (Haury 1976, Doyel 1976). While compound architecture continues into the following Civano Phase, this latter phase “includes contiguous roomed multi-storied buildings of massive character, including Great Houses, larger settlements built in and around Soho Phase settlements . . .” (Doyel 1976: 32). Great Houses (see also Haury 1945, Schroeder 1953, Wilcox and Shenk 1977, Wilcox and Sternberg 1981), like Paquime Phase building at Casas Grandes, were constructed with massive adobe walls, interpreted variously as either poured into forms (DiPeso 1974a: 217) or puddled by hand (Wilcox and Shenk 1977: 117).

The best existing example of a Great House, Casa Grande, is much smaller than Casas Grandes; layout is much simpler, and many of the distinctive details of Casas Grandes building are lacking at the Hohokam Great House. It is clearly beyond the scope of this paper to critically examine Hohokam Great Houses and Casas Grandes, but close similarities in construction techniques and parallel archaeological sequences suggest that the two were closely related. Because of this evident relationship, and the well-documented development in the Hohokam area of platform mounds (an anomalous feature at Chaco), I will consider Classic Period Hohokam building together with Casas Grandes in this section.

In the Casas Grandes report, DiPeso (1974a: 208–211) singled out Chaco for comparison; no other area (not even the Classic Period Hohokam) was so specifically honored. A sequent, rather than contemporaneous, relationship between Chaco and Casas Grandes may affect the interpretation but should not affect the validity of his comparisons. DiPeso utilized the trait list approach to compare the two areas. Traits were grouped under seven headings: 1) captial/satellite organization, 2) intervillage road system, 3) tower system, 4) water control system, 5) village organization, 6) rectilinear stone-walled, truncated mounds, and 7) building details. Only the fifth, sixth and seventh groups will be considered here, beginning with the seventh, “building details” (paraphrased to conserve space).

DiPeso noted that both architectures were characterized by multi-storied buildings with load bearing walls, construction beginning on foundations and proceeding one story at a time (as evidenced by variation in wall widths from story to story). Roof construction employed primary and secondary beam frameworks. Both areas are said to have employed “heavy cast or English cob-cored walls which, only in Chaco, were stone veneered” (DiPeso 1974a: 211). While some walls at Chacoan sites (e.g. Bis sa’ani) may have been constructed of cast or puddled adobe, nowhere at Chaco is there evidence for the type of wall described by DiPeso. Both areas used wide walls to support upper stories. In the Chaco area bedded sandstone was abundant, leading to the characteristic Chacoan wall, while at Casas Grandes the available material was adobe, which whether poured or puddled created an equally characteristic Casas Grandes wall. The two wall types are fundamentally different, but given the available materials this difference, of itself, need not indicate distinct building traditions.
DiPeso noted that Casas Grandes and Chaco shared architectural details such as stairs, T-shaped doors, secondary jambs and sills on some doors, “measured timbers” with ends cut square, rectangular beams (rare in both areas), finished boards or planks, and perhaps sub-floor drains (examples at Chaco, from Kin Kletso and Pueblo Bonito are more likely sub-floor ventilators than drains). Distinctive as several of these features may seem, they are by no means limited to Casas Grandes and Chaco Canyon. T-shaped doors and doors with secondary jambs and sills are known from most areas in the Southwest in sites where wall features are preserved (i.e. multistoried buildings and cliff dwellings). Shaped timber may also, at least in part, reflect better preservation in the unusually massive multistoried ruins at Chaco and Casas Grandes, but cliff dwellings—where wood work is equally well preserved—do not evidence the refinement in wood work shown at Chaco and Casas Grandes. Interior stairs are not restricted to Chaco and Casas Grandes, but their use is certainly not widespread in the Southwest. Chaco and Casas Grandes also shared a host of much less distinctive features (such as wall pegs, wall niches, sealed doors, stone lined firepits in open plazas, etc.) ubiquitous in the Southwest that need not concern us here.

Some architectural details are both quite distinctive and remarkably similar. The use of massive stone disks for seating roof support posts is characteristic of Chacoan Great Kivas, and seems to be a standard treatment (25 of 35 cases) for central roof support posts at Casas Grandes. In both cases turquoise and shell caches were often included with these disks (DiPeso 1974: 213). A remarkably similar use of stone seating disks was reported from even further south in northern Mexico, at La Quemada (Kelley personal communication).

Seven colonnades at Casas Grandes recall the remarkable colonnade at Chetro Ketl discussed above. “Six of these faced enclosed plazas and one opened into the open South Plaza” (DiPeso 1974a: 264). None of these were based on a low wall, as was the Chetro Ketl example, and all appear to have fronted domestic units. The Casas Grandes colonnades were built during both Buena Fe and Paquime phases; the colonnade at Chetro Ketl is very late in the Chacoan sequence, and may immediately pre-date the Buena Fe Phase.

Room-wide platforms, characteristic of Chacoan building from about 1020 to 1075 (Lekson in prep.), were ubiquitous at Casas Grandes. Judd (1964: 29) called these “storage shelves”; DiPeso (1974a: 238) labeled them “bed platforms.” Whatever their use, these features are highly distinctive of both Chacoan and Casas Grandes building. Room-wide platforms may also be present at Casa Grande, a Hohokam Great House. Although the best description of that site (Wilcox and Shenk 1977 and Wilcox and Sternberg 1981) does not mention this feature, some of the “erosion grooves” in ground floor rooms at Casa Grande recall room-wide platform remnants at Casas Grandes. While I feel that room-wide platforms are distinctive in Chacoan and Casas Grandes (and related Hohokam Great House) building, it should be noted that the distribution of room-wide platforms in the Southwest could be a function of site preservation. The platforms are identical to roofs in general construction, and are located about midway up the walls. In any site where walls are reduced to half their original height or less, it would, of course, be impossible to locate the remains of roomwide platforms on the walls, and it would be nearly impossible to distinguish their remains from those of the roof.

These are the “building details” listed by DiPeso as evidence of close connections between Casas Grandes and Chaco. As traits isolated from broader building technologies and forms, most are not convincing. However, colonnades and room-wide platforms are both unique in the Southwest to these two areas.

As a separate trait group, DiPeso noted “rectangular, stone-walled, truncated mounds,” citing specifically the rectilinear stone walled mounds in front of Pueblo Bonito (mentioned in the previous section). He felt that these were comparable to the numerous mounds at Casas Grandes. Eighteen “solid-core structures” were excavated at Casas Grandes; these varied in shape from circular, to rectangular, to zoomorphic, and included mounds peripheral to ball courts (DiPeso 1974a: 270). Most were faced with masonry or occasionally puddled adobe retaining walls and had flat (truncated) surfaces. “None bore evidence of having been used as either building or burial platforms” (DiPeso 1974a: 270). The rectangular mounds at Casas Grandes were apparently all part of ball court complexes. While the association of rectangular mounds and ball courts is possible at Chaco, I doubt that the many excavators of the Bonito mounds were looking for ball courts.

In many respects the Chacoan mounds are more similar to rectangular mounds in the Classic Hohokam area than they are to the Casas Grandes mounds. Mounds may have been in use as early as 500 in the Hohokam area; and round platform mounds are definitely present by 900 in the Sacaton Phase (Haury 1976). But rectangular mounds, like those at Chaco, are limited to the later Classic Period Soho and Civano Phases (Doyel 1974: 175-177; see also Schroeder 1953). These mounds consisted of earth and trash fill contained in massive adobe retaining walls. Several are known to have had internal structures, either rooms or cells for the retention of fill. Structures of various forms were built upon these platforms. The function of these structures is unknown.

DiPeso’s third category of similarities were traits of “village organization.” The alignment of doors and vents, the regularity of various element sizes, the massive scale of building programs and many other
features all attest to centralized design processes at both Chaco and Casas Grandes. There are important differences in design, however; at Chaco, construction generally began with long parallel walls, which were then subdivided to form aligned rectangular rooms. Long parallel common walls were the exception rather than the rule at Casas Grandes, and were probably used mainly in defining compounds (discussed below). Less than 50% of the excavated rooms at Casas Grandes were rectangular. About half of the rooms at Casas Grandes were “L”-shaped, “I”-shaped, cross-shaped, or of even more bizarre forms with up to 20 right angles. While the ground plans of many Chacoan buildings resembled grids, the Paquime Phase ground plan of Casas Grandes looked more like a maze. One reason for elaborate room shapes at Casas Grandes was the ubiquitous room-wide platforms, or “bed platforms,” which were usually built into specially designed alcoves or corners. DiPeso (et al. 1974: 434) states:

... the basic Buena Fe Phase room was conceived of as a rectangle with four adjoining walls and a dividing wing wall that separated a portion of the living area into two alcoves for platform beds. This simple plan was changed considerably in the Paquime Phase ...

If true, this would logically suggest that the form preceding the Buena Fe Phase room was a simple rectangle without alcove walls, like rooms with room-wide platforms at Chaco.

However, there seems to be little evidence for increasing elaboration of room form in the Medio Period Casas Grandes. In Unit II, the best preserved Buena Fe Phase compound, the ratio of rectangular to more elaborately shaped rooms is about 50% to 50%, which suggests that non-rectangular rooms (mostly “L”-shaped) were an established part of the Casas Grandes building tradition in the Buena Fe Phase. Moreover, some of the most elaborate of the Paquime Phase rooms were built upon earlier Buena Fe Phase walls, if I am correctly interpreting Unit 8 (DiPeso 1974a: Fig. 265-4) and Unit 14 (Fig. 86-5).

The basic unit in the Buena Fe Phase seems to have been the compound consisting of either a square central court surrounded by rooms, a rectangular walled area with rooms added to the inside of the rectangle, or a combination of both. The best example is Unit II, a 26m square courtyard surrounded by single story rooms, one (or occasionally two) deep. To the north and west there are no doors to the exterior—all rooms opened into the central court. A formal colonnade entrance to the compound was located in the middle of the east side, while in the south a large semi-subterranean room (thought by DiPeso to be ceremonial) opened not to the interior courtyard but to the exterior of the compound.

The compound may have continued as the basic formal unit throughout the Medio Period. As noted above, Buena Fe Phase compounds apparently continued in use as lower stories of the large Paquime Phase Pueblo-style structure, and there is some suggestion of numerous internal courtyards or small plazas in this larger structure that may indicate an agglomeration of the older Buena Fe Phase form in the later Paquime Phase building. Within these units, DiPeso defined “family clusters” by the interconnection of doorways; these clusters ranged from 1 to 14 rooms, with a Buena Fe Phase average of 2.4 rooms, and a Buena Fe-Paquime Phase average of 4.8 rooms (DiPeso 1974: 235). However, the spatial arrangement of rooms within clusters does not appear to have been at all standardized in spite of the fact “that both the house-cluster walls and their associated components were mass produced” (DiPeso 1974a: 232). In my opinion there is little similarity between the design of residence units at Casas Grandes and those at Chaco, but it should be noted that only 15% of the large Paquime Phase structure at Casas Grandes was excavated, and that sample was limited almost entirely to the western edge of the western arm of the building. How representative that sample is of Paquime Phase domestic architecture is difficult to judge.

DiPeso (1974a: 208) suggested that “at first glance, one would suspect very little similarity between . . . the puddled adobe Paquime and the crescent shaped masonry Pueblo Bonito,” noting that at Bonito, the residential area surrounded the enclosed public/ceremonial plaza, while at Casas Grandes the reverse was true—the residential area was surrounded by public or ceremonial space.

However, Casas Grandes and Pueblo Bonito (in particular) were surprisingly similar in the spatial relationship of domestic and public space. Casas Grandes was in fact “U”-shaped with multi-storied residential building around a main plaza which opened to the south (DiPeso’s “East Plaza”). An enclosing wall ran across the front of this plaza (DiPeso 1974a: 818). Mounds, possible markets, and other public facilities were located outside the enclosed main plaza, notably to the west and south. This description would also fit Pueblo Bonito: a large multi-storied “D”-shaped building with an enclosed plaza opening to the south. If small circular rooms in the plaza at Pueblo Bonito were domestic structures, as I argue elsewhere (Lekson in prep.), the “ceremonial” aspects of the plaza become much less pronounced. Mounds were located outside this enclosed plaza, to the south, and other public areas and facilities outside the plaza may be inferred from excavations at other Chacoan sites (e.g. the East Plaza at Pueblo Alto). From this viewpoint, Pueblo Bonito and Casas Grandes seem reasonably similar, but should Casas Grandes be compared to Pueblo Bonito alone, or to the central Chaco Canyon area, specifically the cluster of buildings including...
Pueblo Bonito, Chetro Ketl, Pueblo del Arroyo and Pueblo Alto? In terms of population housed, the latter is a more accurate comparison, since the peak population of Casas Grandes is more nearly equal to that of the central canyon area than to the population of any single Chacoan site. The central Chaco Canyon area might be more comparable to the poorly understood Buena Fe Phase at Casas Grandes. This is not to imply that Pueblo Bonito is architecturally equivalent to Unit II at Casas Grandes, for example. Unit II, with rooms surrounding a small courtyard or patio, and a semi-subterranean ceremonial room to the south, may bear a superficial resemblance to early Chacoan building, but this resemblance would be applicable to almost all early Anasazi building and not specifically to Chaco.

From his comparative study, DiPeso (1974a: 211, 301) concluded that similarities between Chaco and Casas Grandes indicated that either pochteca from the same area of Mesoamerica built both, or the pochteca organization at Chaco moved south to build Casas Grandes after Chaco was abandoned. There are tantalizing similarities between Chaco and Casas Grandes, most specifically in the use of room-wide platforms and colonnades. Whether these two features are sufficient to support DiPeso’s pochteca interpretations is a matter between the reader and his credulity. Keeping in mind the important caveats regarding preservation bias in the distribution of these forms, I feel that the room-wide platform and the colonnade are strong evidence of continuity, on some level, between the two architectures. Since the basic domestic forms differ between Chaco and Casas Grandes, this might indicate continuity not of the resident populations but on the level of architectural designers, perhaps a decision-making elite. Perhaps DiPeso is correct in suggesting movement of a pochteca group, but as argued above, architectural support for pochteca involvement in Chacoan building is slim.

Similarities between the Pueblo Bonito mounds and the Classic Hohokam platform mounds are even more difficult to interpret. I have tried to suggest a close relationship between Casas Grandes and the Great Houses of the Classic Hohokam; this might suggest some connection between the Chaco mounds, the Casas Grandes mounds and the Hohokam mounds. The Chacoan mounds are rectangular, as are many of the Hohokam mounds; a similar form is not evident at Casas Grandes. Simple linear continuity from Chaco to Casa Grande to the Classic Hohokam, unlikely to begin with, is clearly not supported by the form and chronology of the mounds in these three areas. Beyond noting that similarities may exist, I offer no interpretation of this parallel form in Hohokam and Chacoan building.

Towards a Continental Context

In this section I will briefly discuss three topics: first, domestic building in selected areas of North and Central Mexico, and its relation to Chacoan building; second, continental frameworks for formal architecture comparisons; and third, the articulation of broad areal frameworks and continental processes.

**Domestic building in Mexico:**

Tula, located just north of Mexico City, was the pre-eminent city of Central Mexico during the Tollan Phase, 950–1150/1200 (Diehl personal communication). Domestic architecture at Tula (Healan 1974, 1977; Diehl 1974) was constructed with adobe walls built on low stone stem walls. Roofs were flat. Houses were single story and roughly rectangular with 5–6 rooms which may have been differentiated into kitchens, storage rooms and living rooms. Three or more of these units were built around a rectangular central courtyard, forming a “house group.” The houses were in some cases built upon low platforms creating, in effect, a “sunken” courtyard. All courtyards probably contained small central altars. Several house groups may have shared the use of small platform mound, as a “neighborhood” temple.

Variations on this theme of houses grouped around rectangular courtyards with small central altars also characterized the domestic building of much of north-central Mexico. J. Charles Kelley summarized this building as follows:

> Through the Zacatecas-northern Jalisco-Durango area northwest of the Rio Grande de Santiago one architectural pattern is found in all residential sites. This is a plan made up of one or several units, which presumably represent the quarters of an extended family/lineage group. There is a central court, essentially rectangular but poorly surveyed. The court is cut shallowly into surficial soil (especially caliche) to obtain a level courtyard. Often, but not always, there is a raised masonry altar—a square platform in the center of the court. A narrow elevated banquette or walkway surrounds the court on all sides . . . In initial stages isolated rectangular platforms of masonry walls with rubble/refuse fill are attached at intervals to the exterior of the walkways, leaving “alleyways” between them. Often there is only one central platform on the north side . . . and there are indications that this platform housed a group temple. Platform tops were cobbled and plastered; houses erected on them have rarely survived although adobe-lined firepits are common on their floors.” (Kelley, personal communication 1979)

This pattern of house platforms surrounding a rectangular courtyard with a central altar, is very unlike the Anasazi domestic unit which formed the design module of larger Chacoan building (Lekson 1981). This unit consisted of a large living room backed by two smaller storage rooms. Buildings were created by joining these units in lines, and a large subterranean circular room was constructed in front of every two or three such units. In the larger sites these units
were multi-storied, with upper stories over rear rooms terraced downwards to the front living rooms.

I stated earlier that the North American Southwest was an extension of the Mesoamerican area, and by this I meant that Southwestern architecture was more similar to that of the high cultures to the south than it was to the architecture of the Plains, the Great Basin, or the coastal West. But the argument may be positive as well as negative. The particular form of Anasazi building that we recognize as Pueblo-style—contiguous, multi-storied, flat-roofed adobe or stone masonry buildings—may have real analogues to the south. The evidence is extremely thin, but it suggests the possibility of a discontinuous distribution of Pueblo-style building along the piedmont of the Sierra Madre Occidental and Sierra Madre del Sur, from Guerrero north to the Anasazi area.

At the south end of this area, Pueblo-style structures, dating to about 1000, may be represented by several sites in Guerrero (Chadwick 1971; Lister 1971; Lorenzo 1966), and perhaps by Isabel Kelley’s (1949) "mounds containing multiple, contiguous, quadrangular masonry enclosures." At the north end of this area, of course, Pueblo-style building is known into southern Chihuahua (DiPeso et al. 1974, Brand 1935). Between these two extremes very little is known of the archaeology of the Sierra, but there are tantalizing historic accounts (or rather, interpretations of those accounts) which suggest three-story masonry building among the Acaxee and related groups in west central Durango (Beals 1932).

Frameworks for distant comparisons:

Architectural traits and details have been used to suggest long-range Mesoamerican influence on or even the direction of architectural and socioeconomic developments at Chaco. But these arguments lack an explicit comparative method (Tolstoy 1966). The presence of Mesoamerican traits at Chaco may or may not be remarkable; how are we to judge? This is a purely practical problem of defining a non-trivial problem; the pottery traditions of Chaco had a Mesoamerican origin, but few would suggest that the presence of pottery indicates Mesoamerican control. So also for architecture; how do we support our judgements of Mesoamerican influence in architecture, given the great likelihood that Southwestern architecture is as much a Mesoamerican tradition as is Southwestern pottery? A systematic approach should offer a method for evaluating, as well as recognizing, patterns of similarities. To illustrate both the problem and outline a possible solution, I will briefly examine the Chacoan phenomenon in a broader, continental context.

Chaco was not the only area north of Mexico to experience a remarkable increase in architectural complexity during the 900–1150 period. Cahokia, one of the earliest and certainly the largest of the Mississippian centers, was the scene of developments dwarfing those at Chaco. Bell (1980) feels that the peak population at Cahokia may have been during the Fairmount (900–1050) and Stirling (1050–1150) Phases.

Cahokia has been compared to a Mesoamerican Formative town (Fowler 1980). Numerous earthen platform mounds, and perhaps 13,000 timber "wall-trench" houses (Gregg 1975) surrounded a central temple and plaza complex dominated by the huge Monk's Mound, a structure on the scale of the largest Mexican temple platforms. In population and in total area, Cahokia was equal to Tula (Fowler 1978; Diehl and Benfer 1975).

Obviously, there were Mesoamerican elements in Mississippian architecture (Griffin 1966: 125). The mechanisms and relative importance of Mesoamerican influence on Mississippian developments are matters of debate, and in some ways this debate parallels arguments about Chaco. In the extreme minority, Porter (1973) has suggested that Cahokia was the product of pochteca trading operations, directly analogous to the arguments of DiPeso, Kelley and Hayes for Chaco. But the clear consensus of archaeologists investigating the Mississippian minimizes Mesoamerican responsibility for Cahokia, and "emphasizes the importance of indigenous developments spiced with an admixture of Mesoamerican beliefs and practices" (Stoltman 1978: 723). Reed (1973) even suggests that the platform mound and plaza complex was a combination of two indigenous developments, independent of Mesoamerican influences of any sort. In recent formulations of the Cahokia development (Fowler 1978, Bell 1980), Mesoamerica is not even mentioned.

A significant number of Southwestern archaeologists will claim Mesoamerican origins for Anasazi architecture, but very few Eastern archaeologists would suggest such a relationship for Mississippian building. To what extent does this difference in interpretation reflect archaeological realities, and to what extent is it the product of distinct traditions of interpretation—the Southwesternists being generally more open to Mexican diffusionist arguments than their Eastern counterparts (whose preference for indigenous development can be traced to the early Mound Builder controversies)? Unquestionably, the intellectual history of Eastern and Southwestern archaeologies cloud possible parallels between the two areas. But if we accept the consensus of Eastern archaeologists' opinion that Cahokia was an indigenous development owing little to Mesoamerican direction, we can begin to build a framework for the evaluation of Mexicanist architectural arguments at Chaco.

It should be clear that Cahokia more nearly resembled a Mesoamerican city (Hardoy 1973, Marquina 1964) than did any Anasazi settlement. While domestic architecture at Cahokia and Chaco were regionally distinct from the documented domestic architecture of Mesoamerica, in city plan and elite architecture—two areas where personal direc-
tion by a Mesoamerican elite could be most readily expected—Cahokia was "Mesoamerican" to a degree that Chaco did not approach. From a Chacoan perspective, with only a colonnade and two poorly understood rectangular mounds upon which to pin Mexican hopes, the explanation of Cahokia's development without recourse to (or in recent publications, even consideration of) Mesoamerican involvement suggests that Mexicanist arguments at Chaco are much ado about very, very little.

Continental Process:

If architectural developments at Chaco were not the product of direct Mesoamerican supervision, and it is my opinion that they were not, that does not deny effective connections between the two areas. Architecture is in many respects epiphenomenal, dependent upon the social and economic systems of the group which builds it and which it serves. Mexicanist arguments about Chaco are less concerned with isolated Mesoamerican traits than with the socio-economic discontinuity termed the Chacoan phenomenon (marked most plainly in the scale and form of Chacoan building), a discontinuity which demands explanation. Throughout this paper I have argued for continuity in architectural traits and details, but I have not attempted to explain the remarkable increase in the scale of building, the trend towards urbanization at Chaco Canyon, and the socio-economic developments of which architectural changes are only surface manifestations. We have briefly addressed this topic elsewhere (Judge et al. 1981), but the continental context suggests an alternate approach that merits future consideration.

From 900 to 1150, in a broad band running from the Colorado Plateau to the Middle Mississippi Valley, archaeological sequences are characterized by dramatically increasing complexity. Chaco and Cahokia have been briefly described; remarkable shifts towards more complex village settlement patterns are also documented in the Southern and Central Plains and in the Lower Mississippi and Caddoan areas. In the Plains, the transition from Plains Woodland to Plains Village traditions (ca. 1000) was at one time seen as so abrupt that the Plains Village tradition was considered to be a new population replacing the older Woodlands groups (Wedel 1961). So also were Woodland and Mississippian traditions once thought of as distinct peoples in the Mississippian and Caddoan areas. Judd's (1964) "Old" and "New Bonitians" were a parallel interpretation at Chaco. There are, in fact, remarkable changes in the architectures during this period in all these areas, but population replacement is no longer supportable in any of these regions. Rather, changes are seen primarily as reorganizations of indigenous populations. These reorganizations are now seen as adaptive, and have been linked to regional climatic change (e.g. for the Colorado Plateaus, Euler et al. 1979; for the Plains, Lehmer 1970, Lintz 1974; and for the Mississippian areas, Griffin 1961, Baerris and Bryson 1965).

Similarly, the northern frontier of Mesoamerica has been linked to regional climatic fluctuations (Armillas 1969). On the Mesa Central, the northernmost extension of the clearly defined Mesoamerican culture was evidently from 950-1150 at Zape, in northern Durango, Mexico (Kelley 1971: 795).

In these cases, climatic change is thought to have either permitted or encouraged increased reliance on agriculture. The relationship of increasing reliance on agriculture and increasing social complexity is a topic far too broad for discussion here, but that the two are closely related is a truism. But climatic change and increasing archaeological complexity have been considered mainly on the regional level. Contemporaneity of regional developments in a broad latitudinal belt in the central and southern United States, and the maximum northern extension of Mesoamerican culture in Mexico may have been fundamentally related by continental, or more accurately, hemispherical climatic processes. This suggestion is, of course, not new, but the empirical foundations for such an argument have only recently been proposed by Gunn and Adams (1981). They argue that "precipitation in the southern U.S., northern Mexico, and southern Mexico is controlled by the coordinated north or south movements of bands of dry and wet air" (Gunn and Adams 1981: 90) and that these movements may contribute to the "rise and demise of civilizations" (Gunn and Adams 1981: 98). Increasing complexity in Chacoan architecture could then be seen as a local development triggered in part by continental climatic change, climatic change which also figured in the Mississippian and Plains developments as well as the northward extension of Mesoamerican culture. At the same time the Anasazi area was reaching levels of unprecedented complexity, and perhaps maximum receptivity to inter-regional elite styles in architecture (Sharp 1978), Mesoamerican culture was reaching its closest approach to the North American Southwest. It is not surprising, under such conditions, that some Mesoamerican architectural forms, such as the colonnade and perhaps the rectangular mound, should be found at Chaco.

Acknowledgements

Several individuals have been quite generous in offering unpublished data and "off-the-record" conclusions—for my use of which, they are, of course, innocent. In particular, I must mention J. Charles Kelley, Richard Diehl and Warren Wittry, who shared unpublished data. At the Chaco Center, Marcia Truell, W. James Judge and particularly H. Wolcott Toll aided me in improvement of both form and content. The notion of hemispheric climatic change and its in-
fluence on Chaco was first suggested by William Gillespie, who did not have a chance to review my use of his idea and who may decide to be more careful about what he says around me in the future.

Questions and Comments

Al Schroeder: I would comment that somewhat similar things were happening in that 900 to 1100 period in the Hohokam too.

Jim Judge: He (Lekson) does get into that in referencing Casa Grande, a little later; now, what you are saying is that prior to Casa Grande, prior to the late phase, things were happening?

Schroeder: Yes, like the sudden development in the settlement pattern in that it was spreading all over the place, and the appearance of the small ball court, the greater trade with Mexico, the appearance of copper bells, the plaques and things of this sort. There was a sudden development going on among the Hohokam in the Sedentary Period.

Judge: Well, I really feel it's essential that we don't get too provincial, and most certainly I'm guilty of this and, I think, a lot of people. When you begin to look at a more broad regional and (what Steve is doing) a continental context, things begin to fall into place. Certainly the eleventh and twelfth century developments toward complexity throughout this whole area are interesting. Whether or not they are related to climatic causes (and Steve is very careful not to make an extremely causal argument), the fact that increased or climatic amelioration would favor such complexity is, I think, the key.

Schroeder: Has he by any chance referenced my recent article in the Eric Reed volume on the Hohokam and Chaco relationships?

Judge: I think so, Al. I didn't read the references and unfortunately he didn't give me the bibliography, but he is certainly aware of it because we published in the same volume.

Ted Frisbie: One other comment. Living ten miles away from Cahokia and not giving a damn about it, it's difficult, but that's my opinion . . . When we teach a team-taught course in North American archeology, my midwestern colleagues, of course, do the Midwest and I do the Southwest, and when we start comparing back and forth, as was done in this paper, there are some really striking things that have already been indicated. But when you get later in time, we get the emphasis on the Katchina Cult coming in; it comes in the Southwest a little earlier and I'll talk about this, but it doesn't really get big, with murals and whatnot, until the P-IV. Interestingly, what you find in the Midwest with the Mississippian late, at the same time as the Death Cult, both of which, again are Mesoamerican. We don't know what the connection is there yet, but there seems to be another (here I go, Mesoamericanist) thrust out from Mesoamerica of a major ceremonial thing. Slightly different as far as the manifestations go, but nevertheless very, very similar once again.

Judge: 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.
A number of recent studies have emphasized the importance of trade networks among the Anasazi. While some investigators focused upon an evaluation of long-distance trade with Mexico (Kelley and Kelley 1975, Mathien 1981a, and McGuire 1980), Upham (1980) stressed more localized trade systems. In all instances, the distances involved necessitated the presence of a mobile trader(s) to link various Puebloan groups with one another or with culture groups outside the Puebloan area. Therefore, an understanding of the principles under which a mobile trader operates are useful. These can be applied in the reconstruction of prehistoric trade networks which may have operated among the Chacoan Anasazi and their neighbors.

The Mobile Trader

Plattner (1976) was concerned with variables which would create a need for mobile traders in any area, and not just in modern market systems. While he studied the development of periodic markets cross-culturally, he was particularly interested in the early stages of regional development. His discussion of the role of mobile traders in such developing areas without markets should be appropriate in all situations, historic and prehistoric. It explains when and why such an occupational niche occurs, who would occupy the position, and when and why such a niche would disappear.

The mobile trader can be any one of several different types: a) itinerant marketers who trade out from a central place to various marketplaces and set up fixed stalls in each place, b) long-distance itinerant peddlers who go out from central places to sell door-to-door among dispersed rural homesteads without marketplaces, c) itinerant street peddlers with fixed routes in urban neighborhoods relatively distant from fixed sources of similar goods, and d) street peddlers with minimal stocks who may sell in the marketplace itself (Plattner 1976: 70). In each of Plattner's categories, one major difference is the distance covered by the mobile trader who transports goods from the area of availability to consumers. The goods cited by Plattner range from dry goods and hardware for long-distance peddlers to prepared food, etc. for street peddlers.

Two reasons for the existence of this occupation are: a) alternative means of earning a living offer extremely low return, and b) this occupation offers an opportunity to achieve a return on one's own capital (Plattner 1976: 87).

There are two economic principles which apply to the case of the mobile trader. First, there is a max-
The Mobile Trader...

imum range or distance a consumer will go in order to obtain a good. This will be evaluated by the consumer based not only on his need for the item and the cost of the item (whether or not he can pay for it, as well as how much time and work would be lost while away from his own work), but also on the cost of transportation to and back from the vendor’s location. Second, there is a minimum threshold range for a trading firm. Profits available in the area of the seller’s location must be sufficient to support the vendor on a full-time basis. If not, mobility of the vendor throughout a larger area is necessary to increase the number of consumers. When the minimum income threshold range falls within the maximum distance range consumers will travel, the vendor’s location can be fixed. However, if the opposite holds true, the vendor may choose to move his location periodically in order to include sufficient consumers to offset the costs of transportation and provide a gain in sales (Plattner 1976: 72–73).

The areas encompassed in the route can also be approximated. Plattner (1976: 80) stated that the central area or central place often will not support mobile traders. When fixed stores in the central place offer a range of goods, the variety of goods offered and proximity to customers decreases transportation costs per good and enables fixed vendors to operate. The zone of decreased population density and commercialization, where increased transportation costs affect the consumer’s decision to purchase a good but where the number of vendors decrease to the point where only one or two control the supply available to the buyer, is the area where the mobile trader would make a viable income. Once the distance outward from the center reaches the point where sales per day drop below the threshold income needed, it is no longer profitable to offer goods even if the peddler has a monopoly over the supply of goods.

There are several benefits to mobility. When a periodic market situation exists, the periodicity of the vendor’s appearance affects the market in the following way. Purchase needs are stored up and concentrated sales occur within a particular time period. When consumers know that a good will be available to them on a periodic basis at a price they can afford (possibly because of decreased transportation costs, e.g. less time involved and alternative uses of this time), demand may be increased. When the demand satisfaction can be achieved only at known intervals, the vendor benefits by obtaining an increased volume of business in a compressed time interval. This frees some of his time to either pursue other interests or travel to other hinterlands to increase his market range.

Several factors determine a vendor’s trip. In addition to relationships between the size of the maximum demand range and the minimum threshold range, Plattner (1976: 84) cites: a) time it takes to travel from central town to rural selling area, b) time it takes to exhaust demand in the average consuming community, c) time it takes to travel between independent consumer settlements, and d) period beyond which the vendor will not want to remain away from his customers because of danger of competing traders.

Briefly, Plattner’s discussion states that a mobile trader exists when the price of a good makes it too dear for the consumer to make an individual effort to travel to a central fixed location to obtain it, but where periodic demand is sufficient for the vendor to transact enough exchanges to cover additional transportation costs and still obtain a profit high enough to insure a satisfactory living.

Several comments are pertinent to any evaluation of mobile traders. First, profit is not limited to the price obtained when a good is exchanged. Vendors may purchase rural products to sell in urban markets and thereby gain additional profits. Plattner (1976: 81) sees such peddlers who buy farm goods as well as sell urban goods as facilitators of development of integrated systems. He also noted that as a region develops, transportation improves, and commercialization expands outward from the center, the need for the mobile trader disappears in that area. The trader’s choices are to settle in the area as a fixed storekeeper or move outward into new frontier zones.

If a particular good (either raw material or finished product) is available in a hinterland and the profit to be obtained by a mobile trader who successfully brings it back to the central place is large enough to offset the risks involved, there should be sufficient incentive to attract long-distance traders to venture into the hinterland. This would be especially true if they were already aware of the presence of the item in the hinterland which would bring high prices in the core area. If initial attempts in this new trade are successful and the demand in the home market is sufficient, repeated attempts would follow. Whether or not this develops into a regular pattern would depend upon continued demand or market for the good, ability of the mobile traders to secure good relationships with the suppliers to warrant continued risk taking, and availability of the particular good in other geographic areas where competing factors are not equally balanced, as well as other economic considerations.

The point emphasized is that profit can be gained in different ways. In some situations, the product carried from the center to the hinterland may not be in sufficient demand to generate enough profit to cover the cost of manufacture and transport. However, if it is exchanged for goods which are inexpensive or abundant in the hinterland, and therefore readily exchanged in quantity, but difficult to obtain in the center and therefore in much demand and high in price, then a mobile trader could gain extra profits by their sale and still cover expenses. Wallerstein’s (1974) description of long-distance traders who seek out luxury items and take them back to a core area are ex-
examples of this type of mobile trader. In these instances “money” may not be necessary. Since cultural boundaries are often crossed and the value of a good is dependent upon its scarcity in an area where it is traded, barter or exchange of other locally scarce items would be a more useful method of exchange.

In non-commercialized systems where a hinterland or any regional population has not reached a level of social complexity which supports fixed stores or full-time mobile traders, e.g. the San Juan Anasazi, it is possible to use the same economic principles to evaluate the functions of a long-distance exchange system. Plattner (1976: 76) cited reciprocal exchange typical of tribal societies as the first stage in the development of periodic trading systems and used the kula ring of the Trobriand Islands as his example.

Application to Prehistoric Populations

There are several approaches which can be taken in an attempt to understand the role of the mobile trader prehistorically. One is to review the ethnohistoric data and make inferences about the past. This may or may not be accurate, depending on the amount of change in social organization and population dispersion during the intervening centuries. Another is to examine the archaeological evidence and infer the level of social complexity which existed in a particular prehistoric region and the level of interaction with other regions. In this paper, a combination of both approaches will be used in an attempt to reconstruct the role of mobile traders among the Chacoan Anasazi.

There is considerable evidence among the historic Pueblos that trading groups spent two to three months during certain parts of the year traveling outside of their own culture area to obtain goods not available locally. Other culture groups visited the Pueblos for similar reasons. And interaction among various Puebloan groups has also been documented.

Lange (1968) provides data on Cochiti trade. In order to obtain salt, 10-20 interested men organized parties which were led by any older person who had previously made the trip. Using burros to carry provisions, trips were always to the Salt Lakes of the Estancia Valley, almost 100 miles (160 km) to the southeast; trips were never made to the Zuni Salt Lake or elsewhere. These took approximately two weeks and sometimes longer. Lange (1968: 142) noted that Goldfrank had obtained similar data during her studies of the Laguna Indians.

Cochiti Indians also traveled to the Hopi area and from there accompanied the Hopi to the Havasupai area to trade for deerskins. Trips to Taos and further into Oklahoma facilitated the exchange of bread and cornmeal for horses and buffalo hides. Trade was carried out on the plains of central New Mexico which also were hunting grounds for antelope. Several longer trips to California and Mexico were documented by Lange (1968), but these were felt to be exceptional.

Fairs and ceremonies at neighboring pueblos provided a number of opportunities for the Cochiti to exchange goods. Wares were often sent with another relative or friend if a person could not go himself. Bread, corn, fruit, drums, jewelry, cloth and skins, blankets and pottery were exchanged, as well as Olivella, abalone and other shells. Trips to Laguna were often made to contact the Navajo as Window Rock was considered quite distant. The Cochiti usually went to a friend’s house where trade was conducted. Chili, watermelon, and bread were exchanged for turquoise, silver, blankets and saddles. Navajo traders also exchanged beads and horses (Lange 1968: 152-154).

Traders from the Hopi area came to Cochiti during intervals between ceremonies and on other special occasions. From Zuni, traders came regularly every year. In addition to trade per se, gifts were exchanged during these yearly encounters. Exchanges were usually bread, corn and agricultural products for jewelry, meat and blankets. Intrapueblo trade was usually person to person, and a family tended to monopolize the opportunity by sending the visitor to their relatives or informing their relatives of the trader’s presence before telling the rest of the community (Lange 1968: 156). Lange felt that while modern transportation allowed for increased frequency of trips, it perhaps did not affect the radius of travel.

A salt procuring expedition by the Hopi in 1912 has also been documented (Simmons 1942 and Titiev 1937). In this instance three men participated: one as the acting chief, one as the experienced but common member, and the third as a novice. This was the standard composition of a salt procurement group even though in the past parties had been larger, representing one man per house or clan who went annually. The decrease in party size may have been the result of lack of enemies encountered during later times. Expeditions usually took place in the fall after the harvest had been gathered. The 1912 expedition took four days (two each way from Moenkopi to the salt source on the Colorado River just below its junction with the Little Colorado) and included the use of four burros to carry provisions. The amount of salt obtained (approximately 60 lb/man) was conditioned by the ability of the men to climb down into the canyon and ascend with their burdens, and not by the ability of the burros to transport the goods. Moenkopi is approximately 35 airline miles (56 km) from the junction of the Colorado and Little Colorado Rivers; the distance on the ground is probably somewhat longer.

Simmons (1942: 234) also noted that Havasupai visited Moenkopi to attend dances.

Frisbie (1975) documents several instances of long-distance travel ethnohistorically to procure shells. These include Zuni pilgrimages to the Pacific Coast.
The earliest documented evidence of mobile traders among Southwestern cultural groups including the Pueblos is found in the chronicles kept by the sixteenth century Spaniards who moved north and west from Mexico City into New Mexico. Riley (1976) presented a summary of this evidence; from it we learn that trips over long distances were infrequent.

Bandelier (1890: 39, 63) gave an account of Zuni trips to the Seri and Opata tribal areas in Sonora to exchange turquoise for shell, parrot feathers and plumes. These distances are approximately 700 km in one direction.

The earliest documented evidence of mobile traders from Mexico City into New Mexico. Riley (1976) presented a summary of this evidence; from it we learn of the following:

1. 1530: Reports of a Huastec Indian from Oxtipar (Valles, San Luis Potosi) whose father traveled north into the interior to trade rich colored plumes for precious metals. He reached an area with seven pueblos near Casas Grandes, Chihuahua after traveling 40 days over desert country.

2. 1539: The expedition of Marcos de Niza/Melchior Diaz documents trade between the area of the Upper Sonora/Yaqui Rivers and Cibola (Zuni) to exchange hides and stones. This is a distance of approximately 700 km. No time factor was given. It was noted, however, that it took seven days for expeditions to go between Cibola (Zuni) and Totonteac (Hopi) to obtain cotton. This is a distance of approximately 190 km (linearly).

3. 1540: Hernando de Alarcon noted that trips of 30-40 days along a river path allowed communication between Indians in the area of the Colorado/Gila junction with those from Cibola. For areas south of Cibola this took two months. The Coronado chronicles also document excursions between the Pecos Indians and those on the Plains, presumably to Kansas.

4. 1581: The Chamuscado expedition noted that it took 13 days, according to the natives, to travel from La Junta at the Rio Grande and Conchos north to the pueblos near El Paso. This is a distance of approximately 340 km.

5. 1604-1605: Onate documented exchange between the Lower Colorado and Zuni inhabitants, as well as between the Croados (probably the Yavapai) and the Hopi. “Coral” was obtained from the southern region.

While these data are minimal, they do allow the archaeologist to make several assumptions about prehistoric trade. First, perishable commodities (particularly food items) were probably not traded for long distances outside of the Pueblo area. While historic evidence suggests the Cochiti took as far as Laguna and Taos with breads, chili, and watermelon, etc., the earlier writers mention only goods which would preserve for long periods of time when they discussed distances of 700 km or more. Second, using the distances between the areas involved, it is possible to estimate average distance traveled per day. The trips from the Hopi area for salt covered approximately 35 miles (56 km) and took two days each way for an average of 28 km/day. The Cochiti covered 100 miles (or 160 km) one way on their salt expeditions; if it took two weeks round trip, they must have averaged approximately 23 km/day. The 190 km between Hopi and Zuni during the sixteenth century took seven days, or approximately 27 km/day. In this latter instance there were no pack animals involved, yet the distances per day are similar. Using a distance of 700 km between Cibola and the Lower Colorado, a 30 day trip would average 24 km/day while a 40 day trip would average 18 km/day. The 340 km between La Junta and El Paso took 13 days, which would average approximately 26 km per day. Frisbie (1975: 130) also provides an example of distances traveled in the late nineteenth century by Pueblo Indians. A trip of 220 miles (336 km) from Walpi to Jemez took 8-10 days. This is an average distance/day of 33-42 km. These estimates suggest that a distance of 20–30 km/day is likely when a man travels for several days at a time. They do not negate the observation cited by Riley (1976: Footnote 2) that a Tepehuan could go 50 km/day with heavy loads through the Sierra Madre. However, Riley did not state how many days in a row this distance was covered or whether there were times when the person stopped to rest and/or recuperate. Various chroniclers also note that the Spaniards stopped to let their horses rest at times during their expeditions north into New Mexico. Kessel (1979: 147) documented the amount of time it took Spanish missionaries in the mid-1600’s to make the round trip from Mexico to New Mexico with their supply trains. One way travel time was six months, another six months were spent in New Mexico and a third six months were needed to return. Kessel (1979: 148–149) noted that there were numerous wagons, mules, cattle, mail and other goods going north to the colony, and hides, salt, nuts and other products traveling south. If one uses an estimate of 2,000 km from Mexico City to New Mexico, then their average distances were approximately 11 km/day (180 days). Based on all the above, it is unlikely that a prehistoric Indian would have been able to proceed much faster than 20–30 km/day for long periods of time. Longer distances per day, however, could have been covered during shorter periods of time.
In summary, the ethnohistoric evidence indicates that Pueblo Indians:

1. Traded perishables within their general cultural area or with nearby neighbors only.
2. Traded non-perishables over distances up to 700 km.
3. Were agriculturalists who participated in exchange as a part-time occupation. Exchanges were made in person and at intervals which correspond to ceremonial occasions or during annual trips. Frisbie (1975: 128) provides a similar interpretation.

Placed within the framework of Plattner’s discussion, these part-time mobile traders would exist because:

1. The goods obtained through long-distance trips did not provide sufficient gains to compensate for the loss of subsistence goods which would have been earned if trade were a full-time occupation. Or, demand for trade items was too low to warrant more than periodic trips to distant areas. These trade items or imports may also have been too expensive to warrant travel by individual consumers to obtain them. Therefore, group trips during the non-subsistence season would provide the best solution to the problem.

2. The occupation did not offer means to achieve a return on one’s own capital. In these instances a single person’s capital would not warrant the venture, but by combining the goods of a group and use of selected personnel, such return could be obtained. This necessitates a sharing or redistribution of the profit on capital investment after the expedition returns. The number of people involved in the trip will depend on a number of variables such as the bulk of the items to be exchanged, the means of transportation, and the risks encountered when dealing with foreign groups or strangers.

In the above situations, trips were made when supply, demand and economic reward supported the venture. Economic reward need not always be material gain, but could result in increased status or some other social gain (Schneider 1974). The point stressed, however, is that economic theory is applicable.

Can these inferences be applied prehistorically? Recently Upham (1980) examined the Western Pueblos of the fourteenth century; the area encompassed in his research extended from the Mogollon Rim to the Hopi Mesas, and from Acoma to the Verde Valley. Examination of ethnohistoric Hopi, Zuni, and Acoma living in the same area suggested to him that the political and economic organization at the time of the Spanish Conquest may have been more highly structured than the present egalitarian societies. He suggested that the Western Pueblos in the fourteenth century may have had a hereditary clan system through which a greater degree of socio-economic-political coordination was maintained in these societies. His examination of fourteenth and sixteenth century settlement patterns supported the idea of a previously more complex social system among these Western Anasazi. How much more complex, given that the Southwest is a marginal environment for agriculture, especially if dependent on rainfall, is uncertain. However, Upham’s (1980) settlement pattern studies and ceramic evidence did suggest the presence of regional centers which interacted through elite in exchange networks.

The ceramic evidence discussed by Upham provides data regarding trade on the Colorado Plateau as well as Puebloan links with the Salado area. The presence of turquoise and marine shells also suggest that some contacts with other areas must have existed in order to obtain these materials. The long-distance trade of hard goods was felt to fall under the direction of a few rather than the entire community of decision makers. Differing frequencies of appearance of four ceramic wares (Jediditow yellow, Winslow orange, White Mountain red, and Zuni glaze) suggested to Upham that there were elite alliances within the Western Pueblos as well as links to other groups with similar alliances in the Salado area. More detailed examination of one particular region, the Anderson Mesa, also suggested to Upham that Chavez Pass may have functioned in a manner similar to a port-of-trade, linking three ecological zones within the region. Within the Anderson Mesa cluster, only the large sites at Chavez Pass contained exotics such as turquoise, marine shell, pigment, minerals, figurines and copper bells. These items were not found at sites in the hinterland. The shells and copper bells would have entailed the longest distances to a source area or some form of trade network for their procurement.

The presence of obsidian in all forms except cores at Chavez Pass, and in various stages of manufacture at two secondary centers near the source, but only as finished products in the hinterland sites, suggested to Upham that there was some degree of specialization. Upham also calculated the amount of labor needed for construction of two sites at Chavez Pass (CPS No. 1 and CPS No. 2); the large amounts of basalt and numbers of trees would suggest that a considerable labor force had been organized, especially since Upham feels that these sites were built during one major construction episode. This again suggests some form of social organization other than egalitarian to plan and coordinate these efforts.

With regard to the exchange of food products, however, Upham based his discussion on Lightfoot’s (1979) proposal that 50 km is the maximum radius covered before energy involved in transporting foods exceeded that provided by certain food products. In the entire area under study, only one (Hopi) of the nine regions among Western Pueblos in existence during the fourteenth century would have had over a 50 km span to connect both peripheries of the particular region. He felt that among these Western Pueblo regions, food could have been exchanged in a down-the-line manner in the nine regions. He also felt food
could have been traded in this same fashion between the Silver Creek-Puerco, Puerco-Upper Little Colorado, Puerco-Zuni, and Upper Little Colorado-Zuni regions at that time.

In summary, Upham presented a case for the existence of a more complex social organization among the Western Pueblos during the fourteenth century. The distances involved in the acquisition of goods not locally produced, however, do not differ from those discussed earlier for the ethnohistoric Pueblos. The Chavez Pass site may have functioned as a port-of-trade within the Western Pueblos of the fourteenth century.

Other studies provide similar data regarding the long distances involved in procurement of non-perishable items. DiPeso (1980) noted that at Casas Grandes such imports came from distances less than 800 km. These included shell (400 km), turquoise (247 km), serpentine (282 km), sepulolite (262 km), obsidian (750 km) and alibates dolomite (765 km). Hudson (1978) examined several items traded throughout the American Southwest; her results also suggest that certain items considered “exotic” to archaeological sites were usually found within a 700 km radius of their source. She assumed all materials examined in her study came from the nearest known source. There was a relatively gradual decrease in percent of sites with shell present until a distance of 640 km from the nearest source was reached. Once past this point, this percentage decreased sharply. For turquoise, there was a fairly random distribution pattern for its presence up to 200 km from the nearest source. One major exception to this pattern of turquoise distribution was Chaco Canyon. Hudson did not fully explain this aberration except that there was probably a different distribution system in effect for Chaco. Her examination of obsidian revealed a distribution pattern of 160 km before noticeable changes in fall-off rates occurred.

During recent National Park Service studies at Chaco, similar results were obtained. Sappington and Cameron (1981) reported the following distances to known sources of obsidian found in Chaco Canyon sites: Red Hill (227 km), San Francisco Mountains, Arizona (516 km), Polvadero Peak (144 km) and Grants Ridge (97 km). Studies of other minerals and ornaments from Chacoan excavations (Mathien 1981b) conclude that only copper bells, shells, macaws, and turquoise were not available within the San Juan Basin. Turquoise was available within 200 km of Chaco in the Cerrillos mining district and at just a little over 200 km at mines in Colorado. Numerous other turquoise mines are within a 600 km radius of Chaco. Assuming that the area around Casas Grandes was a trade node and functioned as a port-of-entry or source area for macaws and copper bells entering the Southwest, these would have been available within 700 km of Chaco. DiPeso’s (1974) excavations at the site of Paquime provide evidence to support this proposition during the Medio Period. During earlier periods, e.g. A.D. 920–1020 to which bones from a macaw skeleton found in the fill of Kiva B at 29SJ1360 in Chaco Canyon are assigned, another site (or area) may have functioned in this capacity. A distance of 900 km would make shell items from both the Gulf of California and the Pacific Coast available.

While many ceramics were imported to Chaco, the majority of these came from localities within the San Juan Basin. Non-Basin ceramics identified during recent studies include White Mountain redwares, polished smudged wares and Tusayan white wares from Eastern Arizona, igneous tempered San Juan wares, and occasional pieces from the Socorro and Ceboletta areas (Toll, Windes and McKenna 1980). All these areas surround the San Juan Basin and indicate contact with neighboring culture groups. The few unusual sherds recovered from Pueblo Bonito (polychromes housed at the American Museum of Natural History) are definitely out of place in Chaco; however, it is not certain that they represent a Mesoamerican pottery. Because these have not been examined recently and their foreign origins have not been pinpointed, the staff at the Chaco Center cannot identify them with certainty. Examination of a slide provided by Jonathan Reyman is difficult; they may represent later Anasazi polychromes used by the Navajo. Similarly the vessels identified in the literature as Gallina ceramics (because of their pointed bottoms) may also be Navajo (Peter J. McKenna, personal communication, August 1981). Until the ceramic experts have had an opportunity to re-examine this pottery, no definitive statements can be made. However, if these unusual examples do prove to be Navajo imports into the canyon, then it is likely that the Chacoan Anasazi did not have relationships with groups in excess of 700-900 km from the canyon.

With regard to how well organized the Chacoans were, evidence of social stratification is seen in burial practices (Akins and Schelberg 1981). In addition, planning for the building of standardized construction units and the organization of a considerable labor force suggests some type of leadership was needed during the construction of the large towns (Lekson 1981). Some specialization in the manufacture of turquoise ornaments may also have existed (Mathien 1981b); however, this probably was carried out by part-time craftsmen. At present, it is not possible to specify the level of socio-political control exercised by the Chacoan elite, but the data do suggest that it was more complex than in modern Puebloan society.

While there is evidence of complex social organization from two prehistoric areas, the Western Pueblos of the fourteenth century and the Chacoan Anasazi of the eleventh century, how would such organization affect the role of a mobile trader? Although Upham (1980) has compared the Chavez Pass site with a port-of-trade and Judge (1979) has proposed that Chaco Canyon may have functioned as a redistribution
center in the San Juan Basin, there is no convincing evidence to date of the existence of fixed stores or full-time trading specialists in either area. Both Upham (1980) and Kelley and Kelley (1975) have suggested that the kiva may have functioned as such a locus for exchange within the community. However, archaeological data neither deny nor confirm this suggestion. There is a lack of clearly discernible features with these structures which can be correlated with storage in kivas if these functioned as redistributive centers.

Approaching the problem by examining imports into a region is one way to estimate the role of the mobile trader in Anasazi society. Using the models for long-distance trade provided by Renfrew (1975: 46–48), a preliminary evaluation of data on ornaments and “exotics” (as one category of artifact) among the Chacoans was made (Mathien 1981a: 140–143). A down-the-line model, supplemented by a prestige-chain model, best fit the data. This model needs to be critically evaluated and tested.

The down-the-line model was based on Renfrew’s observations of obsidian distribution patterns in the Near East. When an item, such as copper bells, is evaluated as one in a set of similar luxury items, the number of copper bells should be somewhat readily available for a distance of 200–300 km (the area of direct contact) around a source or supply zone. When the distribution of these artifacts is plotted with respect to distance from the source, the fall-off rate decreases gradually with increased distance. Once outside this radius of 200 km, the proportion of artifacts falls rapidly to about 0.1 percent at a radius of 600 km from the source. Assumptions made include a long series of successive exchanges of material from a point source, and a reciprocal exchange system (Renfrew 1975: 46–48).

Studies of other minerals and ornaments from Chacoan excavations (Mathien 1981b) conclude that only copper bells, shells, macaws and turquoise were not available within the San Juan Basin. Using rounded numbers based on the examination of over 4,000 ornaments and exotic imports recovered during recent National Park Service excavations in Chaco, the following observations can be made. One macaw was recovered, representing 0.02% of this total. Three copper bells were recovered, or 0.07%. A total of 595 shells were recovered, or 15.0%. The shells are well above the 0.1% expected value estimated by Renfrew at the 600 km distance. Shells exceed the 600 km distance by 300 km and must signify an irregularity in the system if this mode of exchange was used. If the Glycymeris shells were not included in these calculations, the 67 other shell species comprise 1.7% of the imports, still above expected. (The three shell species listed were imported since Basketmaker III/Pueblo I and are also the three most abundant shell types found during excavations. The remaining types occurred infrequently and it may be that their meaning in this society was not identical.)

While Renfrew’s model was applied in reverse in this case (measured from the sites rather than the source), it does suggest that if Renfrew is right, a down-the-line mode of exchange could have operated in order to account for some of the exotic materials present in Chaco. The shells, however, may represent a different mode of exchange. One possible explanation for this phenomenon can again be taken from Renfrew (1975: 50–51, Figure 14). Shell items may operate under a variant of the down-the-line model which he called prestige-chain trade. Here, goods are transferred between specific notable persons. The distances between these trade partners are, on the average, longer than those between reciprocal trade partners who are not members of this group.

If one accepts Frisbie’s (1975) review of the data on shell and his conclusions, then shell may have been used as one form of “money.” Amounts in Chacoan sites increase through time. Similarly turquoise appears in Basketmaker III villages, and amounts of this material also increase through time. Frisbie (1975) suggested it may also have functioned as “money.”

The relative volume of exotic items found within Chaco does not suggest the presence of specialized traders. Using data from prior and recent excavations, only 33 copper bells and 36 macaws exist to date. These items occur in proveniences which span a 300 year time interval. The low volume of imports suggests very limited long-distance exchange, especially if one or more items were traded during one meeting. Whether or not such transactions for multiple luxury items occurred is currently unknown. (Hargrave [1970] reported that the 13 macaws found in Room 38 in Pueblo Bonito were distinct from other Chacoan macaws and may have been obtained from a different area. If these had been obtained at one time, the number of trade contacts needed to obtain luxury items would have been fewer.)

These archaeological data indicate that there probably was not a major change in the long distances covered in prehistoric times and ethnohistoric times. This is probably attributable to the means of transportation utilized. As was noted above, the distances from Cibola to the Lower Colorado took from 30–60 days depending upon the routes followed and the nodes connected. These nodes included various culture groups during the sixteenth century, and it is not unlikely that numerous culture groups interacted in order to obtain non-subsistence items during the eleventh and fourteenth centuries. Mobile traders were few and probably not very highly organized; they may have been big-men or incipient chiefs, accompanied by some members of the agricultural community, going to fairs in other regions. Trips to other areas were probably scheduled during periods when attention to agricultural needs were at a low point, e.g.
after the harvest for long-distance trips which required considerable periods of time. (In contrast, shorter trips to neighboring communities involving a few days absence would not impair or harm their crops.)

If one assumes that living in a semi-arid environment with only sporadic rainfall limits the ability of the inhabitants to support non-productive members of the society for many years at a time on accumulated surpluses, then it would be expected that most families would be agriculturalists who would participate in other types of activities in their spare time. Such activities would be only part-time and could range from hunting, gathering of resources, manufacture of objects (e.g. ceramics, textiles, ornaments, tools), trading, ceremonial functions, etc. One would then expect to find archaeological evidence of few, if any, specialists. Rather than supporting full-time traders, it might be a better adaptive strategy to obtain goods during other occasions which provided economy in time and increased efficiency in transport by combining purposes for trips. With the introduction of water control features or other technical innovations, the number of specialists supported could be increased. The level of social complexity within the American Southwest may correlate with the arid environment, the level of technical development, and the lack of pack animals which limited the means of transportation to foot travel. However, the number and frequency of trips to neighboring culture groups may not have differed much from those suggested by ethnohistoric records. Using Plattner’s (1976) concepts about mobile traders, the evidence for long-distance trade among the historic and ethnohistoric Pueblo Indians suggests that exchange systems were probably in the “early” stages of regional development in the American Southwest.

How does this fit with other economic models? Wallerstein (1974), using the work of Cippola (1967), suggested that a distance of 40–60 days, depending on the mode of transportation, is the maximum area encompassed in any “world-system.” A “world-system” is:

An extensive division of labor, not merely in a functional (or occupational) sense but geographically, partly due to ecological factors and the social organization of work, which magnifies and legitimizes the ability of some groups within the system to exploit the labor of others, that is to receive a larger share of the surplus (Wallerstein 1974: 349).

This “world-system” can be an integrating factor, either politically or economically. However, until production for markets existed, no “world-system” existed. Wallerstein made distinctions between trade in Europe during the Middle Ages. His contrast of feudal manors with later city states makes it clear that an area such as the prehistoric American Southwest would not qualify as a “world-system” even though long-distance exchange/procurement of “luxury” goods did exist. His model of a “world-system” definitely prescribes the existence of some type of state level organization.

Wallerstein’s work has been used by some investigators (DiPeso 1980; Pailes and Whitecotton 1979: Weigand et al 1977) to model possible relationships between Mesoamerica and the American Southwest. Elsewhere (Mathien 1981a) this model has been examined in detail as it pertains specifically to the Chacoan Anasazi. The concepts of pochteca and trocador used by some investigators (e.g. Kelley and Kelley 1975) were found to fall within the less culture-bound descriptions of mobile traders, and the effects of earlier mobile traders from areas within Mesoamerica proper were evaluated. It was concluded that they would have had little, if any, effect on the development of the Chacoan Anasazi. This was not for the lack of a state system in Mesoamerica, but because the products involved in the proposed exchange network could have been obtained without going as far north as Chaco Canyon (or as far south as Mesoamerica) and because the distances involved (given that the mode of transportation was on foot) were well beyond the 40–60 day radius proposed in Wallerstein’s model from the Mesoamerican core area. DiPeso (1980), who used a 25 km/day average that a man could walk, noted that his data from Casas Grandes as well as other data from the Near East and Formative Mesoamerica fell short of the 1250 km which could have been traversed under Wallerstein’s model. Instead, the evidence available suggests that 900 km was the maximum distance encompassed. If one accepts the discussion provided above wherein it was suggested that trips spanning longer time periods may have necessitated a slower pace, and also accepts the ethnohistoric documentation that it took 40–60 days to go from Cibola to the Lower Colorado, then perhaps we might have a better estimate of our prehistoric American mobile trader’s range.

While the Mesoamerican state may have supported full-time specialists, Plattner’s discussion of distances covered by mobile traders suggests that:

1. The time involved to make trips from Central Mexico (or Mesoamerica) to the Southwest may have been excessive.
2. The demand for luxury goods in the consuming community may not have been sufficient to warrant the exchange because of the low population density and the few elite in the Southwest. In the Southwest, the elite are not numerous and the archaeological evidence available does not support the concept of great differences in material wealth. The number of imports from Mesoamerica are limited. Quoting Haury (1976: 347):

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If all the Mexican goods recovered from precontact sites are brought together, aggregating several hundred bells of copper, a handful of mirror backs, and some miscellaneous materials, 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."

3. The period beyond which the vendor would not want to be away from customers would be too long. As noted above, the Spanish missionary supply trips took a total of 18 months.

In total, average sales per day probably were below the threshold income needed to make the trip profitable even if the Mesoamerican mobile trader had a monopoly on the goods going in either direction.

Conclusions

Examination of concepts of mobile traders and their evaluation against ethnohistoric and archaeological data in both the American Southwest and Mesoamerica suggest that trade networks as a direct method of interaction between these areas probably did not exist. Instead, the external relations of the Anasazi from any region and at any time period were probably restricted to an area of approximately 700-900 km radii and to infrequent occasions for the longer trips. Factors which played a role in these limitations in the Southwest were (a) the semi-arid to arid environments which affected the production of food, population density and social organization, and (b) the mode of transportation.

Acknowledgments

I would like to thank my colleagues at the Chaco Center, particularly Nancy J. Akins, Catherine M. Cameron, W. James Judge, Stephen Lekson, Peter J. McKenna, John D. Schelberg and Thomas C. Windes, who read the first draft of this manuscript and made several useful comments and suggestions. However, the content, logic, etc. are my responsibility and I take blame for any errors.

Questions and Comments

Al Schroeder: I have two comments and a question. In your field work you shouldn't figure from the Gulf Coast to Zuni or whatever. They weren't manufacturing the finished product there. Most of the shellwork that comes up to the Anasazi are finished products. The Hohokam, we know, show this. The same thing would occur in connection with any of the other things like copper bells coming out of the west coast of Mexico. From the source there were different centers who were moving it from one center to another.

The other comment: Remember there are three types of pochtecas, one of whom is your mobile trader, so don't go pushing the word pochteca out. There were mobile traders such as you are describing here in the Southwest.

Now, the question: I have a recent article somewhat along the lines you have just been talking about. I sent a reprint to Watson Smith and he raised a question that I didn't answer; maybe you can. These mobile traders, did they tell stories like our modern travelling salesmen?

Joan Mathien: Probably, don't we all?

Russ Wagner: In regard to working of the manufactured turquoise products, couldn't this almost of necessity have involved more than one individual? Otherwise, how did the one person learn; he didn't start off and gradually learn from a predecessor.

Mathien: Right, but at this particular site I can't say that so many people lived there, and so and so taught somebody, or there were two brothers. We don't have that information, but you are right.

Wagner: Then we can assume that a minimum of two were involved at least for part of the time?

Mathien: Yes, but at other sites the evidence is so minimal that you would suspect that only one man sat there and made them. Obviously they are learning, but they are going and visiting one another in different sites, and obviously we haven't excavated enough sites in the Chaco to know how all of these integrate. But sure, obviously we're teaching one another.

Question: What kind of burdens were the people carrying that were going these distances that were recorded ethnographically? What I'm wondering is, was he carrying a bag and a couple of tortillas, or what?

Mathien: I don't know; but what I tried to look at in my longer version was some of the other things, like how far did people go for salt. You've got the Hopi going for salt down at the Colorado-Little Colorado junction, and it does say specifically that they carried sixty pounds on their back. This is not an unusual thing. Sixty to a hundred pounds is documented for people walking through the mountains of
Mesoamerica and places like that, so that you could have carried quite a bit. I've seen a man carry a piano in Guatemala City.

**Joe Ben Wheat:** One of the items that has a lot to do with how much people carry and how far they carry it are the records kept by the companies, the factories so-called, in the northern territories—Canada and so on. The standard load of a courier was two 90 pound packs, a hundred and eighty pounds, over the portages. When I was working on the Olson-Chubbock problem, I found that the standard load carried, sometimes considerable distances, was about 200 pounds per woman. It makes the point that we think we are overburdened if we carry 60 pounds; that's a big load. If we use our abilities, our training, to judge what was going on in the past, we're making a big mistake.

**Mathien:** I think so too, and that is one of the reasons why I feel that we have very little evidence of a lot of trade, because there are so few items that would weigh so little, and they come in at different time periods.

**Ted Frisbie:** You can carry one hell of a big load of feathers, but how are you going to find it archaeologically?

**Mathien:** Some of the feathers that we've got are not always imported from long distances either.
A couple of points need to be made about this paper. One is that this isn’t a synthesis like so many of the good papers we have heard thus far. It is a case study in a very peripheral area to the greater Anasazi area; it is an area that is really not very well understood. The other point is that in 1934, Charlie Steen did a very good survey in a small portion of the Wupatki area and I appreciate the work he did. Most of the sites in the area fit well within the Dzil Nez Phase that Dick Ambler mentioned earlier in this symposium.

The first field phase of a five year archeological survey being conducted for Wupatki National Monument has just been completed. This intensive inventory survey is being conducted by the National Park Service, Southwest Cultural Resources Center in Santa Fe, New Mexico. I am the Principal Investigator and the director of the project. This year, Steve Adams, Group Archeologist for the Navajo Lands Group Office in Farmington, New Mexico, was the Assistant Supervisor for the project. Others who worked on the project this summer were: Sandy Rayl, Greg Weldon, Judy Miles, and Judy Schwartz.

There are five primary reasons for conducting this work: 1) the National Park Service must have an inventory of its cultural resources within the park boundary to properly protect and manage those resources and to fulfill various legal mandates, 2) prior to any future proposed development within the park area, cultural resources need to be evaluated for any impacts that might occur as a result of that development, 3) in order to properly evaluate the preservation and stabilization needs that the park has, this additional data is required, 4) to adequately interpret the total resources within the park, information about its previous inhabitants needs to be collected, and 5) it is hoped that several gaps within our knowledge of the region can be better understood and better evaluated through a survey of this nature. This survey was programmed for several years ago, and finally this past year the funding was authorized for the project to begin.

The intensity of the survey is fairly high as the spacing of the surveyors is between 5-10 meters. Each site is located on a 7.5 minute U.S.G.S. Quadrangle map, a blown up version of the quad map, and on 1:6,000 aerial photographs. Each site is recorded on a six page site form, a two page computer form, and a MNA site card. Also a plan map (to scale) is drawn for each site and photographs are taken for each site. A permanent site stake is driven into each site with the field number stamped into it. Isolated artifacts are recorded on a separate section map and are noted separately (no site number is given for isolated occurrences). Collections are taken from some sites, which will be initially analyzed this winter and materials will be selected out for specialized analysis later.
Reporting on the project is done throughout each year with monthly reports, preliminary reports, new research designs for each phase, etc. A final report will be prepared following the fifth year's field phase. Equally, review of the project is handled several ways, primarily through several National Park Service offices involved with the project. However, Dr. James Judge, Dr. Alexander J. Lindsay, Dr. George Gumerman, and Mr. Peter Pilles have kindly agreed to act as reviewers, and to help guide the direction of the project. One of the main reasons that this project is organized in a multi-staged framework is to stay current with other research on-going in the general area and to be flexible to adjust to fit somewhat within other researchers' goals.

This year a total of 268 sites were inventoried and recorded within the 5½ sections which were surveyed. Because of the high density of sites (ca. 49 per 640 A.), we were unable to cover as much ground with one crew as hoped for. To keep the pace and projections for the next phase, two survey crews will be utilized. This initial phase has also been helpful for predicting other project needs and methodology for the following phases. Also, after spending 4½ months at Wupatki National Monument, I have a much better idea of logistics, access into various areas, vegetative cover, terrain, equipment needs, safety factors, and various other aspects than I did before we began Phase I of the survey.

I had developed several research questions which need to be addressed from the data collected during the survey. Most of these questions deal with a) cultural usage of the area, b) settlement patternning, c) relationships to volcanic eruptions from Sunset Crater, 5) agricultural usage, e) water control, f) resource exploitation, and g) park developments which could be confused as prehistoric modifications. To develop answers or interpretations for most of those questions posed, the completed data for the total area will be required; however, some clues are already emerging for some of the questions. Agricultural usage within Wupatki National Monument has only been vaguely discussed within the literature available for the area. The Phase I survey located several agricultural areas which included field markers, alignments, terrace systems, and small field structures (generally 1-2 rooms). These agricultural areas were generally within three distinctive locations: 1) on large open flats (mesa tops or open grassland plains), 2) within small shallow drainages, or 3) on slopes where terracing could control the runoff. Also, water control devices are not well described for the area. Al Schroeder states that "Sinagua farmers in the late 1100's did develop some water and soil conservation devices near Wupatki Pueblo, and also developed wet-weather ponds and water collection features at the bases of ledge overhangs (1977: 55-56)." Within the area surveyed this summer, two fairly extensive dams and water storage areas were located. Also, several smaller natural bedrock catchments were recorded, and one small dam within Citadel Wash was located. A major lithic resource area was located, along with a few building stone quarry areas. Some stabilization rock dumps and highway construction rock dumps were recorded so they would not be confused later as being prehistoric in nature. The remainder of the paper will focus on cultural usage of the area and relationships of the volcanic eruptions in the area of Wupatki.

Most of the prehistoric archeological materials observed from the survey thus far show a mixing of culture—Kayenta Anasazi, Sinagua, Winslow Anasazi, and Cohonina primarily. Isolated projectile points include one Agate Basin point, several Archaic points, and several Basket Maker III points, but none of these are in direct association with any site. Harold S. Colton in discussing cultural frontiers for the Sinagua, describes the area of Wupatki as being open to allow a mixing with Kayenta and Cohonina cultures (1968: 10-15). An over-riding question is what the relationship of the Sinagua and Kayenta Anasazi is and how the two cultures interact? John P. Wilson states, "One persistent problem is how to handle sites along 'frontiers', where various utility pottery wares are found in such proportions that assignment of a site to a group is questionable . . . One might say that different groups were co-residents, or were trading pottery extensively, or one group moved out and another re-occupied the site, and be equally justified in his opinion . . . However, an inability to apply the labels makes 'frontier' situations no less interesting (1969: 476)." It has also been suggested that following the major eruption(s) of Sunset Crater, a prehistoric land rush occurred into the Wupatki area from the north (Colton 1937: 11-12). It would appear that the Sinagua had to undergo a series of changes as a result of this outside influence (Schroeder 1961: 65). Wilson would argue that "throughout their culture history, the Sinagua seem to have exerted very little influence on neighboring peoples (1969: 505)." Evidence for a major migration from the Kayenta area is now somewhat questionable as the Anasazi had been living in the area of the Little Colorado River and Deadman's Wash since Basket Maker III times (Pilles 1979: 472).

I was hoping that I could better address some of the issues stated above through the survey information I was collecting. However, the bulk of the sites I have observed thus far do not exhibit features which can easily differentiate cultural attributes (the architecture generally contains features of both Anasazi and Sinagua, the ceramics basically are mixed with a slight percentage being given to the Anasazi side with no other apparent differences which can be distinguished). I realize that utilization of only surface information limits the kinds of information which can be obtained; however, if anyone has suggestions on how to better approach differentiating sites I would appreciate hearing them.
It is somewhat difficult to archeologically ascertain the effect that eruptions from Sunset Crater had on the prehistoric populations living in the Wupatki Basin, but it probably would not have been greatly different from the effects of the recent Mt. St. Helens eruption on present day populations. Dr. Colton admirably attempted to recreate how it might have been (1937: 9–12). Recent geological work in the area has shown that rather than one or two eruptions between A.D. 1064 and A.D. 1067 (which was previously accepted), the eruptive history of the crater extended over a period of nearly 200 years (Pilles 1979:464). Several previous researchers have argued that the cinder blanket which covered the area following the eruption(s) acted as a moisture retaining mulch for agricultural usage which drew new people into the area. This effect may be somewhat overstated, however, as recent evidence indicates that there was above-average rainfall between A.D. 1050 and A.D. 1130 (Pilles 1979:468). This increased moisture may have produced a much greater positive effect for farming than did the ash cover. The bulk of the sites I have observed thus far appear to date with the Late Pueblo II through the Early Pueblo III periods (ca. A.D. 1070–1200), or roughly within the moist period suggested above. So, rather than a major population increase into the area, as suggested previously, Peter Pilles would argue that the Sinagua responded to this changing environment through changing land-use patterns and redistribution (1979:480). Whether this is totally applicable for Wupatki, remains yet to be seen. In conclusion, it is hoped that the survey data obtained from Wupatki National Monument over the next few years will help to validate some of these current ideas about the region or refute this approach and lead us to more positive approaches to better understand the area. Through the multi-staged research approach, changes can be adapted into the project to better address the Kayenta Anasazi/Sinagua relationships mentioned above to make more substantial contributions to Anasazi and Southwest prehistory. Hopefully, also, as we refine survey methodology we can better address some of these problematic issues. Certainly, there is a wealth of information that has been collected for the area thus far, but there is still much more knowledge which needs to be acquired.

QUESTIONS AND COMMENTS

**Al Schroeder:** One question on the business of the mulch retaining the moisture. Would the addition of the increase in rainfall in that period also bring the forests down much below the level of the other forests around there where there were no cinders? You know the present forest is about a thousand feet lower than the others in the area where there were no cinders.

**Bruce Anderson:** That's right, and this is a problem that Igor had tried to deal with, and I don't think he satisfactorily reached any conclusions on that. I think that is something we still need to investigate.

**Russ Wagner:** How far is this from the Chimney Rock area mentioned yesterday?

**Anderson:** It is a long distance; it's in northern Arizona, about 35 miles north of Flagstaff.

**Dick Ambler:** What does your typical village layout look like? Does it look anything like Kayenta or do they have kiva depressions?

**Anderson:** The large sites do, Dick; they have kiva depressions. They have up to maybe 20 rooms in the larger sites that I'm dealing with. However, the bulk of the sites that I have are field house units, one to two rooms in nature. I have very few large sites within the surveyed area thus far.

**Doug Scott:** This idea of frontier interactions is really interesting. I'm not sure if you would be aware of the literature since its outside anthropology, but I've spent about three weeks in working in historic areas in frontier interactions and modeling frontiers. I've applied some of this to historic military sites and also Stan South and Ken Lewis applied it to some colonial American, British, and Dutch sites in the east to an archeological context and got some interesting results. I can't remember the references just off the top of my head. There is a chance that some of these models, with modification, might be successfully applied to a prehistoric context.

**Question:** What about pit houses: What is the possibility for a lot of those small sites having pit house structures with them, since many in the work we've done in the Shonto Plateau of a later phase, 1200-1250, have a lot of pit house structures showing up.

**Anderson:** That is a real problem for us in the Wupatki area in that we're only utilizing survey information, and to really locate pit houses I would have to do some testing around some of the sites.

**Comment:** Many of the pit houses have associated with them small surface structures of masonry.

**Anderson:** Exactly. Now, there are some sites in the Wupatki area that are like this and Lyn Hargrave excavated some in the 1930's. Some of the sites that we have found do appear to be pit house structures, but I'm not certain; we are going to have to do some testing on some of these things later on. A lot of the sites that we have looked at have absolutely no depressions around them whatsoever.
Curt Schaafsma: Your time frame is interesting; in other words you spent four and a half months there covering five square miles approximately.

Anderson: A little over; about five and one-half.

Schaafsma: In 1975 I reviewed the literature and tried to come up with a generalized estimate of survey rates, time needed to cover geographic space with certain spacing and so on, and I hearkened back to Hayes' 1959 survey where it came out something on the order of thirty man days per section to do the whole Wetherill Mesa. Now your thing is very interesting because it is a repeat in intensity of Hayes' survey and similar to what we got at Chaco. But it's another intensive survey of a piece of geography, and so you are presently working in something that looks like about the same kind of time frame that Hayes used. And it would be interesting for you to keep your time, man days needed, to cover geographic space out front where we can all watch it, because people who are dealing with CRM type projects are constantly being hassled to spend maybe ten man days on a section and yet act as if they were doing just as good a job as you are doing.

Anderson: That is a point I was trying to make there, Curt; I appreciate your comments. I want, as I go along with this, to sort of stay in tune and compare with the Chacoan Survey and the work that Al did here at Mesa Verde. I sort of want to get a feel if I'm approaching it along the same aspects as they are.

Jim Judge: I can't help but add one comment to that of Curt's, and that is that I did write an article recently which is the tail end of the survey volume by Hayes, Brugge and Judge, and it happens to deal with that man day problem. My conclusion, incidentally, was that the number of sites found varies directly with the number of man days in the field. It is a very simple conclusion, but I try to support it.
ANASAZI-MESOAMERICAN RELATIONSHIPS:
FROM THE BOWELS OF THE EARTH AND BEYOND

Theodore R. Frisbie

Author's note: A shorter version of this paper was presented at the Anasazi Symposium.

There are few who would deny Mesoamerica influenced the development of Anasazi culture; however, the precise degree and specific mechanisms by which this occurred continue to be open to much conjecture and debate. Although it would be advantageous for a full-fledged Mesoamericanist to venture northward for comparative analysis with the Anasazi continuum, this is highly unlikely. Rather, a few dedicated Southwesternists tend to venture deeply into the south for bits of data which seem congruent with their inquiries. The concepts of the Greater Southwest and Gran Chichimeca are, of course, not new; nevertheless, the great majority of Southwesternists have not felt the need to pursue the obviously implied relationship. In part, this appears to derive from a preference for regionalism which may take the extreme form—downright isolationism. Herein it becomes easy to show disinterest, disbelief or disgust when someone suggests Mesoamerica was significant in shaping that which we call Anasazi culture.

Rather than enumerate traits which indicate a relationship of some sort, in this paper I will provide a synthesis which focuses upon a rational and acceptable means whereby such features became deeply imbedded within the patterned behavior of the recipient culture. The analysis emphasizes religion, although not exclusively, because this was clearly a culture focus in both Mesoamerica and among the Anasazi.

What is of particular interest in the Anasazi example is that, through time, significant ceremonial additions appear to have been integrated which reflect varying degrees of direct and indirect interaction with Mesoamerica. Care must be taken when utilizing the (ethnographic) present for analogues since this represents the end product of a complex process passed by word of mouth from one generation to the next. Similarly, the complexity of religious expression within Mesoamerica is enormous and variation between and among the various cultures represented is often difficult to document. By the time of the Spanish Conquest under Cortes, the Aztec pantheon reflected an amalgamation of numerous features of conquered groups as well as their own ideology. Not infrequently, thus, a deity manifested several quite distinct guises.

In examining the data, it is hoped that the nature of transmission from the donor to the recipient culture can be elucidated with more clarity. The precise mechanisms involved with this process may also begin to narrow and delimit the possibilities as they existed in the past.

From my perspective, one of the most fortunate circumstances occurred after I agreed to tackle the present topic for the Anasazi Symposium. This was the publication of Randy McGuire’s, “The Mesoamerican Connection in the Southwest” in The Kiva (1980).
The article is an excellent overview, one which should be read by anyone interested in southwestern anthropology. McGuire (1980: 33) acknowledges critical comment and/or discussion of the manuscript by DiPeso, Kelley, Ferdon, Haury, as well as a number of others; he does not, however, state the precise nature of these comments and discussions in the majority of cases. Further, he does not indicate whether or not reactions to the manuscript (or discussions thereof) affected the final, published version of the paper. Finally, the usual statement wherein an author assumes responsibility for the interpretations he or she has made of such comments is noticeably absent. My immediate reaction to the publication is that, while it presents us with an alternative interpretation showing considerable scholarship, this interpretation intentionally or inadvertently fails to consider a number of important data in order to strengthen the author’s point of view. In essence, the paper begs for critical comment.

The main thrust of McGuire’s position is to offer a compromise between the isolationists who feel the Southwest developed in situ without Mesoamerican influence (or at best, a minimal amount) and those who feel that Mesoamerica dominated and determined Southwestern culture history. His compromise involves contact between the two regions through trade; the latter varied in intensity depending upon events in the Mexican hearth area, but includes contact with cultural activities in the present Mexican states of Zacatecas, Durango, Sinaloa, Nayarit, and Jalisco. Unquestionably the two routes of particular significance to this inquiry are: 1) via the Sonoran coast, inland to Arizona (through Hohokam territory) and onward to the Zuni region or other Puebloan points, and 2) via the Sierra Madre flanks, through the Chalchihuites culture province to the Casas Grandes area in Chihuahua and northward into New Mexico to account for contact with Mogollon and Anasazi peoples. As is well known, both of these routes are natural corridors which provide relatively easy access with continued importance throughout the historic as well as the modern period.

For the Anasazi (and other Southwestern groups) the above offers nothing new, including the varying degrees of interaction depending on cultural events involved with the opening and closing of these routes at specific times. McGuire’s position centers on laying to rest (once and for all?) the pochteca (puchteca) theorists with particular references to the Chacoan Phenomenon—a topic to which I shall return shortly. From my perspective McGuire’s paper is “interesting”—an attempt to bind old wounds which have festered for years and will, in all likelihood, continue to do so. Further, he has offered us no more than a logically constructed argument taking a classic “middle-of-the-road” approach which someone was bound to provide in response to the general increase in what might best be termed “position” papers. My immediate and continuing reaction to McGuire is that he does not treat all of the data fairly—as a matter of fact he skews and neglects to cite information which is highly germane if one is to act as the catalyst (shaman?) in the curative rite of healing old wounds. His is definitely not the “tie that binds” from my perception.

Clearly, McGuire has fallen into the trap of “kill” and “overkill” with respect to the question of Mesoamerican influence in the Southwest. In my opinion, this is the crux of the entire issue. Unquestionably there are two factions: 1) Progressives, who represent a flamboyant, “crank it out and let’s fight” groups of rabble rousers (DiPeso, Schroeder, Kelley, and a number of his students for the recent period), and 2) Conservatives, or the “In situist pushers” (i.e. isolationists). Herein reside the ecologically oriented, frequently computerized or otherwise highly structured hypothesizer/theorist types who prefer to ply their trade at the local level. Perhaps the Chaco Project proponents are the most noteworthy example, although the previous paper and discussion thereof at this symposium (Lekson, this volume) indicate that a ray of hope has appeared on the horizon (see also Lister 1978 and Hayes 1981). Without question there is no quebbling about the exceptional quality of work which has resulted to date; however, as of this date the Chaco Project people have yet to face—fully and thoroughly—the Mesoamerican connection.

In this same vein, I find it astounding that a volume the magnitude of The Handbook of North American Indians (Volume 9: Southwest), as well as the forthcoming Volume 10: Southwest—non-Pueblo does little more than pay lip service to Southwestern-Mesoamerican connections (see Ortiz 1979: 690). Would it not have been appropriate for a specific paper to address the problem? Was this simply an oversight on the part of the editor and the planning committee? I do, however, admit that the brief mentions of the topic that do appear provide data for undeniable connections which, I am pleased to state, even appear as incontrovertible evidence—so far as these data go. For example, Woodbury and Zubrow (1979: 46) provide the following:

*Domestic plants, as the primary Mexican contribution to the Southwest, did the most to transform the life of its inhabitants. The introduction of pottery making from Mexico was less basic but of archaeological importance because of the abundance of imperishable evidence it supplies.*
Additionally, it should be noted that DiPeso (1979) contributed, “Prehistory: Southern Periphery,” wherein he offers a brief but comprehensive “tour de force” for the area of greatest importance to our understanding of Mesoamerican-Southwestern interaction; however, within the confines of his topic—south of the border—the obvious charge for the volume, one should not expect more than casual mention of “up north.” Nonetheless, the stage is set for such an undertaking. Any reader should be made aware of the availability of a considerably greater bibliography than the two sources cited to update the 1971 paper to the year 1978.4

That “kill” and “overkill” are reflected in the literature may not sound particularly earth shattering, but in considering this possibility one can almost immediately demarcate his or her gut reaction. In so doing, all that which has gone before from each individual’s perception comes into play. In part, this also reflects one’s training within the discipline. Those not exposed to Southwestern-Mesoamerican interactionism beyond casual mention are not apt to consider it important—the basics are simply taken for granted. Does one really need more? In contrast, those exposed to the excitement of fertile new ground and concepts which appear to fit in logical sequence have little, if any, difficulty building on this base. Though such may be proven correct in the final analysis, it is all too easy for the academic/research community to block an idea wherein a flaw exists.

I recall, for example, presenting a paper at the Society for American Archaeology Annual Meeting in 1972 entitled, “The Chacoan Interaction Sphere: A Verification of the Pochteca Concept within the Southwestern United States.” It attempted to demonstrate that an outside source was responsible for developing a system more complex than one encounters within the pueblos either before or after Chacoan times. The source was stated to be derived from the Mesoamerican pochteca. While several well respected archaeologists commented favorably following the presentation, it was not long before I became acutely aware that the offering was somewhat naive. Based on my training, however, the entire idea originally seemed highly plausible; indeed, the title signified “verification” and also included “pochteca.” First of all, the full extent of the Chacoan satellite system was far less well known than it is today (Marshall et al 1979; Judge 1979); and second, one cannot verify the term “pochteca” with certainty for any cultural entity other than the Aztec of protohistoric and historic times. It would have been much preferred to substitute “Pochteca-like” or a more inclusive term such as “trocadores” for long distance merchants or traders. This is not to say there may not be some merit in the idea; however, the handling of the data left much to be desired. Philoso- philically one may argue ad infinitum and never conclusively prove a point to the satisfaction of someone versed in logic!

In retrospect, I was guilty of “overkill.” Recognizing that fact has been instructive because if we are to proceed toward a meaningful interpretation of what really transpired between Mesoamerica and the Southwest, all arguments must be couched in terms which are acceptable to both progressive and conservative factions. Bridging gaps of understanding requires a considerable amount of openness to varying points of view, as well as logico-deductive constructs which are accurate portrayals of past events. In other words, care must be taken in phrasing as well as in presentation of the data themselves, and semantic differences should not be an excuse to obscure the facts.

As previously indicated (Frisbie, this volume), the Anasazi and Mesoamerica shared a basic hunting and gathering (i.e. foraging) adaptive strategy for a period of time extending over several thousand years. This we have termed Desert Culture or Archaic. Although there was unquestionably regional variation based, in part, on ecological differences, the essentials involved with everyday activities in all probability bore great resemblance throughout the entire area. Further, a number of distinguishing characteristics for this level of socio-cultural integration among extant cultures provide insights for the interpretation of the archaeological record. We can assume with some degree of assuredness, for example, that social organization involved bands averaging between 25 and 30 individuals. Patrilocality would probably have been favored with kin reckoned bilaterally. A band head- man, selected for leadership ability, controlled activity of the group. Seasonality with respect to floral and faunal resources dictated movements. Periodic grouping of bands might also be expected when bountiful resource acquisition provided the means to support it. Increased social interaction at such times in all probability included nuptials, dancing, trade, and the sharing of life’s experiences. Shamanism with both curative and religious practicing kept body and soul placated.

Based on the archaeological record we can state with certainty that shamanistic practitioners assembled medicine bundles with a wide array of items contained therein—chipped stone objects, crystals of various minerals, oddly shaped and/or colored stones, some reminiscent of living forms (fetishes), etc. Sucking tubes to affect cures and deer hoof rattles are also part of the inventory. Prayer feathers were deposited in caves as offerings, a continuing practice among the Pueblo and some other groups.

It is of interest that in the latter instance one might expect considerable complexity within mythology, although we have nothing other than comparative data on which to base this assumption. Religion, of course, is one of the few cultural universals and has been extant at least since Neanderthal times (ca. 100,000 years ago) and perhaps earlier. Living hunting-gathering cultures often manifest highly complex mythologies based on totemism, animism, and...
the like. The Australian aborigines provide a classic, though not exclusive, example of this complexity. On a more local level, the Numic (Shoshonean) speakers of the Great Basin as recorded by John Wesley Powell between 1868 and 1880 (Fowler and Fowler 1971: 215–229) provided aspects of their mythology. Included are such features as the Twin Brothers (Shin-au-av), tales involving numerous animals, sun, moon, Venus, and a variety of stars and constellations (Pleiades). While the editors suggest some features have probable Puebloan origins, is it not possible that such features represent original Desert Culture ideology/mythology? It does not seem unlikely that horticulturists built upon their pre-existing widespread Desert Culture foundation with the addition of complexity when and where they saw fit.

Of particular interest to our inquiry pertaining to religious parallels between Mesoamerica and the Anasazi is the possibility that the shared core emanating from the Desert Culture base could have encompassed considerable complexity, including the High God concept. Such is not restricted to civilization mentality, as is commonly thought; indeed, even introductory anthropology text authors not infrequently equate the concept with hunting and gathering. Such is not restricted to civilization mentality, as is commonly thought; indeed, even introductory anthropology text authors not infrequently equate the concept with hunting and gathering. The implications of this phenomenon are far reaching. I would like to suggest that a number of Middle Formative/Pre-Classic Mexican deities had been extant long before their representation appeared in lasting form (i.e. sculptured stone). Among them must be included the High God of the “mother culture”—namely, the Olmec, ca. 800 B.C.

This supreme being represented a formalization incorporating numerous concepts from an extensive background. It was sexless (at least no sex indicated), and featured a cave-like mouth and polymorphic dragon-like morphology. Associations were with earth, fertility, clouds, rain, water, fire, kingship, and maize. The latter aspects of kingship and maize, in its fully developed form, are the only attributes one can truly associate with the rise of civilization, and therefore are probably more recent additions. The supreme deity and several other gods of less dominant nature appear in other cultural expressions in Mesoamerica through Classic Teotihuacan times and in part thereafter. Interestingly, many Puebloan groups also have a sexless “top of the line” deity in the guise of the “God of the Sky” (Hopi: Sotuqnaq-u/Colton 1959: 78) and the “Universal Deity” (Zuni: Awonawilona/Kroeber 1916: 273; Dutton 1963: 40–41), as well as others elsewhere. Such omnipotent deities are rarely, if ever, impersonated or graphically represented. Lesser ones may be with masks (once these are adopted) or the addition of face paint and the appropriate headdress. I would suggest that such deities have existed during early times they would not have been impersonated or depicted until the support of a priesthood was possible. At this time elaboration within the system could not only support, but offer a need for portrayal involving priest, temple and god.

Among other supernaturals which might have appeared during Desert Culture times are the obvious ones associated with sun, moon, and earth. Such things as mountains, caves, springs, and fire probably figured ritually, too, as did a variety of animals—particularly the mountain lion/jaguar (Furst 1968), and possibly color-directional symbolism (Riley 1963). It is not my purpose to attempt to prove or disprove that any of the suggestions noted above are, in fact, representative of Desert Culture/Archaic. Rather, I have offered them to indicate a series of features which are within the realm of possibility for shared mythology/ceremonialism prior to the time when Mesoamerica should be given credit as the sole originator of such manifestations. Clearly, however, the introduction of agriculture and the concomitant ceremonialism associated with it brought significant changes in the religious system and its complexity. Thus, for the Anasazi it is at this point that we may begin to scrutinize the archaeological record for influences emanating from Mesoamerica.

Although primitive maize (pod/pop) and squash appear within the Southwest at Bat Cave ca. 3500 B.C., these appear to have been supplemental to foraging activities (see Woodbury and Zubrow 1979: 46–51 for a recent discussion of cultigens). Since it cannot be demonstrated that their introduction played a significant role to alter the lifestyle, one might postulate minimal ceremonial alteration until more advanced maize was introduced. The Anasazi Basketmaker II peoples began to rely more heavily on the more advanced Chapalote-Reventador (12–16 rowed) maize which led to a more sedentary way of life. Recent dating for the period provides ca. 100 B.C.–400 A.D. for the western region (Plog 1979: 113) and from ca. A.D. 1–500 for the eastern region (Cordell 1979: 134). With the exception of Los Pinos Brown, with distribution restricted to the Navajo Reservoir area, no ceramics are known. Some villages feature a structure of larger size which Eddy (1966: 477) suggests served as ceremonial structures or kivas, yet these lack specialized features indicative of such a function.

During the subsequent period, Basketmaker III, the “Three Sisters” are united: corn, beans and squash. It is at this point that significant changes may be noted in the material culture as well as more highly developed religious practices. Painted ceramics as well as plainwares are commonplace—including exotic shapes, such as the classic form of stirrup spouted vessel from the La Plata/Durango area (Morris 1939: Pl.192a; Gladwin 1957:70). Pithouses regularly feature sipapus, and in some cases villages have a larger segregated structure with sipapu and other features of ceremonial nature (Frisbie 1967). During this period the initial occurrence of great kivas may be noted at a
number of Anasazi sites (Kelley and Kelley 1975: 191-199).

In discussing the ceremonial aspects of this important period, the composite of traits point strongly to Mexico. The *sipapu*, or symbolic ceremonial opening to the underworld, signifies a cave (or a body of water). Kelley (1966) made a connection with the Mesoamerican plaza, and more recently Kelley and Kelley (1978) have indicated an identical interpretation for a subterranean burial crypt in the Pyramid of the Sun at Teotihuacan (Heyden 1975). Herein lies the "clincher" to the entire question. Caves, of course, figure importantly in ceremonialism throughout much of the world; the specifics, particularly in connection with *sipapu* as symbolic entrances, are more regionally oriented.

Other areas of interest include the introduction of highly productive Maiz de Ocho and its variant Pima-Papago in at least a portion of the Anasazi region (Galinat, Rinehart and Frisbie 1971) which, when taken in conjunction with the above as well as with the introduction of Kokopelli, the humped back flute player (Schaafsma 1980: 136-141; see also Miller 1975), provide close Mesoamerican parallels. In Kokopelli we find the first definite Anasazi deity depicted in rock art. As suggested previously, there were in all probability others as well; I would suggest that Clay Old Woman (Benedict 1931: 12) might appear at this time since it was she who taught the people to make pottery in baskets. I would further suggest that the Old Fire God, intricately involved with the firing of ceramics, made his appearance at this time as well. Ellis (1977: 9) concurs, although it cannot yet be conclusively demonstrated. Her evidence consists of the equal arm cross design which may not yet be conclusively demonstrated. Her evidence consists of the equal arm cross design which may relate to those which first appear in Mesoamerica to decorate both priests and incense burners pertaining to the cult of the Fire God. Such designs are featured on the ceramics of the Chalchihuites culture in north-central Mexico as well as in Southwestern cultures by A.D. 500-600 (Ellis 1977: 4). Similarly, petroglyphs feature the design during the time frame. Ellis and Hammack (1968: 40) suggest an even earlier concern with the Old Fire God for the Cochise and early Mogollon since charred fire pokers have been found with ceremonial offerings in caves. They correctly state that such offerings are "... not yet generally recognized as of religious context" (Ibid). Within Pueblo ethnographic literature the Old Fire God is considered an elderly tutelary supernatural—the elderly kiva fire keeper during Shalako at Zuni may be his representation since his "son" appears in the form of the Little Fire God, Shulawiti, who is a member of the Council of the Gods. Associations of fire pokers and ashes with fire are, of course, obvious. In some pueblos Ash Boys, Fire Boys and Poker Boys are figures of folk tales rather than myth; however, Parsons (1939: 963) notes Poker Boy figures in the Emergence myth of at least Mishongnovi.

The Fire Society within Pueblo culture is of extreme importance; it is also the oldest, or one of the oldest, according to collaborators (Ellis 1977: 16). It undoubtedly predates, as do other curing/medicine societies, introduction of the katchina cult at ca. A.D. 1100. At precisely what point in time one can expect shamanistic practitioners to have begun specialization long before specific curing societies were created. Sedentism, sui generis, provides fixed location for essential needs between villages long before population growth, coupled with lineage/clan amalgamation into larger communities, came into being during Pueblo III or Classic Pueblo times. Thus, the Basketmaker III horizon should not be considered totally inappropriate for such activities incorporating Mesoamerican concepts.

The Maize Deity who appears among the Pueblos as either male or female, but nevertheless thought of as "Corn Mother," is another likely candidate from Mesoamerica (i.e. Chicomecoatl) during the Basketmaker III period. This association is made on the basis of the introduction of advanced varieties of maize noted previously. Its introduction almost certainly was associated with more complex ritual activity than that which preceded it. Again, however, one need not expect a trilogy of maize, ritual and priesthood to account for complex maize ceremonialism. Such could have been more simply handled by a shaman who, nonetheless, preserved and perpetuated a rich mythological heritage within the religious realm.

I do not believe it is of simple passing interest that Basketmaker III peoples became involved with blue and green stones which had already been long associated with Mesoamerican cultures. For the Classic and Post Classic periods, Chalchiuhtlicue, Goddess of Hard Substances or "She of the Green Skirts" (with additional association with water) continued the earlier mythical tradition. For the Pueblos, I would suggest the possibility that Turquoise Woman (Sun’s Wife) became part of the pantheon on this horizon. It is of considerable interest that the Hopi, too, characterize her as the Goddess of Hard Substances, and associate her with wealth in general as well as with turquoise (Colton 1959: 78). Elsewhere she appears variously as Turquoise Woman or in more composite form as White Shell Woman. She is synonymous with Changing Woman among the Navajo. One may even find, as at Zuni, Turquoise Man—here associated dualistically with Salt Woman, both of whom, after being defiled and wasted, removed themselves to new locations (Benedict 1935: 44). The process of transformation involving a series of guises reminds one of that which frequently
occurred among the Aztec pantheon, as well as in earlier Mesoamerican cultures.

Within Developmental Pueblo (Pueblo I and II) there is general continuation of that which occurred previously, as well as a series of additions to account for culture change, some of which emanated from Mesoamerica. Perhaps the change of greatest significance may be equated with the development in Pueblo II of the small kiva in its true sense, namely, a religious structure which was no longer a family domicile as well as a ceremonial edifice (i.e., "proto-kiva"). I would suggest it is at this point that the medicine or curing societies assume their full formalized patterning and position within Pueblo culture.

Great kivas continue, and McGuire (1980: 16-17) is probably correct in arguing that there are no gaps from A.D. 700-850 and A.D. 900-1050 as asserted by the Kelleys (1975). The arguments concerning origins and use of Anasazi great kivas remain obscure; however, all share sufficient size to accommodate a large number of individuals, generally far more people than occupied a given site, at least to Chacoan times. Therefore, it would seem logical to assume such structures represented intervillage gatherings. Such gatherings undoubtedly entailed ceremonialism as well as trading activity, exchange of news, and other social activities. Such agglomerations of groups are deeply rooted in antiquity, including those practicing a Desert Culture way of life well into the historic period. One might further expect for the Anasazi that with the progression of time more formalization occurred. This crystallized into the construction of great kivas during Basketmaker III to accommodate participants/visitors who engaged in a variety of activities at probably fixed intervals based on a ceremonial cycle. The progenitor could well have been found among more southerly neighbors, the Mogollon. They, in turn, could have been inspired by structures found still further south as suggested by Kelley (1971:770, 771, fig. 1a) at Totoate and elsewhere. Schroeder (1981) suggests prototypes occur within the Hohokam.

In considering the varied structural features of great kivas one must not lose sight of local preferences based on specific needs and practices. McGuire's discussion of trade (for the early period up to Chacoan times) may well be appropriate; however, one must include a host of ideological borrowings which moved up the trade routes. While confusion exists within the literature relating to the pochteca-like involvement among the Anasazi, I do not believe McGuire's implied emphasis on direct relationships to central Mexico reflects an accurate portrayal of the theorists. From my understanding and awareness, Mesoamerican high culture pushed northward during periods of expansion and became more and more attenuated with distance. Certainly what is found even in Jalisco, Durango and Zacatecas is not of the calibre found in the hearth. It, too, is attenuated; thus, while impetus may ultimately derive from the central hearth, individuals were not trotting back and forth over the terrain making journeys in excess of 1,000 miles (see Kelley and Kelley 1975: 185-187). This was accomplished in stages with only the most highly valued items moving southward; possibly some of these, such as turquoise, reached the hearth. Until such time that source analyses for turquoise are documented we can continue to twiddle our thumbs, for herein lies the answer to our inquiry.

Meanwhile we can continue to explore a variety of approaches, including such matters as the introduction of basket-handle bowls by Pueblo I and continuing thereafter as another Mesoamerican form involved with Quetzalcoatl (Reyman 1981). This was first suggested by Kelley (1968) and described, in part, by Kelley and Kelley (1971: 115-123, 137-143), and Kelley (1974). Most recently DiPeso (1981) has provided an addendum to Reyman's paper, again asserting the Mesoamerican connection.

Although the Chacoan Phenomenon is considered with Classic Pueblo (Pueblo III), it burst forth during Pueblo II in a relatively short period of time, beginning ca. A.D. 970 and was essentially concluded at ca. A.D. 1120, although this date may be extended somewhat, based on the most recent evidence. With specific reference to the Anasazi and Mesoamerica, it is within the Chacoan Phenomenon that Kelley and Kelley (1975: 207) suggest that first Toltec, followed by Mixteca-Pueblo cults, plied their trade through a series of trade centers, ultimately reaching their most northward expansion at Chaco Canyon. Time and space do not permit a fully documented rebuttal to McGuire's (1980: 26) statement, "... Chaco Canyon must be rejected outright as a pochteca outpost and as a development resulting from Mesoamerican intervention in the Southwest." Instead, I propose to offer a series of data which help to realign the problem and, hopefully, place it within the realm of possibility. Such an approach involves correcting errors in McGuire's logic as well as presenting data with which he was apparently unfamiliar.

Among the most significant errors in McGuire's argument is that relating to Lister's (1978) trait list which he suggested diffused from Mesoamerica to Chaco Canyon ca. A.D. 1000. Thirty items are included, only eleven of which McGuire (1980: 8) cites as Chacoan "firsts." The remaining 19, with an exception or two, derive from Mesoamerica, but appear earlier elsewhere in the Southwest. I believe the point Lister attempted to make was simply that these traits appear with the florescence of Chaco, actually ca. A.D. 1030 rather than A.D. 1000 as noted by Lister. *The obvious importance of the traits is that they appear as a constellation at Chaco Canyon. A single source, namely Mesoamerica, provides their place of origin, with modifications for some. The complexity, particularly in architectural features, cannot help but give pause. Although McGuire (1980: 7-8) states that pochteca theorists ignore Vivian's (1959: 82-84) rebut-
tal of Ferdon’s data, I do not believe they do so because they are unfamiliar with it. It simply has little bearing on the true issue. For example, one cannot equate square column pilasters used in Mesa Verde kivas with those at Chetro Ketl and BC-51 wherein we are confronted by colonnaded galleries. Mention is made of a venerated wall with heavy central core at the Three-C Site within Chaco Canyon. Can one equate this, even if truly earlier, with the rubble core masonry of the major Chacoan ruins? The question of the Talus Unit and its platform mound is, at best, a messy situation, one which McGuire (1980: 8-9) correctly suggests should be left open. In general, should one care to, holes can be poked in the majority of the rebuttals provided by McGuire and others; however, I would admit that we have too little data to evaluate properly some traits at the present time. In this category, I would definitely place tri-walled structures. Because they are of unquestioned ceremonial use, all containing kivas, could they all conceptually relate to the Formative and Classic period West Mexican circular ceremonial structure complexes described by Weigand (1976)?

Among other traits not treated by McGuire for Chacoan sites of Mesoamerican origin are extended inhumations which begin ca. A.D. 1000 (Stanislawski 1963), and the Pueblo Bonito high status individuals interred in a crypt-like tomb with sipapu and evidence of staging in Room 33 (Frisbie 1978: 213-214). 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 tying the sandal to the figure to be precisely like that shown in Mexican Codices for pochteca sandal ties. Such attachment is highly distinctive.

While other traits with ceremonial significance could be examined, I shall wait momentarily to treat these. At this point it would seem more appropriate to provide comment on two related aspects: roads and trade. McGuire (1980: 19) notes the extensive road system with Chaco Canyon at its center. This affords an interpretation of central place theory par excellence. Chaco Canyon was the central place, administering to a vast network of satellite sites fanning out around it and providing maximum utilization of a variety of ecozones and essential materials to sustain the system. Because there is not a well defined (or even ill-defined) road leading straight-away to Mesoamerica in no way precludes the possibility that a well established route did not exist. One has simply to remember that the Spanish Entradas had little difficulty wending their way northward from Mexico following such routes. Certainly, the connection between Chaco and the Village of the Great Kivas (as well as other Chacoan ruins in the Zuni area) places the Chacoan peoples at the terminus of a very well established route, certainly not described as a “Chacoan road,” but definitely of consequence. It is interesting to note that from Zuni southward to Salt Lake, Zuni mythology indicates a straight wide “road” which was cleared during the sojourn of Old Salt Woman from her original location at Blackrock to the new location. A Zuni collaborator stated that in his youth during salt gathering pilgrimages old men told of a clearly marked remnant descending the steep mesa north of the present lake (Frisbie 1979). Was this, in actuality, the remains of a Chacoan Road for the exploitation of salt? Interestingly, too, Bandelier (1892: 3-4) states that salt was one of the items the Pueblos traded for Mexican goods (particularly macaw/parrot feathers). Is it possible a trade route existed southwestward from Salt Lake? This, I have been told, was the direction from which Western Apachean peoples frequently approached; it could, therefore, have provided a “shortcut” back to the main route.

To the east, LA 835, near Santa Fe, has been suggested as a Chacoan outlier (including great kiva) by Stubbs (1954). If this is so, the site appears to be the closest to the extensive turquoise mines at Cerrillos. I have elsewhere (Frisbie 1980) suggested that habitation sites in close proximity to abodes of deities were not permissible because of the sacredness of the locations as well as the potential danger from the supernaturals. The point to be made here is that as the known eastern extension of the Chacoan Interaction Sphere, its inhabitants were directly associated with the other major route to the south, via the Rio Grande valley. Although this route is 200 km. east of the Chacoan center, as noted by McGuire (1980: 18), there is an obvious answer to his question about why a Mesoamerican outpost was not established in the Rio Grande. Previous contacts of long standing had been established within the region. The Chaco area was essentially a central point with respect to both population and resources necessary to develop a far flung network as clearly demonstrated by the satellite sites, and certainly many more than the approximate 120 known sites can be expected to exist. Thus, although containing a highly valuable resource, turquoise, the Rio Grande was peripheral to the overall endeavor.

While I am in agreement with Kelley and Kelley (1975) concerning Chaco Canyon as a pochteca-like outpost, I must agree that their rendering of crammed storerooms appears as somewhat of an elaboration. However, without question the majority of these goods, as indicated by McGuire (1980: 17-18), were intended for use within the Chacoan interactional sphere rather than for export. Such storage, however, is indicative of trading activity of high order not found elsewhere with the Anasazi area to date. The asserted lack of turquoise, other than in burial and ritual contexts and occasionally elsewhere, does not preclude its occurrence during occupation. As noted, such would have been removed by pochteca-like inhabitants and others who possessed it. Workshops,
and particularly debris, can be expected to leave exceedingly little direct evidence; most of the associated tools could function in other capacities, and discarding "scrap" as waste is in all probability a totally erroneous assumption. At Zuni, for example, every bit of scrap is saved for ritual use; when it is sprinkled as an integral part of sacred meal and falls either to floor or ground it is deposited ritually, not as trash. One does not knowingly waste the flesh of Turquoise Woman (or Turquoise Man). Thus, even scrap would be carefully reserved; it, too, had worth. I would suggest a true test would be in seeking floor sweepings containing fine turquoise dust, as well as checking any stone which might have functioned in an abrading capacity for embedded remains. For the above reasons I cannot accept McGuire's (1980: 18-19) statement: "They (archaeologists)—report no evidence of workshop areas or storage of large amounts of unworked turquoise or finished items. The widespread distribution of turquoise in Pueblo Bonito and the lack of evidence for specialized manufacturing suggests that Chaco Canyon was not an exporter but an importer of turquoise."

Other areas of interest which require definite comment relate to the supposed small number of Mesoamerican derived objects—the classic ones (i.e., copper bells, macaws, their feathers, and other exotic items). Those which have been found occur in restricted areas as I have noted elsewhere (Frisbie 1978: 212-213) and were obviously associated with an elite class in religio-political control of the Chacoan Phenomenon. Since I am unaware of any indications relating to warfare or destruction at the time of abandonment, it is clear that items of worth would have been removed; thus, precisely how much exotic material was removed has some bearing on interpretation. 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 the native peoples (see Frisbie, this volume, for further discussion).

If we can assume that the great kiva, and the variety of activities one might expect to have been associated with it, is at least in part analogous to the example above, is the pochteca-like theory not feasible as well? McGuire (1980: 17) has correctly noted that Spanish missions are characterized by easily recognized features throughout the Southwest while great kivas for both Anasazi and Mogollon vary considerably. As I noted previously, the great kivas could and undoubtedly did function without pochteca-like group presence. They served a function within their respective cultures, but one which was almost certainly related. Variation in ceremonial use led to variation in feature elaboration (or the lack of it). Such structures, however, could have easily become an integral part of pochteca-like entradas while not obstructing individual structural variation. Imposing a sine qua non aspect to expectations for architectural replication may reflect a bias far beyond that which is required. Thus, while great kivas commonly occur at Chacoan sites, bearing some variation in structural detail, this may not reflect a concern of those having ultimate religio-political control. This was simply incorporated and obviously embellished upon with the passage of time as evidenced by architectural alterations in some excavated examples. In other words, great kivas may have continued to serve local needs while at the same time serving regional ones.

While I do not disagree with Judge (1979) in regard to the overall development and operation of a complex cultural ecosystem within the Chaco Basin, I do not view it as viable without intervention of an outside group. Herein lies the introduction of a (northern) Mesoamerican derived exploitation group. Their northward push involved capitalization upon pre-existing contact and brought with it a constellation of traits and complexes which revolutionized the lives of the local population. Religio-political organization can with relative ease incorporate ranked societies or a chiefdom level of integration (Frisbie 1980). I do not believe it would have been possible to accomplish the obvious complexity without such organization and control. Since individuals with Mesoamerican derived background had good reason to order such activity, they did precisely this. The exploitation of natural and culturally created resources was their reward. There was essentially nothing to stop their endeavors so long as the system operated efficiently. Both status and non-status holding groups benefitted. Their accomplishments are clearly manifest in the archaeological record.

Among the almost certain benefits reaped by the local population were a more certain food supply and an association with a higher level of social integration which undoubtedly bore considerable cultural enrichment. Within this realm were embellishments to the previously extant religious order. Such a system could, and undoubtedly did, support a priesthood centered around a newly established cult or cults. I have previously alluded to combined religio-political integration and control; as Kelley (1981:436) has pointed out, this follows the Mesoamerican pattern (as elsewhere). Expansion of those in control (i.e., the elite segment of the population) appears to have been fairly rapid if one may consider the rapidity with which various areas were incorporated into the system through establishment of satellite sites. While it is impossible to state with certainty, I would suggest an initial small group in-migration from northern Mexico.
which soon led to inter-marriage with the local populace, perhaps involving lineages or clans which had already assumed greater social standing within local communities.

Another aspect involving closer integration of the population into larger communities relates to the association of lineage or clan groups which previously occupied “unit-type” structures, most frequently manifesting a single small kiva. We should now expect an emphasis on establishing distinct functions for each kiva-affiliated group. This would afford an opportunity to draw membership into curing societies by crosscutting the entire resident population, a practice which continues within Pueblo society at present.

Community-wide religious participation would involve plaza and/or great kiva activities, undoubtedly associated with maize ceremonialism. I suggest that overall control rested in the hands of the high ranking elite group.

There can be little, if any, question about which cult was of paramount importance to the group which established themselves within Chaco Canyon. Although hesitancy is shown on the part of McGuire (1980: 24–25), the timing of the Toltec derived Quetzalcoatl cult fits precisely with the Chacoan Interaction Sphere, as do a plethora of traits allowing the inference that the cult itself was, in fact, manifest. I concur, therefore, with the Kelleys (1975) and DiPeso (1968, 1974) on this point. While it is true that snake symbolism is old and widespread, as suggested by McGuire (1980: 24), there is no reason to doubt the possibility that the Quetzalcoatl cult did, in fact, appear in Chaco Canyon, perhaps manifesting a combination of Classic Mesoamerican symbols as well as those of the Toltec. I believe it would be terribly difficult, if not impossible, to unravel and piece together specific aspects for this or any other Mesoamerican derived cult since modification or re-interpretation almost certainly occurred. Such redefinitions were commonplace in the Mesoamerican heartland and thus, why not for the Anasazi area as well?

Of particular interest is the belief that it was Quetzalcoatl who tutored Mesoamericans in the lapidary arts, and that he held turquoise sacred (Nicholson 1957, Duran 1964, Sejourne 1960). Clearly the ritual use of turquoise in quantity occurs, and has parallels in the modern pueblos as well. There is also the matter of conch shell trumpets. These, too, occur within Chacoan sites and are quite likely associated with the cult of Quetzalcoatl. They continue to be associated with Plumed Serpent ceremonies at Hano (Hopi-Tewa). The Hopi use a gourd trumpet as a probable replacement or substitute for a shell one. At Zuni a conch shell trumpet is also used and is continually blown by the not visible “keeper” (priest) who is holding the tail of Kolowisi (Plumed Serpent) as the procession enters the village prior to the actual ceremony (Stevenson 1904: 94–95). In another context at Zuni, the Big Shell society, a defunct warrior fraternity, could cause enemy death by merely blowing their conch shell trumpet; it was also blown during the Scalp Dance for rain while each of the Bow Priests whirled a rhombus so that clouds might gather (Stevenson 1904: 602). In the latter instance, does the conch shell represent Quetzalcoatl in his warrior guise? At Teotihuacan both the god and the conch appear in sculptured form on the facade of the Temple of Quetzalcoatl. Interestingly, at Chaco a conch shell trumpet was found on the north bench recess of Kiva R in Pueblo Bonito—associated with a bowl having an interior meandering band design highly reminiscent of a serpent (Judd 1954: 305, Plate 82a, b).

While it would be possible to expound on other guises of Quetzalcoatl and the associated cult, I shall defer to the literature—particularly sources which were not cited by McGuire. These include important contributions by Ellis and Hammack (1968: 40–43), Ellis (1977: 9–11), Reymar (1976), Schaafsma (1980: 237–239) and Switzer (1971). Herein one may begin to realize more clearly the deeply embedded nature of the Quetzalcoatl cult in its various symbolic associations. One may also separate Quetzalcoatl, and his guises, from the two horned snake as well as from the rattlesnake. Further, McGuire (1980: 24) errs in citing Tewa as lacking the Plumed Serpent according to Parsons (1939: 184). She notes Tiwa, not Tewa for this distinction.

McGuire has contributed significantly by pointing out the “grab bag” nature which has characterized a number of Mesoamerican-Southwestern parallels—including (perhaps) the Quetzalcoatl cult; however, I believe he has gone to the extreme in attempting to put down the concept. For example, one must note the Toltec were extant at the proper time, they were expansionistic (witness Chichen Itza!), and for the time in question such groups as the Maya, Mixtec, Zapotecs, etc. need not be considered. That is unnecessarily compounding the issue as it relates to the Anasazi. Rather than negatively closing the issue as McGuire has done, would it not be beneficial to suggest the data are inconclusive and in need of reanalysis? In actuality, the sources not cited by McGuire, in part, provide some of these data; however, now that a clearer understanding of the problem has been delineated by McGuire, someone will surely broach it—head on! I believe such an endeavor would be highly productive based on all currently available data. Simply stated, the question is: How, why, when and where did the Quetzalcoatl cult enter and become amalgamated in Anasazi/Pueblo culture? Be explicit with respect to each area of your inquiry!

With respect to other cults, the kachina cult as a manifestation of the Tlaloc cult in Mesoamerica is not well explicated by McGuire (1980: 25). His dates for inception into Pueblo culture are in error by ca. two centuries. Rather than A.D. 1300–1400 as he suggests, A.D. 1050 to A.D. 1100 has been conclusively demonstrated by Schaafsma (1972: 97, 1980: 223)
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235–237). Entry into the Southwest was via the Rio Grande and reached the Anasazi by way of the Jornada branch of the Mogollon. It then spread westward and became highly pronounced in Pueblo IV (A.D. 1300–1400). Schaafsma’s (1980: 237) statement is particularly cogent: “. . . the Jornada Style is important in providing positive evidence for a logical historical and cultural connection between the Tlaloc cult and Pueblo kachina cult, the Jornada region being culturally affiliated with late developments at Casas Grandes, which were ultimately traceable to central Mexico.”

The importance of the Jornada branch of the Mogollon, and of Anasazi/Mogollon interaction in the Rio Grande now offers insights previously only considered in passing (Frisbie n.d.). Coupled with materials gleaned by Riley for proto-historic and historic contact with Mesoamerica via the Zuni region, as well as the work of others for these and earlier periods, one may begin to grasp a more accurate understanding of the complexity of interaction between Mesoamerica and the Southwest.

Conclusion

As indicated throughout the body of this paper, an obvious selective process (or bias) is consciously used to present a specific line of evidence moving toward a hypothesized solution to a given problem. My concern has been to take to task McGuire’s alternative hypothesis to account for Mesoamerican influence in the American Southwest. In so doing I have attempted to offer not only arguments to contradict some of his statements but also my own alternative interpretation. I feel it is important to note that not until all pertinent data have been fully assessed can we hope to discern the true nature of the interaction. Even then it must remain somewhat speculative because we shall probably never conclusively demonstrate precisely what transpired. To date, the pochteca-like explanatory model has been raked over the proverbial coals of academia with both adherents and detractors presenting relatively shaky cases. This is unfortunate, particularly since the arguments have continued on an off-and-on basis for some 26 years. Clearly, something of significance must be involved to sustain interest throughout the duration. Within the last several years increased interest has led to reinterpretations of old data particularly as they relate to the Chacoan Phenomenon, and has provided newly acquired knowledge from northern Mexico. The latter has long been needed; we desperately need more.

While no one should question interaction of long standing nature between Mesoamerica and the Southwestern United States, it is of paramount importance that we begin to grasp the mechanisms involved beyond banterings between two factions. The problem of “kill and overkill” manifest in the literature centers on polemics which cannot solve the problem. Symposia, too, have attempted to bring together interested individuals, but these have been invariably one-sided. On could ask in all seriousness, “Is there a way in which agreement could be reached (or at least be more fully explored)?” I believe to answer this question requires not additional symposia or individual papers; rather, what is necessary involves a roundtable discussion/debate fired by a series of position papers. The latter would be prepared by invited specialists with the express purpose of raising major issues. Each participant would be charged with researching beyond his or her present knowledge to glean new insights where possible and appropriate. The time is ripe for such an endeavor! The proceedings would, needless to say, be published for perusal and comment by all interested in the topic.

Because the relationships between Mesoamerica and the Southwest reflect dynamic interactions of considerable magnitude, it would seem almost inconceivable for anyone to look at the amassed data of incontrovertible nature and deny the existence of the implied relationship. It, therefore, is not possible to consider the Southwest without consideration of Mesoamerican input; there should not be a research design which totally neglects this relationship. Those which do are, in essence, skirting or scuttling an important area of potential contribution. While research designs of limited scope may provide valuable data, the full gamut of their implications not infrequently goes unrecognized. Predominately, research designs are isolationist-oriented by the very nature of their limited scope. Although it is far more exciting to emphasize the “big splash” of research with respect to the local or even regional area, bits and pieces of data which relate to interpretive aspects of the Mesoamerica connection are of extreme importance. These, therefore, must be recognized and reported.

I believe the vast majority of southwesternists suffer from a psychological syndrome which has yet to be labelled, fully recognized and accepted as truth. This might be termed “international borderline syndrome.” At present the few extreme cases may be equated with idiocy, while the vast majority appear to suffer from the affliction in a variety of degrees without recognizing it. Herein it has few or no outward manifestations. These cases may be helped with proper treatment. One simply begins by self-admission of a simple “given”: there is really no cultural barrier between Mexico and the United States other than an (imaginary) line drawn as a result of the Treaty of Guadalupe-Hidalgo in 1848. This “line” has altered cultural connections only since its establishment, and even here one might question
Acquiring the proper perspective may be accomplished by examining the map accompanying Martin’s (1979, fig. 1) paper, “Prehistory: Mogollon.” The text, however, is cautionary: “It is not known precisely where corn and ceramics came from, but it is most probable that they appeared as the result of the interchange of information and trade from Mexico, where maize and pottery were already several millennia old” (Ibid.: 63, 64). Such a statement is of profound interest to my argument since Martin’s map indicates Mogollon culture existed in Mexico as well as in Arizona and New Mexico. Need I say more? For those interested, the question of such fictitious boundaries and frontiers has been more fully explored in a previous paper (Frisbie n.d.)

While inclusion of the Mogollon in this Anasazi Symposium may seem somewhat spurious, such is not the case. Of the major culturally defined entities within the Southwest, only the Mogollon definitely existed in northern Mexico as well as in the American Southwest per se. It becomes, therefore, of considerable importance as a cultural transmitter since it was coterminous with the more northerly Anasazi and on one of the direct routes of cultural interchange. As previously indicated, McGuire treats these routes with considerable acumen, correctly assessing their importance with respect to both Mogollon and Hohokam cultures.

As indicated at the outset, McGuire’s paper affords an excellent opportunity for positional papers to strengthen or dispute his arguments. It is my belief that the majority will attempt the latter, as I have done. The beauty of his paper from my perspective is that it provides the first overall synthesis since that of Kelley and Kelley (1975) and offers an alternative which, though palatable, lacks acceptability for the very reasons he claims pochteca theorists lack credibility: namely, misuse of data and selectivity of those data which support a thesis while neglecting those which do not. He has, nonetheless, positively indicated for the skeptics a high level of interaction between Mesoamerica and the Southwest. For the staunch supporters of this interchange he offers an opportunity to assess where gaps in their arguments exist; he has clearly indicated where they occur. It is hoped that this paper has begun to provide some of these data, as well as a discussion of some of the other related issues.

I would like to close with a comment which has not been addressed elsewhere, but which is of considerable interest. The Southwest has long been in the forefront for the presentation and testing of newly devised methodological and theoretical constructs. The number of past and present practicing specialists is awesome. At the same time we have had what I believe can be shown to be a higher level of expectation for substantiation of fact rather than suppositional statements which are presented as fact. What this implies is that it is easier in most other areas of the world to supply arguments for the solutions to problems on less well documented evidence. Elsewhere, for example, the pochteca concept as a viable theory would not, in my opinion, have received the “flack” it has received in our own Southwest. We demand absolute verification. “The best bet yet” is not sufficient to meet critical levels of acceptance by the majority. One might ask, therefore, “Do we have our sights set too high; can we ever hope to achieve a data set of sufficient merit?” While this is perhaps unanswerable at present, it is in part unanswerable because a number of clouded issues treated above preclude its adoption. Not until we obviate these issues will it be possible to assess the pochteca or pochteca-like theory. In the meantime, it will not be allowed to die a quiet death as McGuire and others might prefer. There is just too much at stake.

NOTES

1. It was hoped there would be others interested in presenting this broad topic at the Symposium. No one came to the fore. Thus my initial offer to present a second paper on this important external connection was accepted as of mid-June. I am indebted to Dr. Smith for extending the October 17th deadline for revised papers so that the presentation version could be revised for publication.

2. Some of the critical comment I level against McGuire incorporates data published subsequent to submission of his manuscript. Therefore, he cannot be held responsible for neglecting these sources.

3. Referral is made to the Index, “Meso-Americans; prehistoric influence.” Thirty-nine paginal citations are included herein.

4. The updating may reflect that done by the volume editor, Alfonso Ortiz, although this in unknown.

5. Sources for the composite analysis presented herein offer some possible variation. I have attempted to select a series of features most characteristic of this level of adaptation based on Service (1966), Lee and DeVore (1968), Coon (1971) and Bicchieri (1972).

6. An excellent review of caves as loci of ceremonial activity in Anasazi/Mogollon cultures is presented in Ellis and Hammack (1968); however, many others are known to be and have been used.
7. Ralph P. Marshall (1978) provides a recent review of stirrup-spouted and ring vessels within the Southwest and elsewhere. Additionally, I have an in progress monograph which provides a somewhat different perspective than that afforded by the above. For temporally later cylindrical vessels (Chacoan) see Washburn (1980).

8. Elizabeth Morris (Personal Communication at this Symposium) suggests Kokopelli was introduced during Basketmaker II, based on petroglyphs appearing in this context.

9. The widespread and persistent trait of making small quantities of basket impressed ceramics throughout the Anasazi continuum may reflect a ritually-oriented act of making pottery in honor of Old Clay Woman. I base this assumption on discussions I have had with Cochiti and Hopi potters.

10. An Aztec appellation who is known by a variety of names elsewhere in Mesoamerica and the Southwest as well.

11. I would note, too, that prior to the recent period, archaeologists were not especially interested in or concerned about identifying such areas. It is unfortunate that the Chaco Project excavations at Pueblo Alto dealt with a pueblo which had been stripped of the majority of its material remains at the time of abandonment. Additionally, a good deal of turquoise occurring within excavations of Chacoan sites during earlier times found its way into native workmen's pockets.

12. See Riley in Frisbie (this volume) for specific references.

13. Here one might wish to include Ootam and Hakataya (see DiPeso and Schroeder in Ortiz 1979).

Questions and Comments

Comment: Going back to the early part of your paper, to hunter-gatherer religion, you mentioned pahos. I don't see why you need to go to Mesoamerica for the fact of their religion because . . .

Ted Frisbie: I really am not. What I'm saying is that Desert Culture, the Archaic, was widely spread and it had a shared thing except for a few odds and ends. I'm not saying this came from Mesoamerica; this is one of the things, including the high god concept, that was part of it all over.

Comment: O.K. What I wanted to say mainly was in the paintings of the Pecos River (incidentally I don't feel that the Pecos River paintings are necessarily related to Mexico) you have a great deal of ideology expressed in these, and a lot of it can be explained in the shamanistic model; and the shamanistic model doesn't necessarily have to emanate from Mexico, it is widespread. There are alternative possibilities.

Frisbie: I agree with that. I was trying to get the point across that for the Archaic or Desert Culture this is a widespread thing. I'm not saying that comes out of Mesoamerica. What I am saying is that when you get the development of high culture, there is something already for them to pull on, to put in perspective, tying in, adding to it, the maize. When that becomes big that becomes part of the superdities.

Question: In the Basketmaker period you have a lot of rock art where their ideology is expressed. Some of the Basketmaker material seems to come out of the earlier Archaic, or it looks like it does. I wanted to ask you if you could actually pinpoint Turquoise Woman, Two-Horned Snake, the Old Fire God, at that time?

Frisbie: Not Basketmaker II.

Question: Or III?

Frisbie: In Basketmaker III, my assumption there is that because we get turquoise, there is a possibility. Here's another thing, and this is an important point: there are, for example, 36 dieties for the Hopi. They generally do not depict dieties. The dieties are something that are up there. When we had a man walking on the moon the Zuni went out of their minds. They had a flu epidemic; it became moon flu because we had desecrated the body of Moon Mother. It was terrible for them. Anyway, they don't normally depict these, they don't often impersonate them. Now they do, some of them. And they come either with face paint or with masks of some kind, but face paint with some kind of headdress, maybe a wig or something of that kind or arrangement. But, masks come in later, obviously, with the whole thing of masking out of Mexico, I think. The dieties were here before and they are part of the mythology. And you don't find them depicted very much at all.

Question: I wondered if you had anything you could consistently tie that into?

Frisbie: No. The first things are the possible maize deity symbols, the cone-shaped stones that are not the ones used as fire dogs in the hearths. There are some of those early that may actually be representations of the maize deity.

Al Schroeder: Talking of masks, it might be that we should consider, as I sort of postulated in this last article of mine, that the Hohokam came in as Mexicans with a diffuse type of pochteca or other organization of social and religious society introducing some of this stuff, and at the same time rejecting other parts, in other words not bringing the full picture in with them, and distributing this through the Southwest by one
means or another through their trade and diffusing it. I also treated with those traits, and every damn one of them occurs in the Hohokam before it does in Chaco except for the masonry types and the colonnade. So I think maybe we should also think in terms of how it's coming up from Mexico, through what groups, and whether or not the trading organizations were still in existence, bringing it up in this manner, and finally breaking down when you get to the edge of the frontier.

Frisbie: I don't recall all of McGuire's statements, but he went through the routes with the opening and closing with shell and whatnot to see what types were coming in when, and the shift from Hohokam introductions or trade over to Casas Grandes where there were specific types of shell. Incidentally, to get into shell, you know the famous thing with conch shell trumpets in Chaco, guess where they still occur: Zuni. The Big Shell Society. It is now defunct and they buried it, but there, again it's there.

Another thing I've got to add real quick; in the myths about Old Salt Woman, the legends, she was desecrated, she left Zuni Black Rock Lake (a spring there) and went to Salt Lake. As she went she dropped her feathers, so you can see her route, and then she continued on. As she moved, the Zuni say, things sort of went away from her, so there was a wide trail. I don't know whether it was thirty feet or not, but they claim that there was, in fact, a wide trail and you could see from Salt Lake, looking up on the mesa, for a long time this wide thing coming down that was where she actually passed. I think it's going back to Chaco days, and I think the Zuni are referring to a Chacoan road that led to Zuni Salt Lake. I talked to Tom Lyons about it. It could be that the modern road may go right over it because it's fairly straight leading in. But salt is very important, and I'm convinced the Village of the Great Kivas and a couple of other Chacoan outlyers were tied in with that.
This section was originally meant to be a true summation, but there is really no way briefly and simply to sum up the diversity of subjects covered, ideas expressed, and questions posed during the two and one-half days of the Anasazi Symposium. It is not altogether certain, for that matter, that a summation is really necessary. Better, perhaps, to let the participants speak for themselves, as they have done in the preceding pages, and let the reader draw his own conclusions.

We did cover a lot of ground, exploring ideas about the origins of the Anasazi, the nature of their interactions with both their natural and their cultural environment, their achievements and their failures, and even what it is, basically, that we are talking about when we say “Anasazi.” Perhaps as important as the individual ideas, however, was the spirit in which they were expressed. There were lively discussions and clear disagreements, but mostly the spirit was one of unity, of maintaining the common bond while disagreeing on specifics. There seemed to be more of a feeling of this shared common bond than might have been the case not very many years ago when relatively superficial issues such as “new” versus “old” archaeology threatened to produce divided and armed camps. The main concern here seemed to be to get on with the business of doing archaeology, regardless of whether the methodology was new or old. This was perhaps the most heartening aspect of that intensive two and one-half days.

One of the highlights of the Symposium was a statement read by Joe Ben Wheat on the last day. It purported to be a letter which someone had written analyzing the proceedings of the first two days. It read:

> Although the ability to diversify a negative imbalance situation is one of the keys to socio-cultural stability, the increase in adaptive strategy, variety, and the modal illustrates the reciprocal nature of cultural-environmental causality, including strategy and enhancement, socio-economic alleviation, and demographic changes which provide insights into the internal dynamics affecting cultural and environmental relationships. This is, of course, providing the subsistence synthetic framework, reconstruction, and functional analysis are seen within a controlled comparative perspective of the interaction spheres. The acceptable data in a logico-deductive model of the Anasazi cultural continuum, although beyond an egalitarian concept, are obviously integrated with the demographic changes as seen within a comparative perspective, and must be correlated with a precise contemporaneity of their connection to the extant adaptive context. Wish you were here.

This all too close approximation to the reality of much of contemporary archaeological writing presented a moment of comic relief to the Symposium. An outstanding aspect of the Symposium was the relative lack of such obscurantist verbage in the papers which were presented. For the most part the papers were direct and to the point and relied on the worth of the expressed ideas rather than an intricate phraseology to carry them. A very pleasant change, to say the least, and hopefully the beginning of a trend.

A question asked by several people during the Symposium was, “Is this going to be an annual affair; are we going to do it again?” We planned this as a one-time affair, and we here in Mesa Verde National Park have no plans to do it again, at least for a while. Perhaps it could become an annual affair, but sometimes when things become regularly scheduled they are held more because it is time to hold them than for any particular need to do so. It would be a nice thing, though, if there were an Anasazi Symposium every few years so that people could gather and discuss things and share their ideas as they did during this one. In any event, hopefully someone will pick up the idea again in the future. Meanwhile, maybe the best answer is contained in the phrase with which this Symposium came to an end: “Until somebody calls us together another time, we stand adjourned.”

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