

I

Introduction

In the middle of a vast, empty, high desert basin in northwestern New Mexico, there is a low-walled, unimpressive canyon where a number of sand-filled washes come together to form a single entrenched dry streambed. In summer the heat can be overwhelming; the air rises in shimmering waves, the low humidity leaves the visitor feeling desiccated and drained. In late summer, towering thunderstorms fill the air with lightning, thunder, and drenching rain; in a matter of minutes the dust-choked streambed can be transformed into a swollen, terrifying torrent, scouring and washing away everything in its path. In winter the canyon is often bitterly cold; in spring it is blasted by endless days of howling, gritty winds. It is a harsh and arid world, empty and silent except for the wind and the occasional cry of hunting raptors.

And yet, here in this hostile and barren place lie the remains of a remarkable and endlessly fascinating prehistoric society. For this is Chaco Canyon, whose immense ruined pueblos, great kivas, earthen mounds, roads, and irrigation works have drawn the attention of explorers, scholars, tourists, vandals, and mystics for 150 years. About a thousand years ago, prehistoric pueblo people not only succeeded in establishing homes in this inhospitable place, they were embarking on a great florescence. Chaco Canyon became a place of power and influence, a place of wealth, monumental architecture, and cultural importance. The visitor today, faced with the inescapable evidence of the past glories of this lonely and desolate place, cannot but wonder how this florescence came about and why.

I first became interested in Chaco because I viewed it as an excellent test case for a larger anthropological problem in which I was interested. In the course of working on a large excavation project studying late Basketmaker III/Pueblo I settlement in a river valley in southwestern Colorado, I had begun to wonder about political evolution and the origins of formal roles of leadership and authority.

During the prehistoric period under study, settlement shifted from a pattern of small, single- or extended-family farmsteads to one of villages comprising 30–40 or more *households*. The crews involved in the excavation project lived in a field camp of *c.* 40 *individuals*, and even though we had all been accustomed to rules, regulations, and authority figures all of our lives and even though we had a preestablished authority figure living in the camp, the process of keeping daily life organized and running smoothly for a group of that size was not always easy. Consequently, I began to wonder how the prehistoric inhabitants of the valley, who almost certainly had no preestablished authority positions beyond those of the family structure, had coped with the increase in organizational complexity caused by living in multifamily villages.

The question that interested me then and that will be the focus of this book was “How do formal roles of leadership arise?” We have a fairly good understanding of the kinship-based and situational kinds of leadership found in mobile band societies. What concerns me here is the next step in the evolution of political organization. With increasing sedentism and dependence on agriculture, the earlier mechanism of conflict resolution – group fission – becomes a less viable option, and (as our fieldcamp experience demonstrated) with increasing group size, organizational complexity increases at an exponential rate. The solution to both of these problems is institutional leadership, but where do these leaders come from? How are they recruited? How do they legitimize their authority over their neighbors? What is the basis of their power?

I chose Chaco as my test case because it is one of the three well-documented instances in pre-contact North American archaeology of a cultural system that apparently moved beyond egalitarian organization and took the first steps toward complex society – the other two being Hohokam and Mississippian. Chaco offered at least two advantages over the other possible test cases. First, the archaeological remains of the Chaco system lie in the remote and sparsely occupied San Juan Basin of northwest New Mexico and thus are largely intact, while large portions of both the Hohokam and Mississippian archaeological records have been disturbed or obliterated by subsequent occupations. Additionally, since I was working at the University of New Mexico, Chaco had the advantage of being close at hand, both physically and in terms of data availability.

Once I began my research on Chaco, however, I discovered (as a long line of previous researchers had discovered) that I was hooked. As an archaeologist, I find Chaco endlessly seductive; we know so much about it, yet every day it becomes clearer how much we still don't know and how little we understand what we do know. The more that I read and the more that I learned, the more I became dissatisfied with previous efforts to account for the growth and nature of the Chaco system. Ultimately my test case took on a life of its own, and I became just as determined to offer an alternative explanation for Chaco as I was to understand general routes to political complexity.

I have tried to balance these two goals in this book. In subsequent chapters, I will describe the Chaco case in detail and discuss previous efforts to account for this remarkable cultural development. I will consider a number of possible approaches to the study of political evolution and suggest how I think one of the more promising approaches can be applied to Chaco, developing my own explanatory model in the process. Before I begin with the specifics of the Chaco case, however, I would like to say a few words about the intellectual tradition within archaeology into which this work falls.

Social organization studies in archaeology

In the early 1970s, as part of what has been called the New Archaeology, there was a good deal of excitement about the possibility of reconstructing aspects of the social organization of prehistoric systems. The classic examples of this genre are Deetz

(1965), Hill (1970), and Longacre (1970). Responding to the call for archaeology to become more anthropological, Hill and Longacre and others modeled their work after that of their colleagues in cultural anthropology, who were deeply immersed in questions of kinship, residence rules, and descent.

The underlying premise of these social organization studies was simple but very important: “the patterning of material remains in an archaeological site is the result of the patterned behavior of the members of an extinct society and . . . this patterning is potentially informative as to the way in which the society was organized” (Longacre 1968: 91). The method employed was also fairly straightforward. A hypothesis, generally one based on analogy with the modern Pueblos, was offered about some aspect of the social organization of the prehistoric system in question, and then test implications were derived that would permit the researcher to support or reject the hypothesis. The test implications were statements about patterns that would be expected in the archaeological record if the hypothesis were true. If the majority of these patterns *were* found to exist in the record, then the hypothesis gained a certain credibility; if the patterning of the record did not match the expectations, then the hypothesis was rejected as false.

These studies were subjected to an extraordinary barrage of criticism – much of it accurate (for a good summary of both the studies and the criticisms, see Lightfoot [1984: 7–15]). It is true, as the critics noted, that these researchers made assumptions that were at best unsupported and at worst demonstrably false. Their statistical methods and use of the ethnographic data were naive. And generally they failed to consider the processes by which archaeological sites are formed, making statements, for example, about the activities of inhabitants of a room based on the kinds of materials found in what would now be recognized as the postoccupational fill of the room.

Despite the validity of many of the specific criticisms of these pioneering studies of social organization, the general conclusion reached by some of the critics was at least as unsupported as the assumptions that they were criticizing. Even the best critiques of these early attempts at social organization studies (e.g., Allen and Richardson 1971; Dumond 1977) used the particular failings of particular studies to reject the very possibility of studying prehistoric social organization at all. Rather than identify the failings of the particular studies and then suggest refinements of method and new approaches to the problem, most critics offered these specific failures as proof that social organization studies in general were impossible.

I find that it is difficult now to make students new to the field of archaeology understand the excitement and sense of expanded horizons that the work of Longacre and Hill and others created for many archaeologists in the early 1970s. The message of New Archaeology was that normative approaches to the past would never permit us to *explain* variability across space or change through time, and that explanation would require new methods and new theoretical approaches. And the message of the early social organization studies was that there was more to the past than material culture, and that through these new methods and approaches we could hope to gain insight into the organizational properties of prehistoric systems.

In the long run, it does not matter whether the occupants of the Carter Ranch and Broken K pueblos (Longacre 1970; Hill 1970) practiced matrilocality or not. What matters is that Longacre and Hill and their colleagues showed us that it was possible to ask and to answer questions about the rich variety of human social organization. The intense criticism to which these early studies were subjected and a growing realization of the extraordinary difficulty of the task that had been undertaken were sufficient to give pause to even the most intrepid researcher, and there were plenty of critics in the 1970s (and in the 1980s) who said that the whole pursuit was doomed to failure. But there were also archaeologists who would not give up, who were determined to learn from the previous research, to avoid the errors that had been made, and to try again.

Two of the most serious methodological errors of the social organization studies of the early 1970s were (a) that they failed to take into account the ways in which items become incorporated in the archaeological record and (b) that they assigned “meaning” to patterns in the archaeological record without providing any arguments as to why these suggested meanings should be the correct ones. For the rest of the 1970s and most of the 1980s, many researchers interested in the behavioral implications of the Southwestern archaeological record dropped back to study very basic questions about how items come to be where they are in such records. Other researchers have spent these years developing means of recognizing patterns within the archaeological record and wrestling with the immensely complex process of assigning meaning to those patterns.

Those of us still willing to attempt to study social organization of prehistoric societies had to contend with two unfortunate methodological holdovers from approaches that were common in the early 1970s. The first of these was a tendency toward too direct a dependence on analogy with the modern Pueblos. The continued presence in the Southwest of descendants of the prehistoric people that we study has been both a great boon and an irresistible temptation to Southwestern archaeologists. For those of us who specialize in the Anasazi, the marked similarities in settlement and subsistence between the modern Pueblos and their ancestors have served to increase the temptation.

Pueblo ethnographies provide us with a wealth of information that is invaluable for formulating models and generating potential explanations. But the danger of ethnographic analogy is that we will short-circuit the process of model development, testing, and refinement and fall into the easy, unscientific trap of using ethnography as explanation. In the case of sociocultural complexity at Chaco, for example, many archaeologists were reluctant to attribute greater complexity to prehistoric Puebloan groups than was apparent among the historical Pueblos. In some cases this led to simple denial that Chaco was as complex as it looked; in others, Mesoamerican influences or individuals were trotted out to account for this seeming anomaly. The irony of this is that the historical Pueblos themselves were not by any means egalitarian. Recent reassessments of political structure among the Hopi (Whiteley 1982; Upham 1989), the Zuni (Upham 1982), and the Tewa (Upham 1989) make it clear that these supposedly egalitarian societies have, at least since contact, exhibited

marked social, economic, and political stratification based on control of land and access to ritual knowledge.

In the research reported here, I have attempted to avoid over-reliance on Pueblo analogy by concentrating on general, cross-cultural patterns of sociopolitical relationships in societies at a similar level of technology, with a similar population size, in a similar environment, or exhibiting a similar adaptation. In this way I hope to avoid prejudging what political structures we are likely to find in prehistoric Pueblo societies. It is perfectly possible that forms of organization that were common prehistorically have become extinct among the modern Pueblos, and a more general, cross-culturally based analogy can provide us with a wider range of possible organizational principles.

The other holdover from earlier social organization studies in archaeology that I have attempted to avoid here is their self-consciously ethnological orientation. Cultural anthropology in the late 1960s/early 1970s was heavily focused on evolutionary typology and on studies of kinship systems. When “new” archaeologists set out to be anthropological, the questions upon which they attempted to shed light were, understandably enough, those of typology and kinship structure.

In a later chapter I will discuss in detail the impact of a dependence on typology on studies of sociopolitical organization, but in general, an emphasis on typology leads to unprofitable wrangling about the assignment of a cultural system to a specific evolutionary category – was this a chiefdom? a ranked society? a stratified society? The problem with this is that we do not have (and possibly will never have) unambiguous criteria for identifying these categories in any prehistoric archaeological record. We may, with careful argument, be able to suggest the specific level of complexity that we believe existed in a prehistoric system, but generally we cannot test our suggestions without the circularity of referring to the same data that led us to our conclusion in the first place. And the preoccupation with typology has prevented us from focusing on aspects of sociopolitical structure for which we *do* have or can develop recognition criteria.

Like studies focused on evolutionary types, attempts to recognize such specific ethnological constructs as matrilineality or moiety organization in the archaeological record are limited by the lack of recognition criteria for these constructs. It may be impossible to develop sufficiently unambiguous criteria to permit us to recognize such abstract and inferential organizational principles archaeologically. And even if we found an absolutely infallible means of identifying kinship structure in the archaeological record, this would not constitute a road map to other aspects of prehistoric social organization.

Lightfoot (1984: 15–18) provides an excellent summary of the problems with depending on an ethnological construct – in this case, the *segmentary lineage* – to define or explain sociopolitical organization. He argues that “the lineage model” – a construct describing societies in which relationships of unilineal descent and corporate landownership provide “the means of group recruitment and the jural norms that structure intracommunity behavior” (1984: 15) – is not sufficient to explain political structure because genealogy *per se* does not in fact serve to structure group

relations. The actual structure is more complicated because descent rules are likely to be manipulated to sanction a person's position in a lineage as that individual attempts to achieve social, religious, and political power (1984: 17).

Additionally Lightfoot notes that the traditional concentration on descent and lineage structure ignores the constant competition for achieved leadership positions that is ubiquitous in so-called lineage-based societies. Finally, he argues, the lineage model implies that "genealogical structure . . . provides an equilibrium-maintaining system for the repression of potential conflicts and the resolution of interpersonal problems" when, in fact, "recent studies of simple societies demonstrate that their sociopolitical organizations are often anything but stable, equilibrium-maintaining systems" (1984: 18).

Upham (1989) also provides a strong argument against depending on ethnological constructs to explain sociopolitical organization in prehistoric or ethnographic societies. In the case of the Hopi, Upham notes (following Whiteley 1985, 1986) that the tidy ethnographic picture of a clan-based economic and political structure ignores the central reality of Hopi sociopolitical organization: "control of political and religious power by core lineage segments who transmit their authority and economic control to their descendants by manipulating agnatic and cognatic ties of all kinds" (Upham 1989: 88). Upham views the resulting political structure as hierarchical and hereditary, a pattern that is even more apparent in the Tewa-speaking pueblos of the Rio Grande. Rather than the simple sequential hierarchy implied by the ethnographic descriptions of the seasonal alternation of moiety organizations, Upham views Tewa decision making as being controlled by "a multitiered hierarchy . . . , with the Winter and Summer chiefs and the societies of Made People constituting a managerial elite" (1989: 92).

If it should turn out that we are never able to develop unambiguous criteria for recognizing matrilineality or moieties or clans in the archaeological record, therefore, this is by no means a fatal flaw indicating that we should give up all attempts to understand prehistoric social organization. It may be true, as the well-known Willey and Phillips dictum (1958: 2) suggests, that "archaeology is anthropology or it is nothing," but that does not mean that paleokinship is our only possible specialization. Social organization, after all, is a structure that consists of groups and relationships. If we concentrate on identifying groups and inferring the nature of relationships between and among those groups, we can gain an understanding of many organizational aspects of a society without all the problems of applying ethnological labels to those groups or relationships.

In the research reported here, for example, I have attempted to study that set of social organizational relationships identified as "political structure" by considering the evidence for a level of organizational complexity beyond familial and situational leadership and by concentrating on the groups labeled "leaders" and "followers." I have attempted to side-step the Big Man/headman/chief kind of question by focusing on relationships of power, and to examine such aspects of leadership as bases of power, legitimation, degree and scope of integration, etc., without having to identify the specific role filled by a leader. I view these as more manageable questions, given an

archaeological data base, and as questions that may, in the long run, prove to be more interesting and informative ones as well.

I have included this discussion of earlier social organization studies because I think that it is valuable to be aware of and explicit about the intellectual tradition into which one's work falls and because I view my research as being built upon the strengths of those earlier efforts. I have had the advantage of seeing their ideas and approaches offered, critiqued, rejected, and refined. Now it is my turn to offer ideas and approaches, my turn to be critiqued, and (I hope) my turn to contribute some small piece to the foundation for future work.

Definitions

At this point I would like to offer definitions for three general terms that appear constantly throughout this book. The first of these is *system* as used in *Chaco system*. There are many definitions of systems in general (e.g., “sets of elements standing in interrelation” [von Bertalanffy 1968: 38]), and of cultural systems in particular (e.g., “System implies both parts and interrelationships among parts, or structure and function, or process” [White 1959: 17]). For the purposes of this book, I have adopted a very simple definition. In using the term *system* I mean simply to imply interconnectivity. A cultural system, in this sense, comprises a set of individuals or groups that interact more frequently or intensely with one another than they do with individuals or groups that are not culturally defined as being part of the system.

Clearly this concept is too vague to be fully operationalized archaeologically. In some cases there is physical evidence of interaction – the presence in one area of materials that occur naturally or were manufactured in another area, for example, or the presence of physical links such as roads (in the Chacoan case) or canals. The occurrence in two separate areas of whole constellations of morphological or technological traits can be fairly persuasive evidence of interaction, but this kind of argument merges into arguments based on style and other poorly understood cognitive phenomena.

Given the vagueness of this use of the term, it is impossible to draw boundaries around systems, but I am not at all sure that this is a major drawback. In fact, I think that *system boundary* may be an inappropriate concept for prestate cultural systems. States often have fixed borders, border guards, passports, customs agents, tariffs, etc. Nonstate systems, I would argue, have none of these things, have no specific boundaries because there is not *a* system. The question of who is or is not a participant in “the system” has different answers depending upon the perspective adopted. If we are looking at the system from the perspective of trade and exchange, we might identify a very different set of participants than we would select if we were examining religion or language or marriage ties or political ties. In nonstate societies, definitions of “us” and “them” are even more situational than they are in states.

Given this definition of *system*, I might just as appropriately have followed the lead of Judge (1979) and Altschul (1978), who independently arrived at the conclusion that *interaction sphere* might be the most appropriate term for Chaco and described Chaco as an interaction sphere rather than as a system.

One of the important qualities of the term *interaction sphere*, as noted by Binford, is that “it denotes a situation in which there is a regular cultural means of institutionalizing and maintaining intersocietal interaction. The particular forms of the institutions and the secondary functions which may accrue to them will be found to vary widely in the spectrum of history” (Binford 1972: 31). In the Chaco case, I will argue, the institution that created and maintained the level of interaction apparent in the archaeological record was a formal, political, leadership structure.

The other two terms that I should define are *political* and *sociopolitical*. By *political* I mean things having to do with the structure of decision making. By *sociopolitical* I mean to include not only the structure of decision making but the social and economic relationships that arise as a consequence of a particular political structure. In this latter category I would include status differentiation, relations of production, etc., as well as the specific relationships of power and obligation that constitute the political realm.

Preview of coming attractions

In Chapter 2 I will provide a background for the uninitiated on what the archaeological record of the Chaco system is like and a brief history of discovery and research. In Chapter 3 I will discuss in detail those aspects of the Chacoan archaeological record that bear directly or indirectly on the question of sociocultural complexity. Chapter 4 describes possible routes to sociopolitical complexity and ways of looking at and understanding political evolution. In this chapter I will consider especially the problems of leadership and possible solutions to those problems along with the relationships of social power that produce the leader/follower roles. In Chapters 5 and 6 the general discussions of Chapter 4 are applied to the specific case of Chaco – first as part of a critique of previous models of this system in Chapter 5 and then as part of the development of my own model of the system in Chapter 6. Chapter 7 provides a summary and suggests specific directions for future research.

2

The Chaco Phenomenon: background and history of research

During the eleventh and twelfth centuries AD a series of sociocultural developments unlike anything else in the American Southwest occurred in the San Juan Basin of northwestern New Mexico (Figure 1). Archaeological remains from this period include sophisticated public architecture, an extensive and well-engineered road network, and widespread evidence for water-control technology. Likewise, there is evidence of participation in a very active trade network that involved both regionally produced goods and items transported from the Pacific and Gulf coasts and from Mesoamerica. A substantial population was supported in an extremely harsh environment, and there is evidence of an organizational structure capable of mobilizing and directing large amounts of labor. Certainly the magnitude of these accomplishments was small relative to those of the complex societies in Mesoamerica or the Andes, but the scale, degree of integration, and organizational complexity implied by these remains are so anomalous relative to previous and subsequent developments in the Anasazi region that they have given rise to the phrase *the Chaco Phenomenon*.

In this chapter I will provide a brief background discussion of Chaco and Chacoan archaeology for those unfamiliar with this culture area. For more detailed discussions of exploration and early research at Chaco, the reader is referred to Lister and Lister (1981) and Vivian (1990a); for a more detailed overview of Chacoan culture history, the reader is again referred to Vivian's (1990a) excellent summary as well as to a variety of standard references, including Hayes et al. (1981), Stuart and Gauthier (1981), Cordell (1982b), Schelberg (1982) and Powers et al. (1983). The abbreviated discussion in this chapter of the controversy over the degree of complexity to be inferred from the Chacoan archaeological record will be expanded in Chapter 3; the short summary provided here of previous models for the development of the Chacoan cultural system will be expanded in Chapter 5.

Environment and paleoenvironment

As noted in Chapter 1, Chaco Canyon today does not exactly correspond to most people's concept of Eden, and indeed the entire San Juan Basin might best be characterized by the adjective "sparse." Vegetation, rainfall, surface water, edible fauna – all tend to be in short supply, while the major environmental variables present in abundance are wind and dust.

The basin is by no means environmentally uniform, however. The central basin covers an area of approximately 12,000 square kilometers and is largely surrounded by uplifts dating to the Cretaceous. Elevations in the basin range from approximately

2,500 meters in the north to 1,500 meters in the west; the drainage pattern flows largely to the northwest. The surrounding mountains reach elevations over 3,000 meters.

Chaco Canyon was created by the entrenchment of the westward-flowing Chaco Wash into the Cretaceous sandstones and shales constituting Chacra Mesa, a low (120–50 meters), east–west trending uplift in the central basin. The combination at Chaco Canyon of a low upland zone that even today supports stands of small coniferous species and a canyon topography where several large drainages come together within a few kilometers of one another creates a unique microenvironment relative to the rest of the central basin.

The canyon itself is approximately 30 kilometers long, 90 to 180 meters deep (although a stepped profile means that the actual canyon walls tend to be 30 meters high or less), and ranges from half a kilometer to a kilometer wide. Because of the dip of the bedrock and the erosional characteristics of the particular strata, the configuration of the canyon is very different on the north and on the south. On the north, the massive Cliff House sandstone has eroded into a wide bench with steep cliff faces, short side canyons, and large exposures of slickrock. On the south the Menefee shale, which underlies the Cliff House formation, has been exposed. This soft, carbonaceous shale erodes into long colluvial talus slopes and narrow terraces with much less bedrock exposure.

The San Juan Basin is located where the limits of several major weather circulation patterns come together, and this means that both the amount and the timing of precipitation can vary markedly from year to year. Generally, however, the precipitation amount is low (from 20 centimeters in the central basin to 40–50 centimeters in the surrounding mountains), and roughly equally divided between winter snows and summer rains that fall in the form of brief thunderstorms.

Temperature patterns are characterized by wide diurnal and seasonal variation. Yearly temperatures in the central basin range from -24°F to 106°F ; temperatures at Chaco tend to be slightly lower at both ends of the scale. The number of frost-free days per year is strongly conditioned by topography. Although the central basin average is 150 days, Gillespie (1985) reports that the frost-free period at Chaco tends to be considerably shorter, owing to cold air drainage. His figures indicate a frost-free period of fewer than 100 days for about half the years between 1960 and 1982.

Soils in the basin are quite variable, but generally those toward the north and east are of marine origin and are fine to medium grained while those in the southern and western portions of the basin are of terrestrial origin and tend to be coarser. Chaco Canyon roughly marks the dividing line between these two general soil types. Overall, vegetation is closely tied to elevation, with grasslands in the central basin, juniper and mixed piñon/juniper woodlands in the intermediate elevations, and coniferous forests on the uplift mountains surrounding the basin. Local variables such as the presence of dunes or badlands have a strong effect on the exact mix of species available.

Major paleoenvironmental reconstructions for the Southwest as a whole can be found in Euler et al. (1979), Dean et al. (1985), and Gumerman (1988): overviews for the San Juan Basin and the Chaco area can be found in Hogan (1983) and Gillespie (1985). As noted by Gillespie in his summary of paleoenvironmental information for