# PINNACLE RUIN AND MESA VERDE: READING A MESSY RECORD

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The Southwest is blessed with sites of great clarity: cliff dwellings standing as if seven centuries had not passed; intact rooms in great houses, filled with wonderful things; burned pithouses with collapsed roofs, each beam specific to a single year. Textbook cases, but, alas, those happy situations are rare. Much of the Southwest's (and most of the world's) archaeology is not so lucky. Sites were churned by later re-occupants, then scrambled by rodents and bioturbation, and, in modern times, all but destroyed by vandals and pot-hunters. Too often the test-pit bottoms out on a sardine can.

Most sites are a mess. Lewis Binford and Michael Schiffer debated, long ago, how to deal with this conundrum (REFS). Binford, in essence, argued that a messy record had to be accepted and read as messy. Schiffer thought the messy record was merely flawed and therefore perfectible; that is, clarity could be restored (or reconstructed) by careful analysis of depositional and transformational histories. Schiffer, I think, won the debate – at least in the Southwest. We look first (quite understandably) for the cleanest site and, since those are rare, we rehabilitate slightly messy sites to render them clean and clearer, insofar as possible. Our tactics typically proceed from perfect Pompeiis, where contexts are obvious and clear, and work down from that ideal in a fighting retreat to reality. We hope for the best but settle for less. There's a pecking order: from perfect Pompeiis, to a normal somewhat battered site, to bombed-out, paved-over disasters dug only when we must, in CRM projects. Even in CRM, there are preferences for clean and clear over unclean and unclear. In the Hohokam area, burned pithouses are preferred over un-burned, because they more likely contain abandonment assemblages of in situ pots and artifacts, and because burning increases the chance of obtaining an archaeo-magnetic date. But that preference clearly biases Hohokam archaeology towards one type of structure abandonment over all others. Similarly, Chaco archaeologists despair over the complications, ancient re-buildings, and modern reconstructions at central sites such as Pueblo Bonito and Chetro Ketl, and seek clarity in less impacted Great Houses outside the canyon. We seek the "best:" the clearest contexts. It's a workable strategy, which has over many decades produced excellent results.

And, of course, biased results. By sampling for post-occupational criteria (lack of looting, burning, whatever) we sample for our methods and techniques, and not for the ancient realities. Is there anything to be gained by turning that methodology on its head or, rather, looking at the problem from a slightly different angle: thinking about messy ways to understand a messy record? (This was not Binford's way; in the end his tactics and Schiffer's converged; REFS.) I have long been curious about smeared, messy records, especially on regional scales (REFS) but also at sites – perhaps because my first interest in the Southwest was (and remains) the notoriously messy Mimbres. Are there ways to read a messy record directly, without restorative interventions? That is, can we use messy data? – the core of my question, because so many of my colleagues and

students disdain or spurn messy data (especially *old* messy data) – yet most of what's out there is messy.

Moreover, we greatly prefer more precise chronometrics over less precise. This seems like simple common sense – and it is: we value dendrochronology over 14C or archaeomagnetism. And it's value for dollar: tree dates are cheap. Somehow, it seems simply wrong to waste money on 14C for a Four Corners pueblo; in the absence of tree-ring dates, archaeologists digging a big stone ruin seldom invest in far less precise, far more expensive 14C.

The following essay compares two chronological cases at opposite poles of clean and messy. The clean: the precision of many thousands of tree-ring dates from hundreds of sites in the Mesa Verde region and the Four Corners – but, as used here, out of their precise site contexts. The messy: the confusion of two-dozen 14C dates from test excavations at two badly disturbed sites in central New Mexico, again out of context. In the process of posing (and answering) questions about these sites, a suggestion of method emerges for dealing with messy sites and messy chronologies. That method, as presented here, is of specific and limited application, but the logic behind it may suggest ways to think about messy archaeology on its own term – not to replace our standard, careful, contextual approaches, but as another way to wring understanding out of a messy record, playing the cards we're dealt.

The essay proceeds as follows. A large archaeological question is defined (in the following section, "Migration from the Four Corners"). An archaeological situation near Truth-or-Consequences, NM is introduced which may address this question ("The Magdalena Phase"). One major site of the Magdalena phase is described as is a second nearby site, which together have implications for evaluating possible migration from the Four Corners ("Pinnacle Ruin and the Victorio Site"). The significance of the chronology of the two sites is discussed at some length ("Dating"): were the two sites sequential or contemporary? With that issue resolved, the dating of the Magdalena phase – based on small number of 14C determinations – is compared to the chronology of population loss and gain in the Mesa Verde region and the Four Corners ("Pinnacle Ruin and Mesa Verde"). Finally, the essay returns to the themes introduced above ("Reading a Messy Record").

## MIGRATIONS FROM THE FOUR CORNERS

The abandonment of the Four Corners is a classic theme in Southwestern archaeology: at about AD 1250, tens of thousands of people lived in hundreds of villages around the Four Corners area, from the Mesa Verde district on the east to the Kayenta district on the west. By AD 1300, the area was effectively empty. The conventional terms for time periods discussed here are Pueblo III (AD 1150 to1300) and Pueblo IV (AD 1300 to1450/1500), with overlap around 1275-1325. All dates in this essay are AD or CE; the prefix will not appear hereafter.

The term "abandonment" is questioned by Pueblo peoples, descendents of Mesa Verde and Kayenta peoples. Pueblo people explain that the Four Corners area remains an active part of Pueblo life, historically and spiritually, and therefore the region should not be termed "abandoned." Demographically, however, the Four Corners went from one of the most densely occupied regions of the Southwest around 1250 to effectively empty by 1300 (Kohler et alia 2010). Call it what we will, the depopulation was a major event in southwestern prehistory.

The literature on southwestern migrations is extensive (e.g. Haury 1958; Jett 1964, Peckham et alia REFS; Spielmann ed. 1998; REFS), of which studies of the depopulation of the Four Corners constitute a large sub-group or genre (e.g. Cameron ed. 1995, REFS, Collins REFS, Kohler et alia 2010, Ortman REFS, Blinman REFS; Lekson REFS). For various reasons—historical and substantive—research on the Four Corners focuses on two geographic "poles" or sub-regional histories: in the west, Kayenta to southern Arizona; and in the east, Mesa Verde to the northern Rio Grande.

Barbara Mills recently reviewed the study of ancient population movements in the Southwest, and reached several happy conclusions (Mills 2011). First, migration is back: a staple of southwestern archaeology through the 1950s, migration was dismissed in 1970s and 1980s as essentially useless, a "non-explanation" (REFS) – despite traditional Pueblo "migration histories" and well-known evidence from central Arizona sites (Haury REFS, Reid REFS). Beginning in the early and mid-1990s, Salado archaeology in the Tonto Basin (particularly the work of Desert Archaeology Inc; REFS) and terminal Mesa Verde studies (particularly the work of Crow Canyon Archaeological Center; REFS) necessarily focused on migration. Population movement was reestablished as a legitimate research interest (Cameron 1995; Clark 2001; REFS). Subsequently, "migration has become a growth industry – and only a few holdouts are resisting the idea that migration was a way of life for all Southwestern peoples" (Mills 2011:345).

Secondly, we are thinking more perceptively about migration: "this [new] literature … incorporates new theoretical approaches" (Mills 2011:345). With the return of migration in the early 1990s, method and theory (silent on this topic, in the southwest, for decades) had to catch up fast. Fifties' thinking was ill-suited to nineties' problems. We no longer expect or insist on the simple clarity of a "site unit intrusion" (REFS: although that rare condition appears in the Southwest: Haury REFS, Clark REFS). It has become clear that archaeological expressions of migration vary widely, along dimensions of scale, connectivity, and transformation (discussed below; Ortman and Cameron 2011; Mills 2011). For example, depending on factors of scale, connectivity, or transformation, a migration might "stand out" or "blend in" with pre-existing populations (Stone and Lipe 2011) – leading to two very different archaeologies. Some migrations leave only demographic evidence, with a sudden rise in population; others leave material clues in out-of-place architecture and artifacts.

Finally, Mills notes the theoretical potential of parallels between ancient Southwestern migrations and modern population movements; specifically, colonization of empty landscapes; internal frontier migration, and diasporas (Mills 2011:355). It is possible that

archaeological studies can inform broader thinking on modern population movements: "…these overarching models … are now common in the migration literature – a literature that could learn from the long-term dynamic histories of our Southwestern case studies" (Mills 2011:356).

The dimensions of variation defined (so far) for population movement include scale, connectivity and transformation. Mills (2011) expands on the original formulation of Scott Ortman and Catherine Cameron (2011); her scheme is summarized in my Table X. "Scale" includes dimensions of social/demographic size, distance, and timing; "connectivity" refers to social networks among migrants before, during, and after migration; "transformation" dimensionalizes the impacts of migration on the immigrant and host societies, and resultant ethnogenisis. Pinnacle Ruin fits thus (Table Y):

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Table X: Dimensions of population movement, from Mills 2011 (elaborating Ortman and Cameron 2011).

SCALE (Social, Spatial, Temporal) (Mills 2011:Table 20.1)

> Social scale of moving unit: individual, household, lineage, clan, faction, community, society.
>
> Destination population: light, dense.
>
> Ratio of immigrants to locals: low, balanced, high.
>
> Pace of movement: singular event, short-term process, long-term stream.
>
> Distance moved: Close, moderate (2 days to a week), far (more than 1 week).
>
> Impact in source area: negligible, significant reduction, depopulation.

CONNECTIVITY (Social networks) (Mills 2011:Table 20.2)

> Reasons: Environmental, climatic, anthropogenic, economic, political, ideological, religious.
>
> Relations immigrant/local: conflict, hostility, coexistence, enclaves, co-residence, blending, assimilation.
>
> Regional interaction among migrants: minimal, limited, economic, ritual.
>
> Choice of destination: Ideological, religious, kinship, exchange, environmental.

TRANSFORMATION (Ethnogenesis) (Mills 2011:Table 20.3)

Long-term impact on destination society: negligible, focused, pervasive.

Expression of homeland identity: none, negligible, brief, extended.

Regional conflict: Low, moderate, ["high" implied but not noted]

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Table Y: Pinnacle Ruin, dimensionalized a la Mills 2011 and Ortman & Cameron 2011.

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SCALE

Social scale of moving unit: faction or community.

Destination population: light, dense???.

Ratio of immigrants to locals: low, balanced, high???.

Pace of movement: singular event or short-term process.

Distance moved: far (more than 1 week).

Impact in source area: depopulation.

CONNECTIVITY

Reasons: political, ideological.

Relations immigrant/local: coexistence, enclaves, co-residence???

Regional interaction among migrants: economic, ritual.

Choice of destination: environmental.

TRANSFORMATION (Ethnogenesis) (Mills 2011:Table 20.3)

Long-term impact on destination society: negligible, focused, pervasive???

Expression of homeland identity: brief

Regional conflict: low

Western (Kayenta) migrations have a long and honorable history in Arizona archaeology (e.g. Haury 1958, Lindsay and Dean 1983, Reid REFS), re-invigorated by CRM projects of the 1980s and their spin-offs (e.g. Clark 2001, Lyons REFS, Stark et alia 1995). In southeastern Arizona, several Kayenta communities have been identified as enclave sites (sometimes defensively situated; REFS) or as distinct areas or "barrios" within larger indigenous settlements (Haury 1958). Identification of migrant sites or groups is facilitated by the coherence of Kayenta groups, who retained many highly distinctive aspects of material culture (both pottery and architecture) and presumably the social and ideological institutions represented by those material objects. Indeed, several migrant Pueblo III and IV sites in southeastern Arizona represent classic "site unit intrusions" (Rouse? REFS)—the sine qua non of migration studies—very closely resembling in layout and in detail Kayenta sites found hundreds of km to the north (e.g. REFS). Archaeologists agree on Kayenta migrations; it's easy to see migrants in Arizona.

Research on the Mesa Verde to northern Rio Grande migration has been less synergetic. To begin with, the record was divided by a state line: most of Mesa Verde was in southwestern Colorado while the northern Rio Grande is mostly in New Mexico. It was generally assumed by archaeologists working in southwest Colorado that Mesa Verde peoples migrated before 1300 (the close of Pueblo III) in several southerly directions, but most importantly southeast to the Rio Grande (REFS). This interpretation was buttressed by an evident demographic surge in the Rio Grande in the early 14th century (Pueblo IV) (e.g. Crown, Orcutt and Kohler 1996) and by the appearance of Galisteo Black-on-white (long considered to be a post-Mesa Verde type; Able et al REFS). The population "spike" and Mesa Verde-style pottery presumably indicated an influx of Four Corners people, vacating the Mesa Verde region. New Mexico archaeologists working in the northern Rio Grande, however, were less certain (e.g. Steen REFS). Recent years have seen questioning of Galisteo Black-on-white's historical linkage to Mesa Verde ceramic traditions (Wilson 2008) and, from several archaeologists, the rejection of significant Mesa Verde in-migration into the Rio Grande (e.g. Boyer et alia 2010, REFS).

One problem with the identification of Mesa Verde migrants anywhere (including the Rio Grande) is the lack of post-migration coherent material culture (Lekson et alia 2002; Lipe 2010). Unlike Kayenta "site unit intrusions" of southeastern Arizona, migrants out of Mesa Verde apparently did NOT retain key material culture elements which characterized the final Pueblo III Mesa Verde towns which William Lipe (2010) has termed the "Central Mesa Verde Archaeological Complex", which included (among other features): key ceramic forms such as mugs (Putsavage REFS), pitchers (Bradley REFS), kiva jars (REFS) and (possibly) ladles; the finely-formed tchamahia (or celt, REFS); unit-pueblo or "Prudden" unit residential forms (e.g. papers in Adler REFS); and the very distinctive "key-hole" kiva, found at almost every Mesa Verde residence – but only rarely in areas to which Mesa Verde presumably migrated (Lekson et alia 2002; Lipe 2010). These architectural forms and artifacts, highly characteristic of Mesa Verde settlements, disappeared in the Pueblo IV southwest – that is, after 1300, no one made or used these things anymore (Lekson et al 2002; Lipe 2010). In contrast to many Kayenta migrants, Mesa Verde migrant groups did not retain coherent material culture harking back to their Pueblo III origins. Unless they all died in place without issue, the several tens of

thousands of people living in the 13th century Mesa Verde region had to go somewhere. But they are hard to "see." This absence of evidence is, in fact, a strong pattern telling us about the nature of migration (discussed below) but, operationally, the absence of "diagnostic" material culture or "site unit intrusions" makes it difficult to identify Mesa Verde migrants. Presumably they went somewhere, even it they are now archaeologically invisible. An alternative, discussed below, is that Mesa Verde population declined precipitously in situ, and therefore relatively few Mesa Verde migrants actually left the region.

Kayenta-to-southern-Arizona and Mesa-Verde-to-Rio-Grande are geographically at the two extremes of the Four Corners migrations. They may also represent two poles on a continuum of cultural coherence in migrant groups (Lekson et al 2002; Lipe 2010): some Kayenta groups retained much the distinctive material culture (and presumably the social practices) of their homelands. Mesa Verde populations in the northern Rio Grande apparently did not. Mesa Verde migrants in the northern Rio Grande (if any) "blended in" with large existing populations, in contrast to the Kayenta situation (Lekson et alia 2002; Lipe 2010; Lipe and Stone REFS). The absence of Mesa Verde artifacts and "site unit intrusions" underwrite denials of substantial in-migration into the Rio Grande (Boyer et alia 2010). Much direct and circumstantial evidence suggests that Mesa Verde people migrated to the Rio Grande (REFS, Ortman, papers in Kohler REFS); but if they did, they are archaeologically hard to see.

A geographically and, perhaps, socially intermediate archaeological situation in southwestern New Mexico may offer new insights on this situation: the Magdalena phase of western Socorro and Sierra counties.

## THE MAGDALENA PHASE

The Magdalena phase (Pueblo IV) comprises three large pueblos and a handful of smaller sites in west-central New Mexico: the 300+ room Gallinas Spring site near Magdalena NM, investigated by unpublished field schools and CRM projects (REFS); the 100-room[?] Roadmap site, west of Truth-or-Consequences NM, currently investigated by Arizona State University (REFS); and the 200-room Pinnacle Ruin, near Monticello NM (REFS). The University of Colorado conducted excavations at Pinnacle Ruin 2000-2004, and returned for an additional season in 2008. Our preliminary interpretations, based on the first two years' work at Pinnacle, and on examination of notes and collections from Gallinas Spring was that the Magdalena phase represented a possible Mesa Verde migration (Lekson and others 2002; see also Lekson 1986).

This interpretation was based primarily on Magdalena Black-on-white, an organic-painted type with clear similarities to Mesa Verde whitewares (Davis REFS, Lekson REFS, Warren REFS). Selected sherds of Magdalena Black-on-white very clearly resemble classic Mesa Verde Black-on-white (generally thought to date to 1180 to 1280, REF) (Figure 1). The vast majority of Magdalena sherds, however, exhibit design styles that could be classified as McElmo Black-on-white, suggesting an earlier type at Mesa

Verde which encompassed a wide range of designs and layouts (REFS). Indeed, McElmo is something of a "catch-all" type, stylistically (REFS), and less chronologically specific than might be hoped. Conversely, Magdalena Black-on-white could represent post-Mesa Verde styles: Mesa Verde Black-on-white many miles and several decades from its original production area. Mesa Verde design traditions may have continued to change and evolve, especially in contact with other traditions. For example, many Magdalena Black-on-white sherds exhibit a particular motif called "corbel hatching," also seen extensively on Gila Polychrome, a post-1300 type (REFS). NOTE 1

The ceramic evidence is striking, but limited. Absent more elements of "diagnostic" Mesa Verde material culture, the strongest argument for migration comes from the site's context, in an area firmly within the Tularosa phase region. The Tularosa phase was a major Pueblo III horizon across the southernmost Colorado plateau (Lekson REFS, REFS), with remarkably large towns near Zuni and Acoma (Kintigh REFS, REFS). The two northern Magdalena phase sites, Gallinas Spring and Pinnacle, appear in areas which have many Tularosa phase sites, including some very large ruins. Numerous small Tularosa sites are scattered throughout the mountains and plains north surrounding Gallinas Spring, and earlier interpretations been suggested Gallinas Spring represented an aggregation and transformation of earlier Tularosa phase settlements (REFS). That is, Gallinas Spring was not a migrant community but rather a Pueblo IV amalgam of existing, local Pueblo III populations. It should be noted that this argument was offered at the height of New Archaeology, which was notoriously skeptical of migration (REFS); later thinking on Gallinas Spring is more favorable to migration (REFS). In any event, we do not control the surface archaeology of the Gallinas Spring area, so it is not possible to evaluate histories and demographies from Tularosa to Magdalena phases.

The situation at Pinnacle is potentially clearer: Pinnacle Ruin shares its valley with one very large Tularosa phase site, the 500-room Victorio community. What was the relationship of the Victorio site (Tularosa phase) and Pinnacle Ruin (Magdalena phase)? Did Victorio evolve into Pinnacle, changing architectural and ceramic styles? NOTE 2 Did Pinnacle intrude into Victorio's valley? Was Victorio abandoned, and Pinnacle a later occupation of an empty niche? These scenarios (and others) suggest fundamental chronological issues: How did the two sites relate chronologically? Victorio and Pinnacle may have been wholly or partly contemporary. If not contemporary, they may have been immediately or discontinuously sequential. Contemporaneity could suggest two different groups, with different architectural and ceramic traditions. Immediate sequentiality could suggest in-situ transformation or evolution from Tularosa practices to Magdalena practices. Discontinuous sequentiality could indicate cultural or ethnic difference, although transformation could have occurred ex-situ. Thus, the dating of the two sites is critical to understanding their cultural and ethnic contexts.

## PINNACLE RUIN AND THE VICTORIO SITE

The two sites are about 1.3 km apart in the Rio Alamosa valley, and are easily and directly intervisible (Figure 2). Victorio sits on a broad open terrace 25 m above the Rio

Alamosa floodplain; Pinnacle is perched atop a presumably defensive, shear-sided butte, about 30 m above the Rio Alamosa.

Pinnacle Ruin (LA 2292) is a large (ca. 200 room) Magdalena phase (DATES) pueblo covering most of the top and much of the south slope of a small butte (Lekson and others 2002) (Figure 2). The butte was evidently occupied in the Early Pithouse or very early Late Pithouse periods, although (as noted below) actual contexts dating to those periods have not yet been found. The massed rooms of the pueblo sit directly above jagged bedrock, leveled with a veneer of clayey fill presumably brought from the creek below. The stone for the pueblo itself was the same bedrock, easily available atop the butte and a scree slope on the southeast face of the butte (most of the circumference of the butte is sheer cliff).

Ceramic assemblages of Magdalena phase contexts are dominated by Magdalena Black-on-white (a possible post-Mesa Verde type), Zuni and Rio Grande glaze types, and related utility types (Laumbach REFS). The earliest decorated types on the site include very small quantities of Mimbres Black-on-white and Tularosa Black-on-white. The latest types found at the site include Ramos Polychrome, Gila Polychrome, and later Zuni glazes (Laumbach REFS.) The assemblages also include high proportions of El Paso brownwares and a distinctive utility ware, Seco Corrugated (Laumbach REFS).

The masonry of Pinnacle Ruin was conspicuously unlike "local" masonry traditions, as known from Mimbres phase and Tularosa phase sites in the vicinity (Laumbach REFS). Many walls at Pinnacle Ruin were carefully coursed, in contrast to the un- or poorly-coursed walls at near-by sites; however, it should be noted that Pinnacle Ruin utilized the immediately local stone, a RHYOLITE that fragments in thin, platy sheets that lend themselves to careful coursing, while other sites generally used river cobbles. It should be noted however that several walls at Pinnacle Ruin demonstrated that construction with RHYOLITE could produce un- or bare-coursed walls, and that this material was available to Mimbres and Tularosa phase builders. It seems likely that the careful coursing of Pinnacle Ruin walls, reminiscent of more northern building traditions (such as Chaco and Mesa Verde) was a deliberate choice, and deliberately unlike the local building traditions. However, the site includes no key-hole kivas and no evident "unit pueblos" – both hall marks of Pueblo III Mesa Verde architecture. And other than pottery, no elements of the Mesa Verde Archaeological Complex have been recovered in our very limited excavations. If Pinnacle Ruin was a Pueblo IV Mesa Verde migrant site, many aspects of Pueblo III Mesa Verde material culture were no longer used (a topic to which we will return, below). If Pinnacle Ruin was intrusive, it falls far short of the ideal "site unit intrusion" of Kayenta sites in southeastern Arizona.

The Victorio site (LA 88889) is a very large (ca. 500 room) Tularosa Phase community consisting of over XX separate masonry room blocks scattered over about XX hectares of a terrace over the Rio Alamosa (Laumbach REFS), built in part over a substantial Late Pithouse settlement (Seamont REFS). Tularosa phase ceramic assemblages are dominated by Tularosa Black-on-white, White Mountain redwares, and related utility types (Laumbach REFS). The earliest decorated types on the site are Late Pithouse

period types, including Mogollon Red-on-brown, San Marcial Black-on-white and Kiatuthlanna Black-on-white (Seamont REFS); additionally, small quantities of Mimbres and Socorro black-on-white probably indicate small components from those earlier phases (Laumbach REFS). The latest ceramic types found on the site are represented by very small quantities of Magdalena Black-on-white and Techado Polychrome (Laumbach REFS). A full description of Victorio is beyond the scope of this essay, as are its origins of Victorio—a major research question but again far beyond my goals here (see Laumbach REFS).

It should be noted that there was minimal ceramic overlap between the two sites, especially in key decorated types. There are only a few sherds of Tularosa Black-on-white at Pinnacle, typically in basal, isolated pockets above bedrock; and there is only one sherd of Magdalena at Victorio, surface context(?). Moreover, utility types were largely exclusive, with Reserve and Tularosa corrugated types at Victorio and a distinctive local type, Seco Corrugated and El Paso brownwares at Pinnacle. NOTE 3

**DATING**

Initially, the presence of many post-1300 types at Pinnacle Ruin and their near absence at Victorio suggested that the two sites were not contemporary but in fact sequential. That is, Victorio was Pueblo III and Pinnacle was Pueblo IV. Taken as assemblages, ceramics suggested that Pinnacle followed (developed from?) Victorio or re-occupied an empty canyon after Victorio's abandonment.

The discovery in 2004 of stratigraphically early structures and deposits at Pinnacle, with Magdalena assemblages largely lacking the later Pueblo IV glazes, suggested that the two sites may have been, at least in part, contemporary—if ceramically distinct. Both sites produced abundant wood (especially Pinnacle) but none of the samples submitted to the Laboratory of Tree-ring research could be dated. We returned in 2008 to obtain 14C samples from early contexts at Pinnacle which could be compared to 14C determinations from later contexts at Victorio. All dates were on maize (an annual); all determinations were made by Beta Analytic; almost all were processed via AMS (a few of our initial dates were radiometric).

Combining samples obtained in 2008 with samples from earlier excavations, the pueblo components at each site each have about a dozen 14C determinations on maize, mostly charred cobs but including a few smaller fragments (cupules, etc.) Thus each determination could and should reflect the death of a maize cob. All cobs were burned, presumably for fuel but perhaps in accidental fires.

Both sites had earlier Pithouse period components, sparse at Pinnacle but substantial at Victorio; 3 of 13 dates from Victorio and 1 of 14 dates from Pinnacle were from Pithouse contexts irrelevant to the current question, and are excluded from the analysis (Figure 3). Ten or thirteen dates constitute relatively small samples for two sites this large, but extensive rather than intensive excavations make it likely that the samples represent a

cross-section of the sites' occupation histories, with the following caveats. Dates at Pinnacle were obtained from contexts at or below floor levels (to approximate construction dates) of each of four excavated rooms, selected to represent the site's several areas; and from early and middle levels of a deeply stratified midden. Particularly in 2008, we targeted early contexts below room floors. So Pinnacle Ruin dates are probably biased toward earlier contexts. Contexts from Victorio were more varied, including the full range of temporal contexts as understood from ceramic assemblages. Victorio samples included floor features which might reflect last use; and (again) in 2008 dates we emphasized late or latest contexts. Thus the Pinnacle Ruin sample was to some degree biased toward early contexts while Victorio was biased toward later contexts.

Detailed chronometric, chronologic, and ceramic analyses of specific stratigraphies, features, and contexts at Victorio and Pinnacle are ongoing (Laumbach REFS; Lekson REFS; Laumbach and Lekson REFS). Here I examine the dates as two sets or arrays, and then as a single ensemble. Each date represents an event: the death of an annual plant (maize). That in itself is a valuable record; I will argue below, post-facto, that the number and rate of corn deaths should reflect population. The dates do not necessarily represent the dates of the features and contexts in which they were found: we can be confident that several cobs dating to the Early and Late Pithouse period and the Mimbres or Socorro phases which were found on or in association with room floors of Tularosa phase or Magdalena phase were NOT in their original contexts—the "old cob" problem? Through formation processes which we do not now understand (and may never fully understand), early maize cobs wound up in much later contexts. (As noted above, those four Pithouse period dates, shown above the line on Figure 3, are excluded from further consideration here.) Based on AMS determinations, it also seems possible that one or two later cobs were redeposited in earlier contexts via by rodent disturbance (discussed below). In the main, however, 14C determinations conform to context-specific ceramic chronological and stratigraphic expectations. Again, interpretations of individual dates and specific contexts is not the focus of the present analysis.

While it is tempting to immediately calibrate 14C determinations and thus approach chronological questions with calendrical dates, we determined to look first at uncalibrated values. Uncalibrated values give us more precise data while calibrated dates give us more accurate, but less precise data. If contemporaniety or sequentiality are addressed as a binary, yes-no questions, clearly the more precise data are of greater value than more accurate, but less precise data. For example, if both sites produced 12 determinations each of 750 BP, then the two sites were very likely contemporary, whatever the calibrated date ranges might be. Conversely, if one site produced 12 determinations of 820 BP while the other produced 12 determinations of 700 BP, then they were likely sequential.

The 14C determinations constitute two distributions, one from each site. I look first at the statistical overlap of those distributions. Pooled means of 14C determinations of each site were statistically significantly different (P = less than 0.0001; T 37.33, df 22) with Pinnacle mean 722.10, sd 11.47, N 13; and Victorio mean 877.94, sd 8.4, N 10. In conventional terms, the difference of the pooled means of the two distributions was

extremely statistically significant; that is, the two sets of determinations were markedly different. Given the bias of Pinnacle dates toward early context and of Victorio dates to later contexts, this difference is doubly meaningful. The statistical difference of pooled means from Pinnacle and Victorio suggests, strongly, that the two sites were not contemporary; a conclusion which could be supported by their largely exclusive ceramic assemblages. But closer analysis of determinations and their calibrated values suggests significant overlap between the two sites (Figure 3).

While comparisons of "averages" of all determinations in the two samples suggests the two sites were not contemporary, comparisons of individual 14C determinations suggests significant contemporaneity (Figure 4). I use the uncalibrated 14C determinations for this analysis, initially, because those measurements will be the most accurate (if not precise) metrics we can employ. Following initial discussion of the uncalibrated determinations, I will then use standard calibration programs for archaeological interpretations.

It is evident on Figure 4 that the AMS determinations cluster at 810-890 BP, 710-730 BP, and 620-640 BP, with a single additional determination at 570 BP. Each cluster is separated from the following cluster by "gaps" of 50 to 70 radiocarbon years. These gaps may represent sampling issues (but probably do not, as we shall see) but do not reflect variation in the production of 14C or other technical or instrumental issues at Beta Analytic (Ron Hatfield, p.c. Sept 17, 2010). That is, there is no reason that samples from either Pinnacle or Victorio would NOT have produced 14C determinations in the ranges of the "gaps." If the distribution of determinations had been random between 890 BP (the earliest) and 570 BP (the latest), then we would in fact expect a more or less even distribution of determinations within decadal intervals; a simple (and unnecessary) simulation of random dating would ultimately produce a very even distribution of dates from 890 to 570 BP. That is not what we see in Figure 4; the distribution is clustered and non-random.

The latest date of 570 +/- BP is a single date, and not really a cluster. To anticipate later discussion of calibrated dates, the calibrated value for 570 +/- 40 is statistically no different than the calibrated date range of pooled 620-640 BP cluster determinations (see Figure CAL). That is, comparing the calibrated value for the 570 BP "outlier" with the pooled calibration of the 620-640 determinations shows no significant statistical difference. While "gaps" between earlier clusters prove significant after calibration, the later 570 BP determination is grouped with the 620-640 BP cluster hereafter. Thus, subsequent discussions address three, not four clusters. This introduces a small circularity in the data, but not in the argument (since the terminal date of Pinnacle is not at issue here), and makes the presentation simpler, shorter, and easier to follow.

Importantly, the close parallels between the two site distributions suggest that the clusters are real. There are dates within each cluster at both sites; that is, the 810-890 BP cluster includes 4 dates from Pinnacle and 7 dates from Victorio; the 710-730 BP cluster includes 5 dates from Pinnacle and 2 dates from Victorio; and the 620-640 BP cluster includes 4 (including the 570 determination) dates from Pinnacle and 1 date from

Victorio. Thus the patterns of clusters and gaps are parallel in toto at both sites. Again, consider the hypothetical simulation of date distributions if the determinations were random between 890 and 570 BP: those simulations would not look like Figure 4.

T-tests between sequential clusters (combining determinations from both sites) indicate, predictably, highly significant differences between pooled means of each cluster; in each case P = 0.0001 or less. That is, the differences between pooled mean of each of the three radiocarbon year clusters are statistically extremely significant. The significant difference of the pooled mean of the entire sample from each site of course reflects the varying distribution of determinations within each site sample: more early determinations from Victorio, and more later dates from Pinnacle. Recall that our sampling biased these samples slightly opposite the observed distribution; that is, we favored later contexts at Victorio and earlier contexts at Pinnacle. This suggests that the observed distributions of determinations may actually represent a real, underlying distribution of determinations that may be partially independent of the site's stratigraphy and structure.

Consider the nature of the specimens submitted for dating: all were maize, almost all were burned cobs. A post-facto heuristic model may begin to explain the clusters and gaps in the observed distributions—and suggest ways for thinking about messy sites and messy data.

Consider these determinations as samples of a population of events at both Victorio and Pinnacle: the annual life and death of corn plants grown, consumed and discarded by each community. Ceteris paribus, the number of cobs should correlate with the number of people consuming corn. That is: 0 people grow/consume 0 corn, 100 people grow/consume X corn, 1000 people grow/consume 10X corn, 10000 people grow/consume 100X corn, etc. If we had all the corn cobs from Pinnacle and Victorio, we could presumably judge the relative populations of those two sites. From ethno-historic and nutritional data, it might be should be possible to estimate a constant of how many cobs per year per person and, ceteris paribus, to derive actual population figures. However, that step in not necessary for the purposes of this paper.

Not all other things are equal, of course. We have only two dozen cobs from the hundreds of thousands of cobs that would have been associated with these sites. Beyond sample size, several major factors potentially affect this heuristic model. Among the obvious factors are variations in (1) productivity, (2) consumption, (3) deposition, and (4) recovery.

1. Variation of maize productivity is a staple of Southwestern archaeology, and many of our explanatory models are based on variation as inferred from dendroclimatology and reconstructions of agricultural potential (e.g. REFS). Productivity no doubt varied from year to year, but Canada Alamosa has large quantities of permanent water from springs a few km upstream, and the quantities of water produced by those springs is very steady within years and from year to year (REFS). Spring water in the Rio Alamosa was almost certainly used for diversion irrigation of a well-defined pocket of about XX acres (XXX hectares) of irrigable, arable land. That is, the same relatively high-quality lands and a

constant, more than sufficient supply of water were available to both Victorio and Pinnacle. While we currently do not control data on variation in temperature and growing season, it appears that climatically- or environmentally-caused variation in productivity should be minimal.

2. Consumption certainly varied from year to year and individual to individual, but perhaps within relatively narrow limits, and probably stochastically. We can assume that the residents of both sites were fully agricultural. Large, masonry, permanent villages built adjacent to irrigable lands presumably indicate a basic commitment to maize farming. There is no obvious evidence of status differences or other economic differences (and the dietary expression of such differences, if present, would perhaps be expressed more in terms of meat protein than in corn consumption). Age-, metabolic-, status- or other individual variations presumably would "average out" over the moderately large populations indicated by the size of the two sites. (A reasonable guess at community size at each site would be in the low hundreds.) Populations residing in those villages probably would, over the relatively short period of occupation indicated by these 14C determinations, consume relatively constant quantities or corn per individual.

3. Deposition (including preservation) depends first and foremost on charring of cobs. Both Pinnacle and Victorio are "open" sites and thus lack the high degree of preservation of uncharred vegetal materials seen, for example, in dry caves. While wood (almost entirely juniper) was well preserved at Pinnacle, neither site produced quantities of unburned, perishable artifacts or vegetal debris. Indeed, we can be sure that most corn was not burned (thus hopes for using corn cobs as a direct index of population are not really practicable). Cultural practices may have varied in the proportion of cobs used in fires, used for other purposes, or simply discarded without further use. Some cobs were burned in hearths; other cobs may have been burned accidentally. It should be noted that fire wood would not be notably limited or scarce at either site, due to the near proximity of forested mountains and foothills to the north and west. Thus, burning of cobs might be more or less random, and thus a reasonable sample of the larger cob population. If we assume that the proportion of cobs burned (however that occurred) and discarded was roughly constant and comparable at both sites—a major assumption—then quantities of burned cobs should reflect the scale or size of the larger cob populations of which they were a part. That is, more corn would production would result in proportionately higher quantities of burned cobs, whatever their histories.

4. Archaeological sampling and variability in recovery has been discussed above. The samples are small and non-random, but still we believe reasonably representative of the sequences of occupation at both sites. Three factors suggest that the sample of burned cobs does represent those occupation histories: first, the recovery of early determinations from later contexts, discussed above, suggests a randomizing of the sample through site formation processes we cannot currently control; second, the contradiction of the observed distributions at each site (Figure 4) to the targeted context sampling; and third, the strong clustering of determinations between the two sites (Figure 4). In the latter case, we can be reasonably confident that the samples from the two sites represent independent samples—that is, people from Pinnacle were not burning cobs from Victorio

and vice versa—because the two sites are 1.3 km distant and on very distinct landforms. While the sites are intervisible, they are not open access.

In summary, it seems likely that productivity at Canada Alamosa was relatively constant; average consumption per person at these two large pueblos should have been relatively constant, deposition can be considered essentially random, and recovery was (despite intentional bias) apparently representative. The latter claim requires further discussion: small samples are more likely to include numerous or ubiquitous items; small samples are less likely to include less numerous or rare or unique items. Thus small samples of cobs are more likely to include cobs from periods of high corn productivity, consumption and deposition (i.e. more corn), and less likely to include cobs from periods of low productivity etc. (i.e. less corn).

The heuristic model requires several assumptions and conditions. We believe that these assumptions and conditions are not unrealistic, and thus it seems likely that more cobs within a time span indicate relatively more people within that time span (and vice versa), and thus that clusters in determinations probably indicate higher population levels while gaps in 14C determinations probably indicate lower or even absent population.

Uncalibrated 14C determinations from Pinnacle Ruin and the Victorio Site show three distinct clusters separated by 70-year gaps. Samples from both sites fall in all three clusters. There is no reason to suspect that these clusters or the gaps that separate them are the result of the 14C process. Presumably, determinations within the gaps were equally probable, a priori, to the determinations which were actually made in the three clusters. That is, there is nothing in Beta Analytic's process or in the known variation in 14C environments (REFS) that would "create" gaps or clusters in this radiocarbon time-span. In short, the clusters and gaps are real – in radiocarbon years BP.

THE FACT THAT TWO SITES PRECISELY REPEAT THESE PATTERNS (I.E. GAPS) STRONGLY SUGGESTS THAT THE PATTERNS ARE REAL. THIS IS NOT A STATISTICAL FLUKE – according to Beta Analytic. RUN THIS WITH OXCAL BAYESIAN PROGRAM.

Radiocarbon years BP are not, of course, calendrical years. The determinations were calibrated with Calib 6.0.1 (Figure 5). These individual determinations will be used in ongoing context-by-context analysis (REFS); the goal of this paper is to examine the chronometric data at coarser scales. Recognizing that the clustering of uncalibrated determinations are statistically significant, the means and s.d.s were pooled each cluster and calibrated using Calib 6.0.1 (Figure 5). As noted above, the three calibrated distributions overlap at two-sigmas, but remain statistically significantly different. Because calendrical calibrations almost invariably extend ranges over distributions broader than un-calibrated determinations, the "gaps" between the three clusters are less clear, but still evident. In each of the three probability distributions shown in Figure 5, the likelihood of the actual pooled date is of course highest at the mode of modes of the distribution. Thus, the likelihood is high that the actual pooled date of the middle cluster falls at about 1275, the peak of this very sharply uni-modal distribution. Conversely, the

pooled date(s) of the late cluster will most likely be at about `1310, 1370 or 1390 – the three modes. That is, there is a higher probability of the actual value falling at or near the mode than elsewhere in the distribution. The form of the distribution – modality, skew, etc. – is a combination of the actual date(s) and the variation in the calibration curve. The distribution is not entirely the product of calibration; the actual date(s) clearly play a role. Since the actual date(s) are directly contributory, the shape of the distribution itself can, in some cases, be useful in chronological interpretation (REFS)

Calibration, in addition to correction of the 14C date to a calendrical date, invariably "broadens" date ranges because the correlation between 14C and calendrical dates is not perfectly linear. Indeed, the calibration curve is famously (and very significantly) irregular: a roller coaster of "shelves" and "wiggles." These important irregularities create multiple intercepts for a single 14C determination and therefore a series of calendrical dates with correspondingly large statistical ranges. The individual calibrations (CALIB 6.01) for Pinnacle and Victorio dates is shown in Figure 3.

If we accept the logic of the heuristic model above, the 14C clusters can be interpreted as clusters of dated events during periods of relatively higher population. To follow this line of inquiry, means and sigmas of each cluster of determinations were pooled and the resulting "averages" calibrated (CALIB 6.01).

For the three clusters, this procedure produced two-sigma ranges of 1160-1215, 1230-1280, and 1290-1400, rounded to even semi-decades for convenience (Figure CAL MEANS). For convenience, these spans can be termed early (1160-1215), middle (1230-1280) and late (1290-1400). The three calibrated pooled clusters were tested for statistical difference, and again each calibrated cluster distribution differed with high statistical significance (P = ????) from the other clusters. That is, despite overlap at two sigmas, the three distributions are statistically significantly different (T test, P = 0.0001 or less).

The two sites both were large communities, with considerable construction, modification, and (in Pinnacle's case) extensive vandalism. Complicated site formation processes may have (and demonstrably did) redeposit charred cobs dating earlier events in later contexts and, to a lesser degree, vice versa. Again, detailed analysis of specific stratigraphies and contexts is ongoing, but it is likely we will never fully understand those complex formation processes. However, we can say with some confidence that events left chronometric evidence at both Victorio and Pinnacle in all three time spans (Figure 4); and we can say with slightly less confidence that the intensity of events shifted from Victorio in 1160-1215 to Pinnacle in 1230-1280 and 1300-1400. Thus, it appears that the two sites were contemporary with the chronometric "center of gravity" shifting from Victorio to Pinnacle sometime after 1215, with a very notable peak at 1275 (Figure 4; in part a function of the calibration curve—but real).

The markedly varied forms of the probability distributions of the clusters (Figure 5) suggest an accumulation of individual events at both sites (but primarily at Victorio) during 1160-1215; a tight concentration of events in the 1230-1280 span (primarily at

Pinnacle); and a continuous but sporadic accumulation of events from 1300-1400 (almost exclusively at Pinnacle). For reasons discussed above, these chronological clusters probably represent real chronological "events," that is peaks of population. Pushing these data further (and perhaps into less firm ground) extrapolating from the number samples, it appears that 1160-1215 was represented by more events at Victorio than at Pinnacle; 1230-1280 comprised more events from Pinncale than Victorio; and 1300-1400 was heavily weighted to Pinnacle. Pushing even farther, the shapes of three curves might be interpreted as a steady accumulation of events mostly at Victorio from 1160 to 1215; a sharp concentration of events at both Victorio and Pinnacle at 1230-1280; and possibly lower intensity or even discontinuous events mostly at Pinnacle from 1300-1400. (The "wavy" nature of the 1300-1400 calibrated distribution in part reflects a smaller number of AMS determinations (5) but more importantly the nature of the calibration curve for that span: the five AMS determinations might fall in any or all of the three calibrated "peaks.")

As noted above, detailed contextual analysis of ceramic and chronometric data is ongoing, but the hypothetical scenario just presented conforms reasonably well to our current understanding of the two sites and their occupation histories (Laumbach REFS; Lekson REFS). Thus, although the pooled means of each site's total assemblage of AMS determinations were statistically significantly different and suggested the two sites were not contemporary, analysis of the three clusters of determinations suggests significant overlap between the two sites.

## PINNACLE RUIN AND MESA VERDE

Ceramics strongly indicate connections between Pinnacle assemblages (and, presumably, populations) and the Mesa Verde region (Laumbach REFS). Magdalena Black-on-white is not, precisely, Mesa Verde Black-on-white; rather it looks like McElmo and Mesa Verde black-on-whites, recreated XXX kms and perhaps XX years from the "homelands" of those Four Corners types (Figure 1). The argument for migration is based mainly on sherds (there are differences in masonry and massing between Victorio and Pinnacle, but those differences – while real – are not conclusive.)

At Pinnacle we do not have the clarity and coherence of a San Pedro "site unit intrusion" – but we have more than the mere demographics of the northern Rio Grande.

If 1160-1215, 1230-1280 and 1300-1400 represent events or clusters of events, how do these events correlate with larger regional dynamics? What was happening in the Four Corners at those times?

With tree-ring data and a clean, clear site, it is possible to do wonderful things. The acme remains Jeff Dean's analysis of two large Kayenta Cliff dwellings (Dean REFS). But most sites are not so fortunate. For example, see "Chimney Rock and Chaco."

Detailed analysis of the many thousands of tree-ring dates from the Mesa Verde region is far beyond the scope of this essay. Fortunately, those data come "pre-crunched" by Berry and Benson's (2010) analysis of almost 7,000 cutting or near-cutting dates from over four hundred sites across the Four Corners region (including the Mesa Verde region). Berry and Benson (2010) use tree-ring dates as a proxy for relative population (a method discussed further below). Of more immediate use here, they correlate tree-cutting and climate (specifically, variations in precipitation) and conclude: "…very pronounced timber-cutting maxima between AD 1030 and 1130 and between AD 1190 and 1290 are associated with two of the wettest periods…In contrast, the timber-cutting minimum between AD 1130 and 1200 and the rapid decline in tree-ring dates after AD 1290 were each prefaced by several decadal-scale mega-droughts…which occurred between AD 1130 and 1177 and between AD 1273 and 1297" (Berry and Benson 2010:62-63). They interpret timber cutting minima as indicative of reduced tree-cutting, and therefore greatly reduced population. That is, based on a very large chronometric sample, the posit general reduced or de-population of the Four Corners around 1130-1177 (Berry and Benson 2010:Fig. 3.4; see Figure 6).

Several recent demographic syntheses which utilize other lines of evidence are available (Glowacki REFS, Ortman REFS, Ortman, Varien and Gripp 2007; Ryan 2008; papers in Kohler et alia 2010). Here I rely here on Varien's (2010) summary of demographic models for the central Mesa Verde area, supplemented by Glowaki's (REFS, 2010) observations on population elsewhere in the Four Corners. Varien (2010:15ff) uses models from the Village Ecodynamics Project (VEP), that encompasses most of the Great Sage Plain, but not Mesa Verde itself or the eastern and western districts of the Mesa Verde region. For this area (which includes most of the largest Mesa Verde sites), Varien states, "the VEP population estimates show that population peaked at about AD 1225 and then declined rapidly" to about half the 1225 peak by 1260, with final abandonment by 1285 (Varien 2010:15). Varien (2010:16; citing Ryan 2008), argues that "the study region was not depopulated during the 1140-1180 period, despite the decline of Chaco Canyon as a center and despite the severe drought." Indeed, the VEP model suggests population growth from 1140 to 1180 of about 10% over four decades. Continuity and growth in population through the mid-twelfth century drought is inferred from lines of evidence other than absolute dates: 12th century tree-ring dates are relatively rare. Explanations for the low numbers of cutting dates during the mid-12th century drought generally appeal to retrenchment dynamics, or "hunkering down" (Lipe 2006:REFS).

Berry and Benson (2010) suggest significant depopulation over the Four Corners region corresponding to the mid-12th century drought, while Varien (REFS) and Ryan (REFS) argue for population growth during this period, in the central Mesa Verde district. These two perspectives are, perhaps, not as contradictory as they may at first appear. The VEP model applies only to the central Mesa Verde district. Berry and Benson's analysis covers a much broader area of the Four Corners. Donna Glowacki (2006) notes that population trends in other areas of the Mesa Verde region did not always parallel those of the central Mesa Verde district. Indeed, while central Mesa Verde population increased, that of the western and eastern districts decreased, to some extent contributing to population growth in the central Mesa Verde. Elsewhere in the Four Corners, there

seems to be firm evidence for population decline and depopulation, much as indicated by Berry and Benson's analysis. Thus population growth (as suggested by Varien and Ryan) in the VEP/central Mesa Verde district does not negate the larger pattern of population decline in many other districts of the Mesa Verde region and the larger Four Corners.

Seemingly anomalous population increase in the central Mesa Verde during the mid-12th century drought parallels a similar, contemporary phenomenon in Hohokam (Abbot REFS), when population increase in Phoenix Basin was caused in large part by in-migration from previous peripheries, as Hohokam "collapsed" into its central core area (Abbot REFS; Lekson 2009. While the Four Corners almost certainly saw extensive out-migration at the time of the mid-12th century drought (Berry and Benson RES), increasing population in the central Mesa Verde presumably reflects political or cultural factors which held settlement systems together in the face of severe environmental challenges (Varien REFS; Ryan REFS; Glowacki REFS). Aztec Ruins appears to have been a major "re-creation" of Chaco Canyon's scale and formality, with monumental construction from about 1110 to 1280 (following Brown, Windes and McKenna REFS and contra Lipe 2006 "Notes from the North"). If Aztec Ruins represents a continued, if diminished, Chaco-style socio-political center (Lekson 1999, 2009), it may have been a central factor in the coherence and continuance of central Mesa Verde populations in the face of the mid-12th century drought. Aztec Ruins' presence in the central Mesa Verde district may be demonstrated by bi- and tri-wall structures (REFS). These structures, which form the key nodes of Aztec Ruins' "cityscape" were particular that 12th and 13th century center (Stein and McKenna REFS). The earliest dated triwall was built at Chaco Canyon – and then immediately razed (Lekson REFS; REFS). Thereafter, three large tri-walls were constructed within Aztec Ruins (including the huge, possibly "quadri-wall" Mound F, central to the entire site) and then at numerous communities in the central Mesa Verde district (see Glowacki 2006:Fig. XXX). It seems likely, on this evidence, that bi- and tri-walls were Aztec Ruins' unique "signature" and a marker of its region of influence—which thus included the central Mesa Verde district, the western San Juan Basin, and not much else (Lekson REFS). If not Aztec Ruins, however, then other social or political forces held the central Mesa Verde district together during the 12th century drought, while much or most of the rest of the Mesa Verde region and the Four Corners experienced severe de-population.

In summary, population trends for the Mesa Verde region and the Four Corners parallel or reflect major climatic events, with several important exceptions. Significant population loss occurred coincident with the mid-12th century drought, except in the central Mesa Verde district and perhaps the Aztec Ruins area ("Totah"). A process or series of events leading to complete or nearly complete depopulation began with out-migration beginning before the mid-13th century (i.e., a time of relatively favorable climate trends), ending with the Great Drought of the late 13th century. How do these trends correspond with early, middle and late date clusters at Pinnacle Ruin and the Victorio site? How does the Canada Alamosa (Pinnacle and Victorio) chronology compare with events in the Four Corners (Figure 6)?

1160 to 1215: The nearly rectangular form of the probability distribution (Figure 5) indicates that the actual date or dates of these events have nearly equal chances of falling anywhere between about 1160 and 1215. Much of the Southwest, including the Mesa Verde region and the larger Four Corners, experienced a severe mid-12th century drought between about 1130 to 1180. The early cluster at Victorio and Pinnacle corresponds to the later decades of the mid-12th century drought and the decades immediately following that drought. As noted above, there are conflicting interpretations for the demography of the central Mesa Verde district, but appears that much of the southern Colorado Plateau was substantially depopulated during this drought. Significant numbers of people left the Four Corners looking for more favorable agricultural environments, even while many people remained in the Mesa Verde area. The early (1160 to 1215) cluster at Pinnacle Ruin may reflect movements in response to the mid-12th century drought. The earliest ceramic assemblages at Pinnacle Ruin are dominated by Magdalena Black-on-white (without later glaze types). Thus one possible interpretation of early cluster dates at Pinnacle Ruin might see this as the foundation of the site by migrants from the Mesa Verde (ceramic) region fleeing the mid-twelfth century drought, leaving Mesa Verde perhaps in several waves at the height of the drought and immediately thereafter. NOTE 4

1230-1280: The very sharp peak at 1275 (Figure 5) indicates a high probability of an event at about 1275. We suggest that this peak represents a late, large-scale movement out of the Mesa Verde area. To arrive at Pinnacle around 1275, populations would have to have left their original settlements before that date; that is, well before the onset of the Great Drought of 1275-1300. The apparent consensus of recent Mesa Verde chronologies and demographies is that out-migration occurred throughout the 13th century; contrary to the conventional (and popular) understanding that a Great Drought about 1275-1300 forced out-migration. It appears that substantial numbers of people were leaving the area long prior to 1275, and Mark Varien is of the opinion that the entire area was completely depopulated by 1280 (only five years into the drought) (Varien 2010). Indeed, the Great Drought was merely the final punctuation of a long process. Varien notes "pre-1260 decline [in population] is almost as great as the depopulation that characterizes the post 1260 period" (Varian 2010).

1300-1400: The late cluster of 1300-1400 reflect the shape of the calibration curve for that period, with multiple modes (Figure 5). These multiple modes may (or may not) indicate a series of events, reflecting continued occupation through the 14th century, as suggested by late pottery types. NOTE 5

**READING A MESSY RECORD**

Conventional strategies for building site and regional chronology ideallly work "up" from arrays of contextually clear provenience and stratigraphies. A clean stratigraphic column is worth its (archaeologically) weight in gold. Extensive excavations at relatively solid sites – for example, a large great house like Salmon Ruin, protected by its vast bulk (REFS) – can produce seriations and Harris matrices and other chronological heuristics. Those text-book tools are usually honored only in the breach in southern New Mexico,

where large sites – often complicated by invasive multi-componcies – are almost universally severely vandalized (Lekson REFS). This is especially true for the Mimbres sites on the Rio Grande, Mimbres and Gila rivers. The sites discussed here, Pinnacle and Victorio, post-date Mimbres on the northernmost Mimbres frontier (REFS), but neither escaped the regional enthusiasm for illicit digging. Because of its spectacular setting atop a sheer-sided butte, Pinnacle Ruin was well known to local pot hunters as "Machu Pichu." Victorio, while slightly less battered than Pinnacle, suffered its share of vandalism.

"Top down" approaches to chronology are rare and, in the Southwest, they are greeted with intense skepticism. For example, Barry and Benson's regional chronological studies, based on the chronological distribution of thousand of tree-ring dates from the Four Corners region (Barry 1982, Barry and Benson 2010) have met with harsh criticism (e.g. Dean REFS; papers in Kohler et alia 2010). While skepticism is always appropriate, I believe the rejection of these "top down" chronometric approaches is perhaps short-sighted, based on a latent "Pompeii premise" in the archaeology of the northern Southwest. The correlation of Pinnacle/Victorio date clusters, regional climatic events, and Barry and Benson's construction chronologies (REFS) are probably not a matter of chance or coincidence. NOTE 6

If, for example, Four Corner's archaeologists argue that significant population remained in the region during the mid-12th century drought, corn cobs could prove or disprove that interpretation. However, there's the matter of money: AMS = $??? Vs. dendro = $???

Large Mesa Verde sites are certainly complicated and require skilled excavation; but the contextual clarity of those sites only seldom can be matched outside that district. It the conventional standards of proof and evidence customary in the Four Corners were strictly enforced in southern New Mexico, we would probably have to abandon archaeology altogether in that half of the state.

Even in the Four Corners, archaeology is messy. I see merit in Barry and Benson's approach, and I have attempted to apply a comparable strategy to an even messier archaeological situation: an array of two dozen 14C dates from very limited testing at two large, vandalized sites, Pinnacle Ruin and the Victorio site. Neither is a Pompeii.

APPLES AND ORANGES: 14C vs. dendro. Small samples vs. big samples.

I believe that the chronological heuristic developed in the course of initial chronometric analysis of these two sites suggests possible tactics that might be of use even in the Four Corners – and area were 14C is reserved only for Archaic Period cave deposits. Four Corners (and especially Mesa Verde area) archaeology relies almost exclusively on dendrochronology, which typically dates construction but less certainly duration of occupation. Mesa Verde middens contain abundant burned corn; I suggest that studies of site occupation (Varien REFS) would do well to invest in AMS dating of a sample (random or stratigraphic) of maize.

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NOTE 1 A full description of Pinnacle Ruin and its artifact assemblages is far beyond the goals of this essay; and indeed must await on-going analyses. Typological and INAA analyses to date suggest that distinctive non-decorated corrugated types also differentiate Pinnacle from earlier, local ceramic traditions (Laumbach REFS), and that both Magdalena and the corrugated types were locally made, as well as including vessels probably made at the larger Gallinas Spring site (Ferguson REFS).

NOTE 2. A terminal Pueblo III population at Victorio (presumably reduced in number from the site's 500-room peak) shifted material culture and site location, building Pueblo IV Pinnacle Ruin. While the differences in material culture (ceramics and architecture) are marked, comparable changes in material culture from Pueblo I to Pueblo II, and from Pueblo II to Pueblo III have not conventionally been viewed as representing population replacement or in-migration.

NOTE 3 It is worth pointing out that Kayenta migrants at Point of Pines (the classic Southwestern "site unit intrusion;" Haury REFS) made their traditional decorated pottery, but used local utility wares (Haury REFS). The situation at Pinnacle is not fully understood, but reliance on El Paso series brownwares (characteristic of contemporary sites on and across the Rio Grande, about XX km from Pinnacle) may parallel the situation at Point of Pines.

NOTE 4 Subsequent return migration to (or continued contact with) Mesa Verde region settlements may well have provided key information for a second, larger migration to Pinnacle Ruin, about 1275. Repeated migrations and return migrations would not be unprecedented; the Mesa Verde region had seen at least two cycles of depopulation and repopulation prior to the final "abandonment" (Wilshusen REFS).

NOTE 5 The 570 BP "outlier" which was statistically folded into the late cluster calibrates to two sigma approximately 1300 to 1370 and 1380 to 1430, with very clear peak probabilities at about 1335 and 1350; this date may well represent an event which represents the latest occupation of Pinnacle Ruin. Additional dating of late contexts may better define the nature and span of post 1275 occupation at Pinnacle Ruin; the aim of the chronometric program was not on the terminal occupation of Pinnacle Ruin, but rather its founding and possible chronological overlap with the Victorio Site.

NOTE 6 Barry and Benson's (2010) is, in my opinion, very useful. An alternate version could improve their approach d by correcting the raw total of latest cutting or near-cutting dates in individual contexts (rooms, sites, etc.) to eliminate the problem of multiple dates from single structures. Susan Ryan (REFS) provides an example of the problem:

> "For example, a single kiva recently excavated in the central Mesa Verde region yielded 73 cutting dates from the A.D. 930–970 period which, when added to the

> regional tree-ring cutting record, essentially erased a gap that had previously existed (Ortman personal communication)."

In this case, the problem is demonstrated by 73 from "a single kiva" are taken to demonstrate that no gap existed 930 to 970. A single context (in this case, a kiva) does not eliminate a gap; there are still fifty years with precisely one dated structure.

Figure 1 Mesa Verde Black on white (left) and Magdalena Black on White (right), from Gallina Spring. N.B. Very few Magdalena sherds look THIS much like Mesa Verde; indeed it is tempting to label them "McElmo" – a catchall type in the Four Corners – but I think instead that Magdalena Black on white represents Mesa Verde ceramic traditions several hundred miles and several decades removed from the Mesa Verde area.

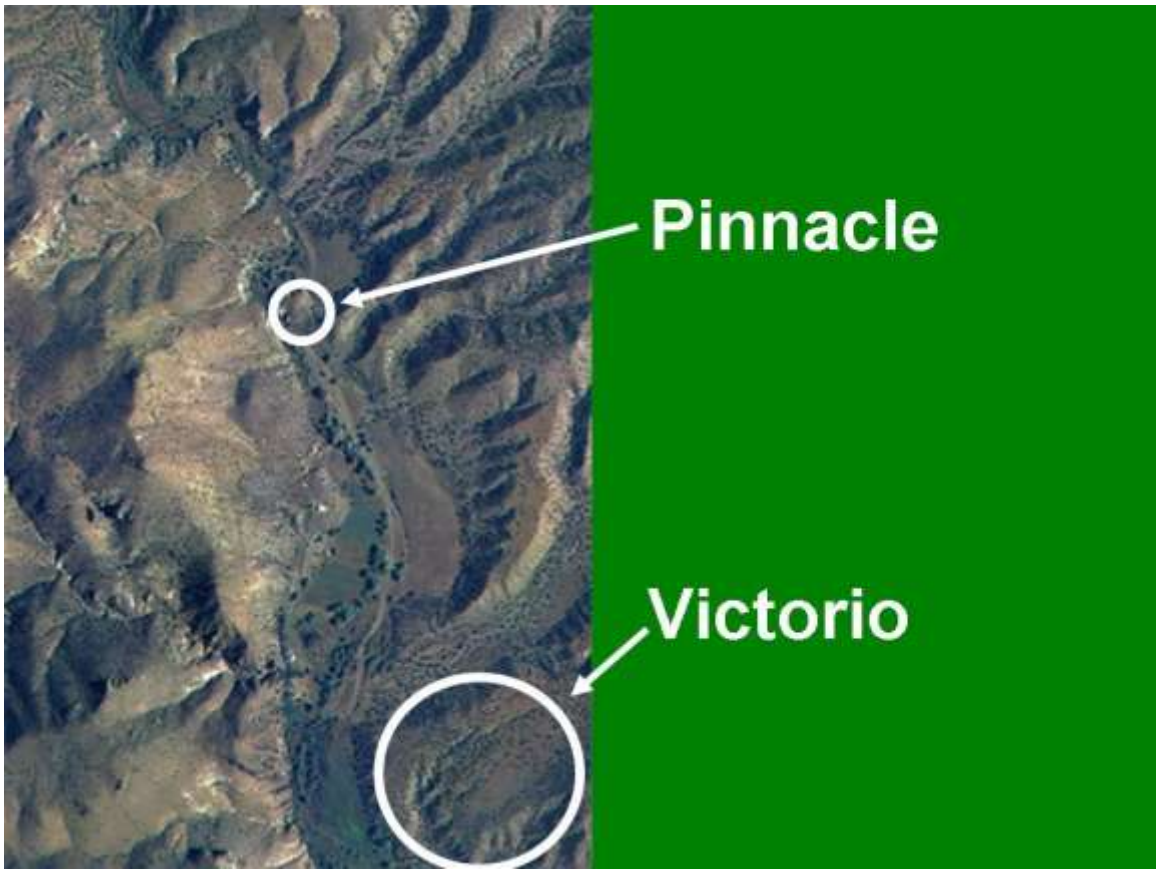


Figure 2. Pinnacle Ruin in relation to the Victorio site. Distance about 1 km. The sites are intervisible.

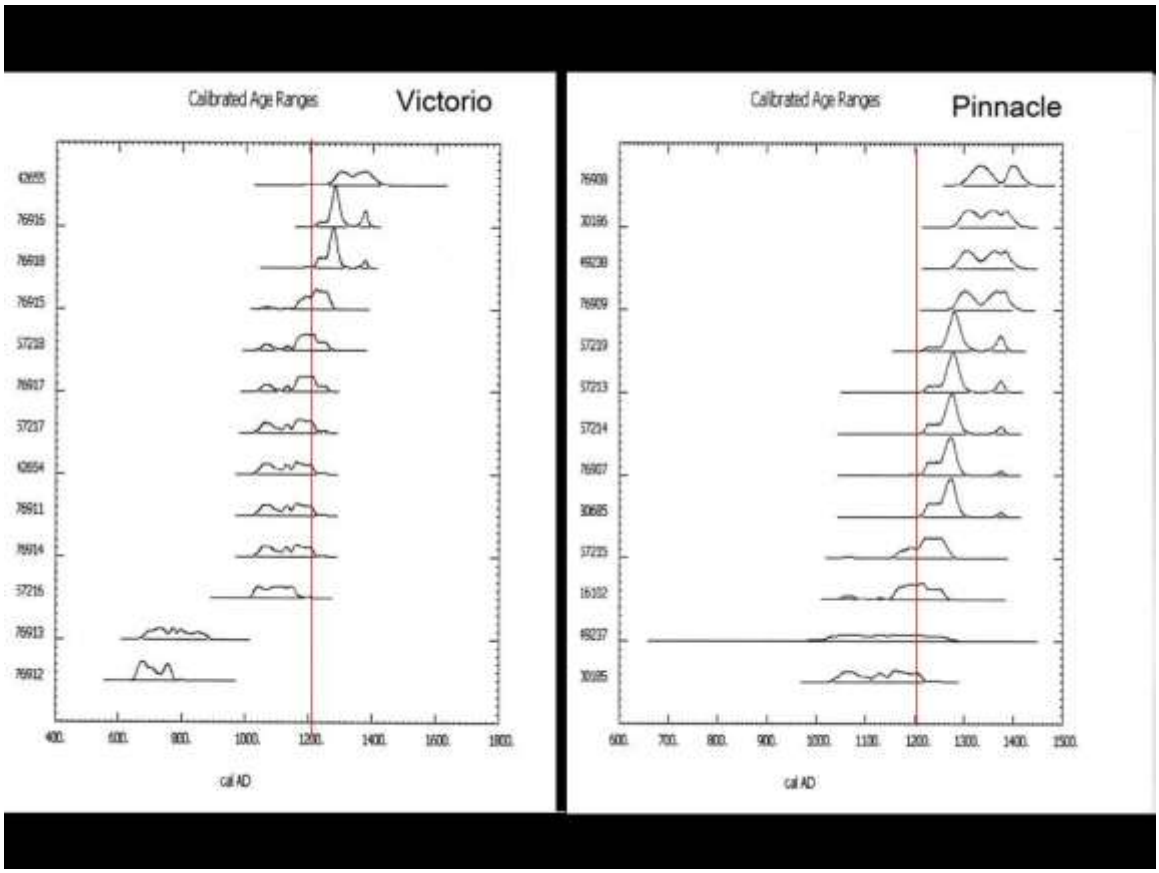


Figure 3. 14C dates on maize, calibrated. Red line = 1200.

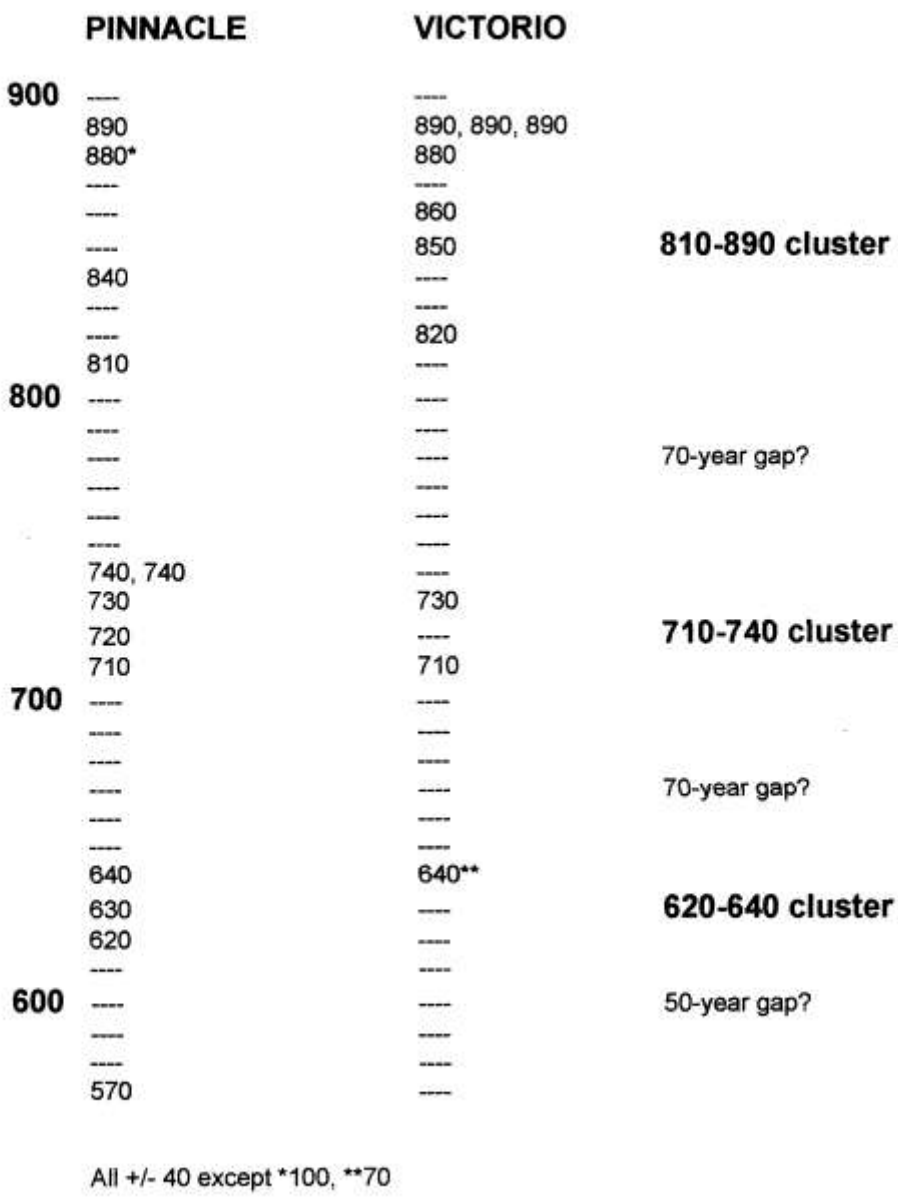


Figure 4. Clusters and gaps in uncalibrated 14C determinations.

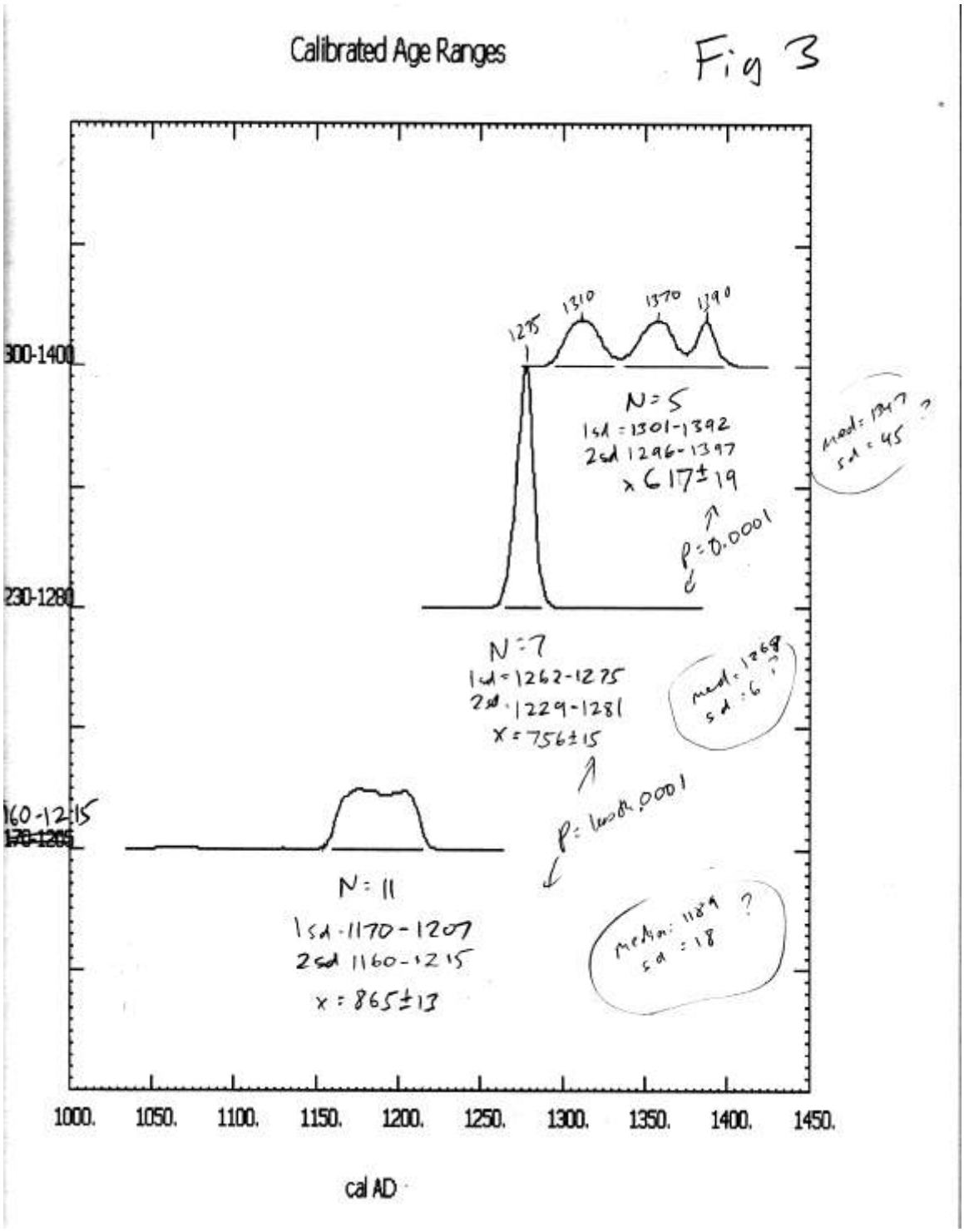


Figure 5. Clusters, calibrated.

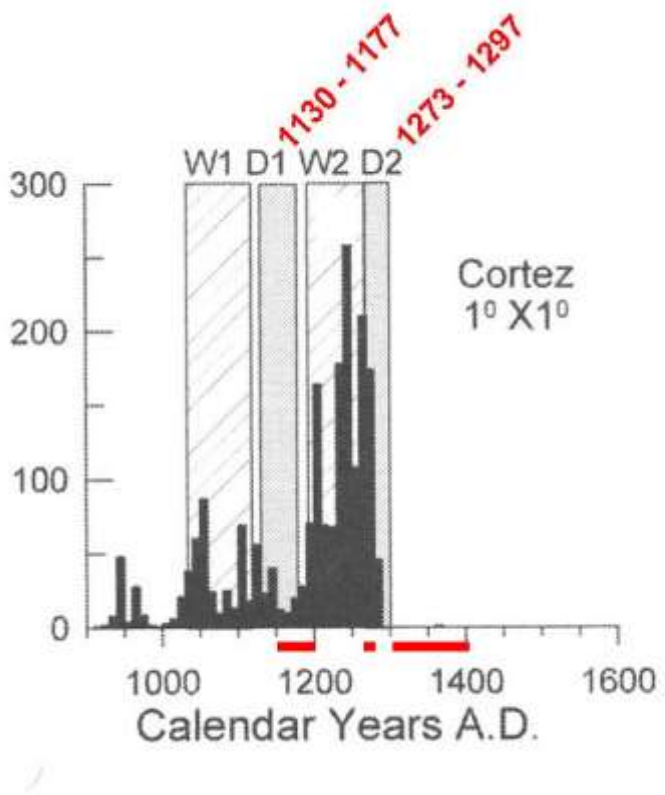


Figure 6. Canada Alamosa clusters, calibrated, compared to cutting dates from the Four Corners (Berry and Benson 2010).